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Improving Job Matches in the U.S. Labor Market

EDUCATIONAL AND POLITICAL LEADERS are calling for improvements in the signaling and certification of worker skills. During the 1992 election campaign, President Bill Clinton called for "a *national examination system* to measure our students' and schools' progress" in meeting the national educational standards.[1] In 1989 the Secretary of Labor's Commission on Workforce Quality urged schools to "develop easily understood transcripts which at the request of students, are readily available to employers. These transcripts should contain documentable measures of achievement in a variety of fields as well as attendance records."[2]

For educational reformers, better signaling of job skills is not an end in itself, but a means of inducing students, parents, teachers, and school boards to place greater priority on learning. It will not be easy, however, to design a system that certifies academic and occupational achievement; attracts employer participation; satisfies federal nondiscrimination requirements; and maximizes student incentives to learn, teacher incentives to set high standards, and parental incentives to demand and pay for a quality education.

Improving education is not the only reason to improve signaling of skills. Better matching of workers to jobs also creates private and social economic benefits. Jovanovic and Moffitt (1990) have estimated, for example, that if the "information about the job match that arises as a byproduct of experience" were obtained instead by "a perfect screen

1. Clinton and Gore (1992, p. 85).
2. Commission on Workforce Quality and Labor Market Efficiency (1989, p. 12).

at the beginning of their working life,'' the earnings of younger workers would be higher by as much as 13 percent.[3]

This paper examines how government can facilitate better job matching. The first section describes the results of a recent survey by the National Federation of Independent Business (NFIB) on the experience of 2,599 firms with new hires. Managers of small and medium firms were very often unpleasantly surprised by the performance of new hires. After six months on the job, more than one-quarter of new hires were producing less than 75 percent of what was anticipated when they were hired.

The second section examines the characteristics of workers and firms that produce successful job matches. It appears that employers rationally use the information they have about job applicants. Mismatches thus arise from imperfect information, not from imperfect use of the available information. What kinds of errors in predicting worker skills and personality are responsible for the large negative differentials between actual and expected productivity? This question was tackled by examining the relationship between the surprise in productivity realizations and *ex post* assessments of six separate skill and personality dimensions. This analysis suggests that reliable *ex ante* assessments of work habits, of occupational skills at the time of hiring, and of the ability to learn new occupational and job skills would generate significant improvements in matching workers to jobs.

The third section examines the effect of investments in information gathering on the quality of job matches. Worker productivity and match profitability appear to be higher when employers have obtained recommendations from individuals who know the candidate well. Workers hired on the basis of a reference obtained from a previous supervisor or from someone whose recommendation had been used in the past were significantly more productive. Those hired on the basis of a recommendation by a personnel office were significantly less productive and profitable than those recruited through friends, relatives, and current employees.

I also investigated the quality of matches facilitated by labor market intermediaries such as schools, the U.S. Employment Service (ES) in

3. Boyan Jovanovic, personal communication, July 17, 1992, summarizing the findings of Jovanovic and Moffitt (1990).

the Labor Department, and private employment agencies. New hires referred by public agencies were significantly less profitable, significantly less productive, less willing to stay late to complete work, and more likely to be fired. Those recruited through advertisements were less productive, less likely to make suggestions for improving productivity, and more likely to be fired. Those recommended by a vocational teacher were significantly more profitable after six months.

The last section reviews other evidence on the effectiveness of the Employment Service. The share of new hires placed by the ES has fallen from about 20 percent in the early 1960s to roughly 7 percent in the late 1980s. Only 18 percent of its registrants found a job through an ES referral in 1990, and the quality of these jobs was not high. An evaluation of the ES in 1980–81, funded by the Department of Labor, found that women who received a referral had shorter spells of unemployment and a higher probability of being employed than women who applied at the ES but were not given a referral. Receiving an ES referral did not increase employment rates for men, however, and did not improve the quality of the jobs obtained by either men or women.

How Effective Is the Matching of Young Workers to Jobs?

A 1987 survey by the NFIB offers unique data on the effectiveness of job matching. A four-page questionnaire was mailed to approximately 11,000 firms; 2,599 responses were eventually obtained.[4] The questionnaire focused on the firms' experiences in hiring and training workers in a particular job. Owners were asked to select the job for which they had hired the most people in the past two or three years; if they had done a lot of hiring for more than one job, they were asked to answer questions regarding the job that required the greatest skill. After a series of general questions about the character of the job and the

4. The 500,000 members of the NFIB were stratified by employment, and large firms were oversampled. Salaried managers in charge of subunits of large, publicly owned corporations are not eligible for membership in the NFIB, so the sample does not contain data on job matches at large firms with many establishments. Business owners who had no employees in the previous year or who had not hired anyone in the last three years were asked to check a box and send back the blank questionnaire; 569 of the returned questionnaires were of this type.

worker qualities that were sought, the manager was asked to select two individuals who had been hired for the job and answer all future questions with reference to those two workers. The selection was made in response to the following question:

> Please think of the last person hired for this job (job X) by your firm *prior* to August 1986 *regardless of whether that person is still employed by your firm.* Call this individual person A. The individual hired for job X immediately before person A is called person B. Do not include rehires of former employees.

The owner was then asked several questions about these two employees. Information was obtained on 1,624 person *A*s and 1,403 person *B*s. Managers were asked to report starting and current wage rates and to rate the worker's productivity "on a ratio scale from zero to 100" at three different points: the sixth day after starting work, the end of the first six months," and "currently or two weeks before leaving the firm."[5] Owners were also asked to state what they had expected the new hire's productivity to be after six months.

The difference between this retrospective report of what was expected at the end of six months and the respondent's report of actual productivity provides a measure of the accuracy of hiring decisions. Actual productivity at six months (mean = 64.6 on the 0–100 scale) was 12 percent less on average than expected productivity (mean = 73). Although only 16.5 percent of the new hires did five or more points better than expected, 50.4 percent did five or more points worse than expected, and 25.8 percent did twenty or more points worse.

Differences in actual productivity between two workers on the same job at the same firm during the same six months were large and poorly predicted at the time of hiring. The standard deviation of the differential between realized productivity of person *A* and person *B*, $SD(P_{Aj}^{6m} - P_{Bj}^{6m})$, was 33 percent of the mean level of productivity at six months of tenure. The standard deviation of the discrepancy between person *A*'s surprise and person *B*'s surprise, $SD[(P_{Aj}^{6m} - P_{Aj}^{EX}) - (P_{Bj}^{6m} - P_{Bj}^{EX})]$, was 31 percent of mean productivity. (Here, surprise means realized minus expected.) Thus, the ratio of the variance of the surprise

5. See appendix B for an evaluation of the degree to which respondents used the requested ratio scale when they reported on the relative productivity of the two new hires.

Table 1. Distribution of Job Tenure for Selected Countries
Percentage

		Job tenure		
Country	*Year*	*Under one year*	*Under two years*	*More than five years*
Australia	1981	25.0	38.8	37.2
Belgium	1972	n.a.	24.8	51.1
Canada	1983	22.7	33.1	45.3
France	1978	n.a.	17.8	62.5
	1984	n.a.	n.a.	57.5
Germany	1972	n.a.	25.0	51.0
	1985	n.a.	18.6	63.0
Italy	1972	n.a.	20.0	49.7
Japan	1982	9.8	21.2	66.8
Netherlands	1972	n.a.	25.2	50.3
United Kingdom	1979	13.8	24.4	52.4
United States	1983	27.3	38.5	39.6
	1987	28.8	40.1	40.5

Source: OECD (1984, table 32, p. 57); Buchtemann and Standing (forthcoming); and U.S. Department of Labor, Bureau of Labor Statistics (1987).
n.a. = not available.

differential to the variance of the realized, six-month productivity differential was 0.88. The variance of the surprise differential was three times the variance of differentials in expected productivity.

Another indicator of the quality of job-worker matches is how long they last. The job tenure distributions presented in table 1 provide one yardstick. In 1987, 40 percent of American workers had been on their current job less than two years. With the exception of Australia, no other industrialized nation had such a high proportion of short-tenured employees. For workers with less than a year on the job, the probability of a separation in the next twelve months was 59 percent in the United States and 24 percent in Japan.[6] Adjusting tenure distributions for workers' ages diminishes the tenure gap somewhat but does not eliminate it.

Job shopping and hiring trials are more common in the United States than in Japan and Europe. The data on surprises in job performance suggest that poor initial matchmaking may be one of the causes of this phenomenon. Institutional barriers that prohibit the free flow of infor-

6. OECD (1984, tables 33 and 34, pp. 57, 59).

mation about job applicants—such as equal employment opportunity testing guidelines, the failure of some high schools to provide transcripts, variations in grading standards, and the threat of law suits if bad recommendations are given—do not exist in other countries. German and Japanese employers also appear to be more careful than American employers are in their selection of blue-collar and clerical employees.[7]

Poor initial matchmaking imposes significant social costs. Training a new employee costs, on average, about one month's wages, and much of that training is specific to the firm.[8] Fired workers experience some period of unemployment. Although turnover helps workers find a better match, it reduces the benefits of on-the-job training and discourages investments in firm-specific skills.[9] Many of those trained move on to other firms where the firm-specific components of their previous training yield no benefits. Moreover, turnover disrupts learning whether the skills being learned are generic or firm-specific.

The Correlates of the Surprise in Productivity

Proposals to improve the signaling of information about worker skills and to reform the ES need to be based on an understanding of hiring decisions. This section examines the characteristics that predict successful job matches.

Empirical Models

Assume that in a sample of recent hires that the outcome of a match between job j and worker i, Y_{ij}, depends upon a vector of the individual's characteristics (X_i), the characteristics of the match (selection procedures, for example) (S_{ij}), and characteristics of the job and the firm (Z_j). Individual characteristics and recruitment sources have different effects on productivity, turnover, and profitability of new hires both

7. Rosenbaum and Kariya (1989).

8. Bishop (1991b).

9. Employers will invest in specific training if it generates a monthly return that exceeds the sum of the monthly turnover rate (generally above 2 percent a month in the United States and often greater than 10 percent a month, particularly for new hires) and the cost of capital (which is about 1 to 1.5 percent a month). Monthly turnover rates are typically much larger than the monthly cost of capital and also vary more among firms.

within and across firms.[10] A simple specification that takes this into account is:

$$(1) \quad Y_{ijk} = \beta_k'(X_i - X_j) + \beta_k(S_{ij} - S_j) + A_k'X_j + A_kS_j + \phi_kZ_j + u_{ijk} + v_{jk},$$

where

- Y_{ijk} is the k^{th} measure of match success between employee i and job j. Measures include turnover, wage rate, and reports from supervisors about the worker's productivity and profitability.
- X_i is a vector of background characteristics of individual i that describe generic competencies (means of these characteristics for a job are X_j),
- S_{ij} is a vector of characteristics of the match between worker and job (for example, recruitment source, reference checks, and training) that affect performance in job j (means of these characteristics for a job are S_j),
- Z_j is a vector of measurable characteristics of the job j, including characteristics of the employer,
- u_{ijk} is a random error specific to the match between individual and the job.
- v_{jk} is an error specific to the job or employer respondent.

The β'_ks and β_ks characterize the within-job relationship between measures of match success and the hiring process that led to the match; the A'_ks and A_ks characterize the average relationship across jobs. Economists, however, seldom have the data necessary to estimate equation 1. Supervisory ratings of skills and job performance and measures of individual output are all relative within any one firm. Moreover, different jobs require different measures, and firms do not use comparable measures. Second, specific means of X and S are not available.

For measures that have metrics that are comparable across jobs (such as wage rates, turnover, absenteeism, and percentage changes in productivity), the following model is commonly estimated:

10. There may also be variation of the β_ks across jobs and firms. If such variation is important, the models estimated, which impose equality of β_ks across jobs and firms, are estimating an average β_k.

$$(2) \qquad Y_{ijk} = b_k' X_i + b_k S_{ij} + \phi_k Z_j + v_{jk} + u_{ijk}.$$

Table 2 contains estimates of this model. The assumption that A_k' and A_k equal β_k' and β_k is fine in some applications but a problem in others. Additionally, if unmeasured characteristics of the individual, the job, and the firm (v_{jk}) are correlated, one cannot obtain unbiased estimates of these parameters.

If one is interested in within-job relationships, using only the βs, both of these problems can be finessed. In contrast to industrial psychologists who obtain estimates of the βs by analyzing workers on the same job at the same firm, I compare two people in the same job at the same firm as a function of differences in their background characteristics and differences in the method by which they were recruited. This model differences equation 1 across workers *A* and *B*.

$$(3) \quad Y_{Ajk} - Y_{Bjk} = \beta_k'(X_A - X_B) + \beta_k(S_{Aj} - S_{Bj}) + (u_{Ajk} - u_{Bjk}),$$

where person *A* and person *B* both work in the same job *j*. Least squares estimates of this model are unbiased if the X_is and the S_{ij}s are not correlated with the u_{ijk}s.

In estimating equation 3, the purpose is *not* to estimate a structural relationship between worker traits and job performance for the entire population of workers and jobs. The process by which employers chose *A* and *B* is not revealed by the NFIB. Nor is it revealed by data from the Employment Opportunity Pilot Projects (EOPP) Employer Survey, a similarly structured survey conducted in the spring of 1982 (see appendix A for a description).[11] I am examining instead conditional expectation functions describing the relationship among personal characteristics, hiring process variables, and measures of the success of the match. In fact, that job incumbents are a selected sample is critical to the hypotheses being tested.

Do Employers Rationally Predict Job Performance?

As the previous section showed, employers' predictions of the productivity of new hires are imperfect. Is this because they lack infor-

11. Structural models of the relation between background and performance in a sample of job applicants cannot be estimated without bias using these data because the sample is truncated (the applicants who were believed to have low productivity were not hired, so observations on their job performance are not available) and because many of the traits used to select new hires are unmeasured. See Brown (1982).

mation or because they misuse the information they have? One can distinguish between these two explanations by using objective information in the data set that was available to the employer when the hiring decision was made to try to predict productivity surprises. If surprises cannot be predicted, then the problem is not irrational use of available information. Of course, it cannot be said with certainty that employers' predictions are rational, because not all information available to them was included in the data set.

Many employers were remarkably casual about their hiring selections. Although more than half of all new hires had received relevant formal training from a previous employer, only 7 percent of new hires had shown their prospective employer a certificate of training received on previous jobs. It should come as no surprise, therefore, that employers sometimes learn only later about previous training. Thus, one might expect previous training to predict productivity surprises. Specifically, I consider (a) whether the discrepancy between realized and expected productivity is predictable by information on worker characteristics that was available when the hiring decision was made, and (b) whether it is predictable by new information on worker quality that was not available when the worker was hired.

Table 2 presents evidence on the predictability of the surprise in productivity realizations. The first two columns report estimates of equation 2. The within-firm difference models (equation 3) are found in the third column. (An expanded model, including hiring process variables, appears in column 1 of table 8.) My hypothesis that expectations were rational is supported by the insignificance of worker characteristics known at the time of hiring and of joint F-tests on the background and recruitment source coefficients in each regression. The inability of the background variables to predict the surprise in productivity realizations contrasts with their ability to predict expected productivity (column 4).

The few exceptions provide some support for the second hypothesis. Employers were pleasantly surprised by the productivity of workers with relevant work experience and unpleasantly surprised by those with irrelevant work experience (typically older workers). Because employers could easily measure total work experience before hiring, they should have foreseen the combined effect of these two variables. Other than

Table 2. Ability of Worker Characteristics to Predict the Surprise in Productivity Realizations and Tenure

	Surprise in productivity			*Expected productivity*	*Logarithm of actual tenure*		
Worker Characteristics	*Person A*	*Person B*	*Firm fixed effect*	*Fixed effect*	*Person A*	*Person B*	*Firm fixed effect*
Years of schooling	−0.004 (0.006)	0.0004 (0.0067)	0.011 (0.008)	0.004 (0.004)	0.002 (0.011)	0.011 (0.015)	0.004 (0.019)
Years of classroom occupational training	0.002 (0.007)	−0.002 (0.008)	0.012 (0.010)	0.006 (0.005)	0.0012 (0.013)	0.011 (0.015)	0.017 (0.023)
Did not complete high school	. . .	. . .	. . .	. . .	−0.152** (0.066)	−0.119 (0.097)	−0.110 (0.100)
Years of relevant work experience	0.0079* (0.0047)	−0.0008 (0.0055)	0.013** (0.005)	0.0110*** (0.0026)	−0.016** (0.008)	0.001 (0.011)	−0.006 (0.012)
Years of relevant work experience squared	−0.0001 (0.0002)	0.0001 (0.0002)	−0.0002 (0.0002)	−0.00024** (0.0007)	0.0006* (0.0003)	0.0002 (0.0004)	0.0004 (0.0004)
Formal on-job training	−0.029 (0.020)	−0.010 (0.024)	−0.049* (0.026)	0.041*** (0.013)	0.017 (0.033)	−0.063 (0.049)	−0.005 (0.060)
Formal off-job training	−0.006 (0.037)	0.001 (0.044)	0.027 (0.054)	0.025 (0.026)	0.110* (0.060)	−0.136 (0.089)	0.014 (0.120)
Age 18	−0.0050* (0.0029)	0.0025 (0.0039)	−0.007* (0.004)	0.0003 (0.0018)	−0.0017* (0.0049)	0.0024 (0.0077)	0.008 (0.008)
Age 18 squared	0.0001 (0.0001)	−0.0001 (0.0001)	0.0001 (0.0001)	−0.0000 (0.0000)	−0.0001 (0.0001)	−0.0000 (0.0002)	−0.0001 (0.0002)

Female	0.040	−0.026	−0.003	−0.007	0.024	−0.030	−0.037
	(0.026)	(0.031)	(0.037)	(0.018)	(0.045)	(0.063)	(0.082)
Black	−0.039	−0.035	−0.029	−0.038	−0.133*	−0.016	0.056
	(0.049)	(0.057)	(0.050)	(0.025)	(0.081)	(0.113)	(0.111)
Hispanic	−0.020	0.012	0.010	−0.041	−0.171**	0.132	0.089
	(0.047)	(0.056)	(0.054)	(0.027)	(0.082)	(0.123)	(0.128)
Married	−0.005	0.060*	0.045*	0.010	0.042	0.001	0.088
	(0.028)	(0.035)	(0.027)	(0.013)	(0.049)	(0.072)	(0.061)
Husband	0.027	−0.095**	−0.049	−0.008	0.017	0.035	−0.147*
	(0.036)	(0.043)	(0.035)	(0.017)	(0.060)	(0.089)	(0.078)
Temporary job	0.055	0.060**	−0.009	−0.005	−0.097**	−0.138**	−0.237***
	(0.025)	(0.029)	(0.035)	(0.017)	(0.041)	(0.059)	(0.076)
Log time since hire	. . .	. . .	. . .	. . .	0.992***	0.966***	0.964***
					(0.022)	(0.027)	(0.035)
Log time since hire squared	. . .	. . .	. . .	. . .	0.017	0.014	0.008
					(0.011)	(0.010)	(0.015)
Adjusted R^2	0.0022	−0.0021	0.0075	0.0845	0.6545	0.5724	0.4478
F test on inclusion of 13–14 background variables	1.16	0.87	1.45	7.02***	1.87**	0.97	1.02
Root mean square error	0.280	0.305	0.324	0.167	0.506	0.678	0.800
Number of observations	1,021	842	833	914	1,194	990	985

Source: Author's estimates of equations 2 and 3 predicting differences in productivity surprises, expected productivity, and tenure, using NFIB data. Standard errors are in parenthesis.
*Implies Prob. LT .10 on a two-tail test.
**Implies Prob. LT .05 on a two-tail test.
***Implies Prob. LT .01 on a two-tail test.

Table 3. Self-described Accuracy of Employer Predictions of Employee Skills

Assessment	*Mean*	*Standard deviation*
At end of six months		
Overall job performance	7.59	1.14
Before hiring		
Reading, writing, math, reasoning ability	5.64	1.95
Job skills already acquired	5.29	1.86
Ability to learn new skills	5.13	1.85
Work habits, attitude	5.08	2.01
People skills	5.32	1.95
Leadership ability	4.61	1.91

Source: NFIB survey. Responses of employers to the question: "How accurately do you feel you can assess each of the following abilities in employees prior to hiring them?" Ratings were made on a scale of 1 to 9, with 1 signifying that the employer was "not at all" confident the assessment was accurate and 9 signifying "absolute accuracy." Employers were also asked to use the same scale to assess overall job performance at the end of six months.

sample selection bias, it is unclear what process leads to their opposite signs.

Columns 5, 6, and 7 of table 2 try to predict a worker's tenure with the firm. Tenure, measured as of the interview date, is one indicator of the quasi rents being generated by a match. Tenure reflects the satisfaction of both the worker and the employer with the match (not just employer quasi rents, as with the surprise variable). Although there is some weak support for Weiss's hypothesis that a high school diploma is a sign of a "sticks-to-it" character trait, the background variables consistently failed joint significance tests.[12]

Why Are Employers Surprised?

It appears that employers in the data set rationally used objective information on age, years of schooling, and relevant work experience to predict job performance. What, then, caused the frequent disappointments? Employers in the NFIB sample were asked to rate, on a scale from 1 to 9, the accuracy with which they felt they could assess six separate abilities in prospective employees. A rating of 1 signified "not at all," and 9 signified "absolute accuracy." The mean and standard deviation of these ratings are presented in table 3. The trait

12. Weiss (1985).

that employers felt best able to predict before hiring was reading, writing, math, and reasoning ability. Next came people skills and occupational skills at the time of hire. Work habits and ability to learn new occupational skills were more difficult to predict. Leadership ability was the trait employers felt least able to predict accurately. Employers felt they were very good at measuring overall job performance of workers after six months on the job. The mean of their assessments of the reliability of their performance ratings was 7.6 on the 1-to-9 scale.

These findings suggest that errors in predicting the work habits, learning ability, and leadership ability of new hires may account for most of the hiring mistakes (negative surprises). Such a conclusion, however, requires evidence that work habits, learning ability, and leadership are important determinants of overall job performance. It must also be determined that it is indeed difficult to make accurate predictions about work habits, learning ability, and leadership potential.

The NFIB data offered an interesting way to search for answers to these questions. The employers were asked "which of the two employees (*A*/*B*) proved better" on each of the following: occupational and job skills, ability to learn new occupational and job skills, work habits and attitudes, people skills (teamwork, appearance, getting along), leadership ability (organize, teach, and motivate others), and reading, writing, math, and reasoning ability. They were asked to evaluate whether *A* was much better, better than, or no different from *B* or whether *B* was much better or better than *A*. In most cases employers perceived differences between the two workers. In 33 percent of the cases, one was judged to have much better occupational skills than the other. Only 22 percent of the pairs were judged to be no different in occupational skills, only 30–31 percent of the pairs were judged to be "no different" in learning ability and work habits. Leadership was judged to be no different 40 percent of the time, and reading, writing, math, and reasoning skills were judged no different 42 percent of the time.

The ratings of relative ability have positive correlations, ranging from 0.72 for the correlation of learning ability differential ratings with occupational skill differential ratings to 0.47 for the correlation of basic academic skills ratings with work habit and people skill ratings. The correlation between ratings of learning ability and basic academic skills was 0.65. Correlations are only slightly lower when the sample is limited to pairs where both members are still with the firm. Individuals

in a particular job do not appear to have offsetting strengths and weaknesses; employees judged to be good at one aspect of a job tend to be judged good at other aspects as well.

The contribution of skill and personality traits to productivity surprises can be estimated by regressing productivity surprises on worker traits. These descriptive regressions show how much of the overall productivity surprise can be explained by *ex post* ratings of relative skill and personality, the accuracy of the *ex ante* predictions, and which worker skills and traits appear to be critical to productivity. Similarly, regressions explaining differences in job turnover between A and B can assess which employee traits (as perceived *ex post* by the employer) reduce turnover. These analyses also may provide clues about ways to improve job matching. The models reported in table 4 have the form:

$$(4) \qquad Y_{Ak} - Y_{Bk} = \beta_0 + \beta_k \Delta R_{jm} + u_{Ak} - u_{Bk}, \text{ and}$$

$$(5) \quad F_A - F_B, Q_A - Q_B = \beta_0 + \beta \Delta R_{jm} + \beta_1 \ln(PT_A/PT_B) + \beta_2[(\ln PT_A)^2 - (\ln PT_B)^2] + u_A - u_B,$$

where person A and B both work in the same job j, and

ΔR_{jm} is the rating of employee A relative to B on ability dimension m. (The rating variable is $+2$ when person A is much better, $+1$ when person A is better, 0 when there is no difference, -1 when person B is better, and -2 when person B is much better.

F_A, F_B = a dummy variable equal to 1 when A (B) is dismissed, laid off, or induced to quit.

Q_A, Q_B = a dummy variable equal to 1 when A (B) quits voluntarily.

PT_A, PT_B = the potential tenure of A and B. Potential tenure is the calendar time between the date of hire and the date the questionnaire was completed.

Because the traits that enhance job performance may vary across occupations, separate analyses were conducted in four categories of occupations: craft workers, operatives and laborers; service workers and sales workers at all levels; low-skill, white-collar workers; and

professional, technical, and managerial workers and sales representatives.

The results show *ex post* judgments of worker skills and traits to be highly correlated with productivity surprises. Indeed, that correlation is higher than the correlation of worker skills and traits to expected productivity. The R^2s of the surprise equations lie between 0.36 and 0.42, while the R^2s of the expected productivity relationships range from 0.08 to 0.16 (see table 4).

EFFECTS OF WORK HABITS. Work habits have the strongest correlation with productivity surprise. For operatives and craft workers, the largest group, the negative productivity surprise was substantially smaller (13.4 percent—two times 6.7 percent—of mean productivity at six months) for the member of the matched pair judged *ex post* to have much better work habits. The screening process appears to have predicted incorrectly which worker would have the best work habits. The expected productivity of the individual who *ex post* had much better work habits was 4.2 percent *lower* at the time of hiring than the expected productivity of the second worker.

Work habits also have a strong association with turnover. The worker judged *ex post* to have much better work habits was twelve to nineteen percentage points less likely to be forced to leave. That worker was twenty-four percentage points less likely than the worker with poorer habits to quit a low-skill white-collar job and 9 percentage points less likely to quit a craft and operative job. Good work habits are clearly important determinants of a worker's success on the job. When asked to rank the six traits in order of importance in their hiring selections, the NFIB employers placed work habits at the top.

If those with good work habits could be identified in advance, selecting new hires on the basis of this trait would probably be highly profitable. Personality tests that measure a "dependability" trait have been developed, and research conducted in both military and civilian settings suggests that dependability scores do predict some dimensions of job performance.[13] Some large firms and placement agencies are increasing their use of these tests, but their spread has been slowed by concerns about their acceptability to applicants and the possibility that applicants may learn how to fake the preferred personality profile.

13. Hough (1988).

Table 4. Apparent Causes of Productivity Surprises and Turnover: Employer Ex Post Assessments of Worker Skills by Occupation

	Ex-Post Employer Ratings of Relative Skill Levels										
	Reading, writing, math	*Job skills at hire*	*Learning ability*	*Work habits*	*People skills*	*Leader-ship*	*Potential tenure (yrs)*	*Potential tenure squared*	R^2	*Root mean square error*	*Number of observations*
Productivity expected at six months											
Professional	0.018	0.044***	−0.031**	−0.002	0.004	0.024*	. . .	. . .	0.137	0.158	232
	(0.013)	(0.011)	(0.013)	(0.013)	(0.013)	(0.014)					
Sales, service	0.032**	0.033**	−0.037**	−0.008	0.016	0.042***	. . .	. . .	0.161	0.174	209
	(0.014)	(0.013)	(0.015)	(0.016)	(0.015)	(0.016)					
Low-level	0.020	0.009	−0.020	−0.002	0.001	0.037***	. . .	. . .	0.081	0.142	198
white-collar	(0.015)	(0.013)	(0.014)	(0.013)	(0.012)	(0.013)					
Craft, opera-	0.013	0.058***	−0.029**	−0.021*	−0.013	0.036**	. . .	. . .	0.148	0.169	321
tive	(0.012)	(0.010)	(0.012)	(0.011)	(0.013)	(0.015)					
Productivity surprise at six months											
Professional	−0.003	0.054***	0.057**	0.100***	−0.000	0.013	. . .	. . .	0.418	0.274	210
	(0.023)	(0.020)	(0.024)	(0.024)	(0.024)	(0.026)					
Sales, service	−0.026	0.061***	0.070***	0.073***	−0.012	−0.011	. . .	. . .	0.370	0.269	188
	(0.024)	(0.022)	(0.025)	(0.026)	(0.026)	(0.026)					
Low-level	0.024	0.046*	0.048*	0.095***	−0.029	−0.005	. . .	. . .	0.387	0.264	182
white-collar	(0.029)	(0.024)	(0.027)	(0.024)	(0.023)	(0.024)					
Craft, opera-	−0.021	0.025*	0.031*	0.067***	0.022	0.030	. . .	. . .	0.359	0.220	290
tive	(0.016)	(0.014)	(0.017)	(0.015)	(0.018)	(0.020)					

Involuntary quit											
Professional	−0.036	−0.011	−0.054	−0.075**	−0.093***	0.066*	−0.044	0.004	0.207	0.411	257
	(0.033)	(0.028)	(0.034)	(0.033)	(0.033)	(0.036)	(0.042)	(0.005)			
Sales, service	−0.047	0.023	−0.042	−0.093***	−0.068**	0.052	−0.012	0.004	0.195	0.381	241
	(0.029)	(0.028)	(0.033)	(0.034)	(0.032)	(0.032)	(0.052)	(0.008)			
Low-level white-collar	−0.038	0.015	−0.049	−0.081**	−0.024	0.019	−0.014	0.000	0.155	0.382	241
	(0.035)	(0.032)	(0.036)	(0.032)	(0.031)	(0.032)	(0.042)	(0.005)			
Craft, operative	0.079***	−0.037	−0.047	−0.059**	−0.060*	−0.015	0.102**	−0.010*	0.158	0.423	363
	(0.029)	(0.023)	(0.029)	(0.026)	(0.031)	(0.035)	(0.042)	(0.005)			
Voluntary quit											
Professional	0.072*	−0.035	0.006	−0.001	−0.013	−0.091**	0.147***	−0.008	0.080	0.527	257
	(0.042)	(0.035)	(0.043)	(0.042)	(0.043)	(0.046)	(0.054)	(0.007)			
Sales, service	0.058	−0.007	0.028	−0.065	0.007	−0.098**	0.098	−0.004	0.054	0.563	241
	(0.043)	(0.041)	(0.049)	(0.050)	(0.047)	(0.048)	(0.077)	(0.011)			
Low-level white-collar	0.092*	0.045	−0.059	−0.122**	−0.028	0.047	0.247***	−0.018**	0.129	0.571	241
	(0.052)	(0.047)	(0.054)	(0.048)	(0.046)	(0.049)	(0.063)	(0.008)			
Craft, operative	−0.024	−0.012	−0.021	−0.047*	−0.031	0.005	0.052	0.001	0.093	0.455	363
	(0.031)	(0.025)	(0.031)	(0.028)	(0.033)	(0.038)	(0.045)	(0.005)			

Source: Estimates of equations 4 and 5 predicting differences in expected productivity, productivity surprises, and turnover of two recent hires for the same job at the same firm using NFIB data. Expected productivity and the surprise were both normalized on the mean productivity rating of all workers at six months. Professional includes technicians, managers and high-level sales personnel. Involuntary turnover includes layoffs, dismissals, and induced quits. The independent variables were the manager's *ex post* rankings for six ability dimensions of employee *A* relative to employee *B*. Potential tenure is the time between the individual's hire date and the approximate date the questionnaire was completed in years. Standard errors are in parentheses.

*Prob. LT .10 on a two-tail test

**Prob. LT .05 on a two-tail test.

***Prob. LT .01 on a two-tail test.

Reference checks with both schools and former employers may provide an alternative means of assessing work habits.

EFFECTS OF LEARNING ABILITY. The ability to learn new occupational and job skills also appears to have been poorly anticipated by employers. *Ex post* evaluations of learning ability had a significant negative relationship with expected productivity at six months and a significant positive relationship with positive productivity surprises. Learning ability may not have met expected productivity in part because employers may hire some workers who were expected to be good learners but who were not expected to reach full productivity by the end of six months. The key finding, therefore, is the large positive effect of *ex post* judgments of learning ability on positive surprises. This implies that many of the differences between workers in learning ability that can be seen after the worker has been at the firm a year or so were not anticipated at the time of hiring.

Learning ability had no systematic relationship with quit probabilities. In the full sample (not shown), there was a significant negative relationship with involuntary turnover. Those rated much better in learning ability were eight to eleven percentage points less likely to be dismissed.

These findings suggest that improved *ex ante* measures of the ability to learn new occupational and job skills might substantially improve job matching. Because the ability to learn new occupational skills is difficult to measure directly, it is sometimes proposed that job applicants be tested in English and mathematics. Competence in these areas presumably facilitates learning new occupational skills. Research in the military, however, suggests that the ability to learn how to do a nonclerical job is more highly correlated with prior technical knowledge and competence than with prior competence in math and English.[14] Spatial ability and dependability scores are also highly correlated with measures of the ability to do a job.[15] The analysis of the NFIB data produces results consistent with the military research. Although ability to learn occupational skills had a 0.65 correlation coefficient with academic skills, it also had a 0.66 correlation with leadership, a 0.61 correlation with work habits, and a 0.72 correlation with current oc-

14. Bishop (1991a).
15. Hough (1988).

cupational skills. The ability to learn new occupational and job skills thus does not appear to be synonymous with academic ability. Again, reference checks may offer another way to predict learning ability.

EFFECTS OF LEADERSHIP ABILITY. The data seemed to contradict my hypothesis that poor predictions of leadership ability were responsible for some of the disappointments. Workers who were judged *ex post* to have leadership qualities had been expected to be more productive at the six-month milestone, and they were indeed more productive. There was no tendency for *ex post* judgments of leadership to be associated with smaller negative productivity surprises.[16] In high-level white-collar and sales occupations, leadership qualities were associated with higher probabilities of being fired and lower probabilities of quitting. These results suggest that developing better predictors of leadership qualities need not be a high priority, at least for the small companies and entry-level jobs studied here.

EFFECTS OF OCCUPATIONAL SKILLS. Employers ranked a prospective employee's existing occupational and job skills the fourth most difficult to evaluate, and the regression evidence confirmed that occupational skills are imperfectly signaled. Although the *ex post* evaluation of occupational skills had a significant positive relationship with expected productivity, it also had a significant relationship with surprises. Workers who were rated *ex post* as much better in occupational skills had smaller negative surprises in productivity (5 to 12 percent of mean productivity) at the six-month milestone than other workers. There does not appear to be any relationship between occupational skills and turnover.

EFFECTS OF PEOPLE SKILLS. The second most predictable trait was people skills. No significant relationships between this rating and productivity surprise were found at the six-month milestone. Neither was there a relationship with expected productivity, suggesting that people skills were not an important determinant of overall job performance.

16. The contradictory evidence on leadership may be due to the way the question was asked. Employers were asked to predict leadership ability, which was defined as the ability "to organize, teach and motivate others, and solve problems. However, the question asking for a comparison of leadership ability in the two recent hires inadvertently left out the words "solve problems." One way to make the results from the two questions consistent is to conclude that employers are able to predict the ability to organize and to teach and motivate others, but not the ability to solve problems.

This was the case at the six-month milestone, but not for productivity at the time the questionnaire was completed (not shown) or for involuntary turnover. Workers rated much better in people skills were 9 percent more productive at the time the questionnaire was filled out (in an aggregate model not shown) and five to nineteen percentage points less likely to be dismissed.

EFFECTS OF ACADEMIC ABILITY. Employers thought it easiest to evaluate the reading, writing, math, and reasoning ability of prospective employees, and the regression evidence supports that judgment. The *ex post* rating of basic academic ability had no significant relationship with the surprise, but it did have a modest positive association with expected productivity, which was statistically significant in the sales and service occupations and in the full sample. Probably because greater academic skills improved a worker's chances in the external labor market, workers with high academic abilities were more likely to quit white-collar jobs. In craft and operative occupations, they were more likely to be fired.

These results suggest that improved signals of basic academic skills may not be as effective in making better job matches as improved signals of other worker skills would be. Basic academic skills, however, can be measured more cheaply and reliably than other worker skills. Tests can be taken in just a few hours, cost $10 to administer, and are highly reliable measures of academic skills; numerous studies have demonstrated that these tests are valid predictors of performance in a great variety of jobs.[17] One reason why these tests predict job performance so well is because people with good academic skills tend to have good work habits and the ability to learn new occupational skills quickly—two important worker skills that are difficult to measure directly. The need to generate incentives for students to take demanding courses and to study hard is an additional reason for investing in improved signaling of academic skills.

Better measures of work habits, occupational skills, and the ability to learn new occupational and job skills should reduce the mismatches between workers and jobs and the disappointments and turnover that result. However, even expensive, sophisticated *ex ante* measures of these traits will have much lower correlations with job performance and

17. Hunter (1983); Hartigan and Wigdor (1989); and Wigdor and Green (1991).

turnover than the *ex post* assessments of these traits used here. The relationships described above suggest some places to look for underutilized selection criteria that could reduce mismatches. A presumed relationship between work habits, learning ability, and/or occupational skills and a proposed selection criterion does not, however, justify its immediate adoption. The specific selection criterion must be validated empirically.

Does More Careful Screening and Selection Improve the Job Match?

This section analyzes four specific selection criteria—recruitment sources, reference checks, performance tests, and training certificates—to determine their effects on the job match. Because the data come from employers, match success is defined primarily from their point of view. Four indicators of match success are examined: the surprise in productivity realizations, the profitability of new hires, the productivity of new hires, and turnover. Two of these—surprise and profitability—measure the firm's share of the quasi rent generated by the match. Indicators of worker productivity can be viewed as proxies for the sum of the wage paid the worker and the quasi rent received by the firm. Turnover is a negative indicator of the sum of the quasi rents generated by the match.

Literature Review

Hiring new employees involves recruiting a pool of job candidates and then gathering information about them. Applicants follow a similar process, checking available sources of information and job leads and then applying to those firms with job openings that appear to be attractive places to work. The better the information that either party has before the hiring decision, the better the match between worker and job is likely to be. The recruitment channel is likely to affect the information that the two parties have about each other, which, in turn, may affect the quality of the matches.

RECRUITMENT SOURCE. Conventional wisdom holds that employees recruited through informal channels—friends of the boss and current

employees—are generally more successful than employees found through formal channels—newspaper advertisements and labor market intermediaries.[18] Five hypotheses have been proposed to explain the phenomenon. In his "realistic job preview" hypothesis, Wanous suggested that those recruited through a current employee have longer tenure because they knew what they were getting into when they took the job. Presumably they talk about available jobs with their friend and do not apply for those they think likely to be unsatisfactory. As a result applicants who are friends of current employees are more likely to perceive work conditions as satisfactory and stay with the job.[19] The "screening" hypothesis posits that current employees screen job-seeking friends. Sometimes the boss may ask current employees about their friends' reliability and other job qualifications. In other cases, employees may encourage a particular friend to apply because they are confident the individual's performance will reflect well on them. The "showing-the-ropes" hypothesis posits that current employees help their newly hired friends to learn the job and to be accepted by coworkers, which increases both performance and tenure. In the "teaches-you-the-limits" hypothesis, the friend tells the novice how to avoid trouble with the boss for doing too little and how to gain favor with coworkers by not doing too much. One outcome under this hypothesis is longer tenure, but a second outcome is a reduction in the new hire's effort level and rated job performance.

The "individual difference" hypothesis posits that different recruitment channels tap different pools of potential job candidates and that some of these pools tend to yield higher-quality candidates and longer-tenure employees than others.[20] Under this hypothesis employers encourage satisfactory employees to refer friends. Employers will try to avoid hiring friends of current employees with whom they are dissatisfied.

My search of the literature found ten empirical studies of the effect of recruitment source on turnover, job performance, and absenteeism. Most of the studies were based on small samples of employees from a

18. Many of the studies cited below were stimulated by the pathbreaking work of Albert Rees (1966).

19. J. P. Wanous (1980).

20. Schwab (1982).

single firm. The findings of these studies are summarized in table 5. Turnover, defined as the proportion of new hires recruited through a specific channel who left before the end of the first year, was the most frequently studied outcome. Turnover rate data are available for new hires for nine different jobs and or firms, a group of recently laid-off workers, and a sample of 1,700 new hires at roughly the same number of firms. Even though widely varying jobs and time periods were examined, the results were remarkably consistent. Those recruited through a current employee at the firm invariably had lower turnover rates than those recruited through advertisements or public and and private employment agencies. Walk-ins had higher turnover rates than people recruited through a current employee in six studies of specific companies, in Reid's study of displaced blue-collar workers in the British Midlands, and in Holzer's analysis of EOPP data.[21] In two establishments, however—a social service agency and a technical sales job—the turnover rate for walk-ins was below that of new hires recruited through a current employee.[22]

Four studies of the association between recruitment source and job performance outcomes produced different findings. New employees recruited through a current employee were generally rated as *less* successful and more likely to be absent than walk-ins and employees recruited through newspaper ads. There is little data on the job performance of referrals from public employment agencies. One study with only six such referrals found that the job service referrals performed as well as walk-ins and ad respondents but were absent more often.[23] A study of retail clerks found that 43 referrals from private agencies and the public employment service were 9 to 15 percent more likely to be classified as unsatisfactory than walk-ins and new hires recruited through newspaper advertisements.[24]

Normally tenure and job performance move together. It is not clear whether the contrasting findings are real or due to heterogeneity of the true relationship across firms. Tenure and job performance were studied in only two firms. Only two of the hypotheses discussed above—"learns

21. Reid (1972); and Holzer (1987).
22. Swaroff, Barclay, and Bass (1985); and Breaugh and Mann (1984).
23. Taylor and Schmidt (1983).
24. Caldwell and Spivey (1983).

Table 5. Effect of Recruitment Source on Success of the Match: Review of Previous Studies

Outcome	Walk-in	Newspaper ad	Referral from: Public agency	Referral from: Private agency	Referral from: School	Referral from: Current employee	Referral from: Relative or friend of new hire	Rehires	Number of observations
Turnover first year									
Bank employees[a]	0.288	0.394	. . .	0.390	0.218	0.265	. . .	0.213	6,390
Bank employees[b]	0.428	0.333	. . .	0.478	. . .	0.311	. . .	0.342	514
Insurance agent[b]	0.358	0.426	. . .	0.375	. . .	0.305	. . .	. . .	1,753
Clerical-comp A[c]	. . .	0.880	. . .	. . .	. . .	0.750	. . .	. . .	. . .
Clerical-comp B[c]	. . .	0.740	. . .	0.620	. . .	0.280	. . .	. . .	. . .
Food packaging[d]	0.550	0.597	0.750	. . .	. . .	0.490	. . .	0.360	283
Professional/technical									
Abstract service[b]	0.100	0.210	. . .	0.062	. . .	0.045	. . .	. . .	199
Social service[e]	0.130	0.240	. . .	. . .	. . .	0.170	. . .	. . .	98
Technical sales[f]	0.208	0.268	. . .	0.246	0.239	0.225	. . .	. . .	618
Mix of firms and jobs									
Displaced blue-collar[g]	0.750	0.836	0.773	. . .	. . .	. . .	0.611	. . .	279
Small firm employees[h]	0.341	0.325	0.302	. . .	. . .	0.291	0.139	. . .	1,703

Satisfactory employee									
Retail clerk[i]	0.406	0.446	0.344	i	. . .	0.321	. . .	. . .	1,400
Technical sales[f]	0.688	0.605	. . .	0.762	0.782	0.698	. . .	. . .	618
Supervisory rating									
Food packaging[d]	2.69	2.71	2.71	. . .	. . .	2.47	. . .	2.93	242
Chem/biologist[j]	3.91	3.83	. . .	. . .	3.65	. . .	. . .	. . .	112
Technical sales[f]	3.60	3.60	. . .	3.34	3.38	3.50	. . .	. . .	618
Absenteeism									
Food packaging[d]	2.00	2.09	2.50	. . .	. . .	2.18	. . .	1.49	242
Chem/biologist[j]	3.75	7.47	. . .	. . .	3.50	. . .	. . .	. . .	112

Source: Author's calculations based on data from following sources.

a. Gannon (1971). New hires of a large New York bank between 1961 and 1964.

b. Decker and Cornelius (1979). New hires during 1974 for a bank, during 1972–74 for an abstracting service, and during 1970–74 for sales agents at an insurance company.

c. Ullman (1966), described in Breaugh and Mann (1984)

d. Taylor and Schmidt (1983). Seasonal employees packaging food items. The turnover variable was 1 minus the ratio of actual to potential tenure. The performance variable was the mean of ratings of quantity, quality, and attitude on a 1 to 4 scale where 4 = "above average." The absenteeism variable was 4 minus the absenteeism rating on the same 1 to 4 scale.

e. Breaugh and Mann (1984)

f. Swaroff, Barclay, and Bass (1985). The measure of successful employee is the mean share of the sales quota made in year 1 and 2. The supervisory rating was on a 1 to 5 scale with 5 = "exceptional."

g. Reid (1972). Engineering and metal trades workers displaced from their job in the West-Midlands of England in 1966–68.

h. Holzer (1987). Linear probability analysis of Employment Opportunity Pilot Projects data on most recent hire at 1,703 different firms. Length of the period over which turnover was measured varied but averaged about a year. Model included controls for industry, firm size, unionization, number of current vacancies, training time in first three months, wage of typical employee with two years of tenure, screening methods used to evaluate the new hire, and the new hire's education, age, gender, and previous relevant experience. The model lacked controls for occupation or the time period over which the turnover variable was defined. Predicted turnover was calculated by applying coefficients on recruitment source dummies to known turnover rate for walk-ins.

i. Caldwell and Spivey (1983). Retail clerks hired during 1979, none of which stayed a year or more. Proportions are weighted averages of race specific success rates. The number recruited through the employment service and private agencies was forty-three.

j. Breaugh (1981). BA, MA, and PhD chemists and biologists hired for applied research positions. The supervisory rating was an average of ratings on a scale of 1 to 5 for quality, quantity, dependability, and job knowledge, with 5 = "exceptional." The absenteeism variable is the mean number of days absent per year.

the limits'' and ''realistic job preview''—predict that informal recruitment channels have a positive effect on retention but a negative effect on job performance. Clearly, more research is needed on the topic, and more firms need to be studied.

REFERENCE CHECKS AND PERFORMANCE TESTS. Most employers in the NFIB data set checked at least one reference of the applicant they hired. Forty percent checked with a previous supervisor. Twenty-three percent of the new hires took a performance test—a typing test or job tryout, for example—before being hired. These methods of gathering information impose costs on the firm and are presumably undertaken because they screen out applicants who are unlikely to perform well. Industrial psychologists have conducted numerous studies of the validity of reference checks and performance tests. These studies were conducted by correlating the outcome of interest—generally supervisory ratings, promotion rates, and/or tenure—with reference ratings and performance tests.

Meta analyses of this literature indicate that although some other predictors have higher validity, both reference checks and performance tests have useful levels of validity. Dunnette's review of twenty studies of the validity of job tryouts found a correlation of 0.44, uncorrected for measurement error in supervisory ratings.[25] Reilly and Chao found that the mean correlation between reference checks and subsequent supervisory ratings in seven studies was 0.17, uncorrected for measurement error in supervisory ratings.[26] Hunter and Hunter found mean correlations of 0.18 for supervisory ratings (uncorrected for measurement error based on ten studies with a total sample of 5,389), 0.16 for advancement (three studies with a total sample of 415), 0.23 for training success (one study with 1,553 cases), and 0.27 for tenure (two studies with a total sample of 2,018).[27] The recency of the reference's experience with the applicant seems to matter. Rufus Browning's study of newly hired teachers found validities of 0.20 for references given by the most recent principal or supervisor, 0.07 for an earlier principal or supervisor, 0.09 for the cooperating teacher during practice teaching, and 0.20 for the dean of the college or the head of the college education

25. Dunnette (1972). Quoted in Hunter and Hunter (1984, p. 83).
26. Reilly and Chao (1982). Quoted in Hunter and Hunter (1984, p. 83).
27. Hunter and Hunter (1984).

department.[28] A study of 100 recent high school graduates during their first year at Lockheed Corporation found that days absent from school correlated 0.30 with days absent from work and 0.20 with tardiness at work. Teacher ratings of work habits (and cooperation), found on high school transcripts, correlated 0.44 (0.45) with the supervisor's conduct ratings, 0.41 (0.39) with the supervisor's production rating, and −0.26 (−0.29) with absences. Three-year grade point averages had a correlation of 0.37 with the supervisor's conduct rating and 0.34 with the production rating.[29]

These studies suggest that performance tests and reference checks should improve the quality of the matches that result.[30] Implementation of these selection techniques may, however, fall short of the optimal selection models put forward by industrial psychologists. Since most of the studies of references reviewed above were conducted, a growing number of lawsuits have been lodged against employers who gave unfavorable references about a particular employee. As a result, references have become less honest and may no longer be as useful a screening device. Similarly, the high stakes involved in performance tests may affect behavior in ways that change validity. To deal with this problem, it is useful to study actual selection techniques.

Hypotheses

If assessments of differences in the expected productivity of job applicants grouped by traits such as schooling and training are generally accurate, competition for workers should result in wage offers that reflect expected differences in productivity. Many employers respond to these competitive pressures by paying higher wages to more-qualified new hires, even when they all have the same job title. Predictable productivity differences among new hires at a firm are thus to be expected. This implies that tests of the effectiveness of intensive search

28. Browning (1968).

29. Brenner (1968).

30. It should be noted that these correlations come from samples of job incumbents who have been hired and retained. These two selection processes tend to attenuate the true relationship between predictor and criterion, so opportunities to profit from using these selection techniques are probably more substantial than the quoted validities suggest. See Brown (1982); and Weiss and Landau (1987).

activities must contain controls for the visible characteristics of job applicants that predict job performance and turnover propensities.

The quasi rents generated by a job-worker match—the gap between a worker's productivity and the worker's reservation wage—vary considerably across workers in the same job, and the magnitude is often predictable. Match-specific quasi rents occur when a worker's skills are useful at only a few local firms or when an employer has access to information about the quality of a worker that is not available to other employers. Above-normal quasi rents may also result from a low reservation wage (possibly because one has a close friend or relative at the work site). From the firm's point of view, a wide gap between a worker's productivity and the reservation wage is good, because turnover will be low and the expected profitability of the match will be high (either because of lower wage offers or higher productivity). Thus, indicators of the lack of inside information on a worker's likely job performance (such as recruitment through advertisements or a labor market intermediary) may correlate negatively with the profitability of the match and the productivity of the worker.

The question, therefore, is what methods of evaluating and selecting new hires produce successful job matches, holding observable characteristics of new hires constant. In 60 percent of the hiring decisions, employers did not check the reference of the new hire's previous supervisor. For the other 40 percent, did the extra time devoted to getting such references pay off by helping the employer select more productive and profitable employees?

The threat of lawsuits has made many employers reluctant to give honest references. Personnel offices are particularly sensitive to the legal dangers of giving references, so the information content of their references has probably deteriorated the most. Some personnel offices have been said to be in the lemon-marketing business. Therefore, when a personnel office was the only reference checked (which was the case for 4.5 percent of new hires), is the productivity and profitability of the match likely to be low?

The credibility of the individual providing a reference is obviously important. Hiring a second or third time on the recommendation of a particular person may indicate satisfaction with the accuracy of the previous information. Twenty-five percent of new hires were based on

such recommendations. But are these new hires more productive and profitable than other new hires?

Twenty-three percent of the new hires at the NFIB firms were referred or recommended by a relative or personal friend of the hiring employer. Presumably, an employer can trust personal friends and relatives to give honest references, but is it reasonable to hypothesize that employees who were hired at their behest are more likely to be successful on the job?

Are employer predictions about future job performance more accurate for new hires recruited through informal channels than for new hires recruited through formal channels such as newspaper advertisements and the public employment service? If so, new hires recruited through channels that provide lower-quality information about applicants are likely to be less productive and less profitable and to have higher turnover.

Specification and Data

These issues are addressed by estimating within-firm difference models. These models regress measures of job match success on dummy variables for types of reference checks, recruitment source, and a comprehensive set of background characteristics. The analysis uses both the NFIB and EOPP survey data. Both data sets measured schooling, relevant public and private vocational education, relevant work experience, age, gender, tenure, potential tenure, time since hire, and whether the job was originally defined as temporary. The analysis of the NFIB data also contained data, not available in EOPP, on marital status, race, Hispanic ethnicity, receipt of classroom training through the Job Training Partnership Act (JTPA), and receipt of formal employer-sponsored training.

The indicators of a successful match analyzed in the NFIB data are the surprise in productivity realizations at six months of tenure, expected productivity at the six-month milestone, relative profitability (initial, at six months, and at the time of the interview), productivity at the time of the interview, an index of innovative suggestions made by the worker, and an index of the worker's willingness to stay late to complete work.

I calculated the profitability differential between two employees by subtracting the wage and training cost differentials from the productivity differential (I scaled this measure so that the average productivity of workers with six months of tenure equaled 1). Appendix B describes this variable and the ratio scale. Because recruitment and turnover costs are not included in this profitability measure, I also examined turnover.

The innovative suggestions index comes from the following question: "Has _____ suggested any ways of improving sales or productivity?" There were four possible responses: no; yes, but not adopted; one or two ideas adopted; or three or more ideas adopted. Ranging from 1 to 4, the index has a mean of 2.01 and an across-firm standard deviation of 1.12. The "stays late" index is based on the following question: "If work is not completed, does this employee stay late to finish it?" The four possible responses were: no (coded 1); yes, if paid (coded 2); yes, exempt employee (coded 3); and yes, without pay (coded 4). This index has a mean of 2.29 and an across-firm standard deviation of 1.02.

In the analysis of EOPP data, the outcome variables predicted were productivity (during the first two weeks, the next eleven weeks, and at the time of interview or separation), training costs, starting and current wage rates, tenure, dummy variables for a quit and for involuntary turnover, and profitability at two points (during the first three months and at the time of the interview or separation).

Despite differences in sampling, selection processes, and definitions of the variables, the two data sets generate remarkably similar regression results. For example, productivity at the time of the interview increases with years of schooling and years of previous relevant work experience but not with total work experience (age) or relevant classroom occupational training from a public school or college.

REFERENCE CHECKS. Information on reference checks came only from the NFIB data. Employers were asked whether before hiring they had obtained a reference from: "(a) Immediate supervisor on previous job(s) (b) Personnel office at previous job(s). . .(i) Relative or personal friend of yours" The personnel office reference variable is 1 when (b) is selected and (a) is not; otherwise it is 0. Employers were also asked if they had "ever hired someone on this person's recommendation before? (1) Yes; (2) No; (3) Some of both." This variable was set equal to 1 for either a "yes" or a "some of both" response. Employers were also asked if before the hire, they had obtained information about the new

hire from performance tests, such as a typing test or job tryout, or from training certificates.

RECRUITMENT SOURCE. Employers in the NFIB data set were asked whether the new hire had heard about the job opening through walking in, an ad, a friend or relative, another employee, a referral, or some other source. Details on who made the referral came from the employers' response to the questions on reference checks.

How Different Are the Workers Recruited Through Different Channels?

Table 6 presents the mean characteristics of jobs and new hires for each recruitment source. There are few statistically significant differences among the recruitment sources. Firms that used informal networks had average turnover (0.244 in the first year), wage rates ($6.05 an hour), and propensities to hire blacks and Hispanics. These firms, however, tended to be smaller than other firms in the sample, less likely to use a performance test, less likely to seek a reference from a former supervisor, and more likely (41 percent versus the 23 percent average) to obtain a reference from a relative or personal friend. The firms that recruited walk-ins and respondents to newspaper ads had slightly higher than average turnover rates and slightly larger negative surprises. Walk-ins were less likely to be female, and ad respondents were more likely to be black or Hispanic. Firms that used public employment agencies were larger than average, were more capital-intensive, paid lower wages, and had higher turnover. The people hired through this channel were more likely to be black or Hispanic but not dramatically so. The firms that recruited through private employment agencies were even more capital-intensive, paid higher wages, and recruited better-educated workers who were more likely to have received vocational training at a private school. The workers who got their job through a school referral or placement office were younger, were more likely to have received vocational training at a public school and were paid less. Not surprisingly, employers who obtained referrals from a vocational teacher reported that occupational skills were a high priority. Those who obtained referrals from a principal or guidance counselor viewed occupational skills as less important.

The bottom three rows of table 6 give the number of person *A*s and

Table 6. Characteristics of Jobs and New Hires by Recruitment Source

Characteristics	*Standard deviation*	*Informal referral*	*Walk-in*	*Newspaper ad*	*Referred by*			
					Public agency	*Private agency*	*Vocational teacher*	*Other teacher*
Firm								
Employment[a]	46.7	23.1	27.2	28.6	46.2	33.0	27.7	43.4
Turnover first year	0.23	0.244	0.315	0.282	0.272	0.239	0.191	0.257
Capital per worker[b]	—	$18,600	$17,200	$17,300	$25,300	$30,000	$21,000	$15,520
Share general skill	0.47	0.673	0.655	0.681	0.638	0.690	0.719	0.701
Number of labor market competitors[b]	—	18.4	16.3	23.1	16.0	18.9	15.3	31.8
Starting wage	$4.25	$6.05	$6.08	$6.22	$5.18	$7.38	$5.28	$5.45
Work habits[c]	1.09	2.17	2.19	2.27	2.17	2.24	2.46	2.45
Job skills[c]	1.76	2.79	2.58	2.73	3.00	3.19	2.53	3.30
Reading, math[c]	1.46	3.99	4.11	4.02	3.91	3.71	3.89	3.82
New Hire								
Years of school	1.75	12.59	12.09	12.75	12.31	13.38	12.72	12.98
Female	0.49	0.436	0.304	0.495	0.416	0.628	0.391	0.519
Black	0.18	0.030	0.032	0.043	0.065	0.040	0.010	0.000

Hispanic	0.18	0.049	0.054	0.086	0.052	0.040	0.041	0.057
Age	28.6	28.1	29.2	28.7	29.4	28.5	22.2	21.5
Public vocational school	0.41	0.265	0.191	0.226	0.208	0.300	0.531	0.400
Private vocational school	0.34	0.094	0.120	0.118	0.130	0.280	0.082	0.109
Reference checks with								
Previous supervisor	0.49	0.33	0.34	0.55	0.30	0.57	0.44	0.47
Relative, friend of boss	0.46	0.41	0.12	0.07	0.10	0.11	0.20	0.20
Performance test	0.42	0.18	0.21	0.27	0.39	0.36	0.32	0.30
Surprise[d]	0.29	−0.149	−0.164	−0.196	−0.135	−0.143	−0.164	−0.228
Number of person As		728	255	335	77	53	100	60
Number of person Bs		567	254	306	74	45	98	55
Number of pairs where one but not both are from this source		—	293	279	60	42	94	54

Source: NFIB survey. Employers were asked "How did this individual hear of your job opening? (1) Walk in; (2) Advertisement; (3) Friend/Relative; (4) Other employee; (5) Referral, e.g. school, employment agency, etc.; (6) Other."

a. Full-time equivalents

b. Geometric mean

c. Ranked on a scale of 1 to 6

d. Mean actual productivity was 64.6.

person *B*s recruited through each source and the number of firms where one but not both the new hires came through this source. About half the companies recruited both new hires through the same source. That is a much higher proportion than would occur by chance. There were, however, many cases where the two individuals were obtained from different sources, which means the difference model (equation 3) can be used to estimate the effect of recruitment source on the success of the match, while the job and/or firm is held constant. Unfortunately, the most interesting recruitment sources from a policy viewpoint—public employment agencies, private agencies, and schools—have low incidence rates. That likely reduces the power of hypothesis tests that compare these sources to the other sources. Having two data sets is thus an advantage.

Multivariate Analysis Results

The regression results for equation 3 are reported in tables 7 and 8. They confirm the hypotheses about reference checks. Twenty-eight of thirty-two coefficients had the predicted sign, and the four reference check variables as a group had significant effects on five of the eight variables: the surprise in productivity realizations, profitability at six months, current productivity, the innovative suggestions index, and the "stays late" index.

The extra time it takes to check a reference from a previous supervisor clearly pays off. New hires for whom such reference checks were made were 4.7 percent more productive than expected at six months, 7.0 percent more productive at the time of the interview, significantly more likely to make suggestions that improve sales or productivity, and significantly more willing to stay late to finish work.

Workers with references from personnel offices were generally less productive. New hires with a reference from the personnel office at a previous job but not from a previous supervisor were 10.8 percent less productive than expected, 11.9 percent less productive at the time of the interview, and less profitable by 17 to 19 percent of average productivity.

Workers recommended by relatives and friends were significantly more likely than average to make innovative suggestions and to stay late to complete work. They also received significantly more training

on the job, were significantly less likely to quit the job, and were viewed as having better-than-average people skills and work habits (not shown). There is also some support for the hypothesis that references from people who have recommended previous successful hires are more accurate. New hires referred by such people were more profitable (7.7 percent of average productivity) both at six months and at the time of the interview.

These results clearly suggest that recommendations from reliable sources can significantly improve hiring decisions and that employers should devote more time to obtaining reliable recommendations from previous supervisors. Here, the problem of lawsuits currently limits what is possible. One solution might be to ask job applicants to sign a release that protects past employers from suit in the event they give a negative recommendation.

PERFORMANCE TESTS. About one quarter of the firms that use a performance test used it to screen one of its new hires but not the other. In those cases, the employee who took the test was not significantly more productive than the new hire who did not take the test. The employer expected the tested new hires to be 6.6 percent more productive after six months, but that expectation was not fulfilled. As a result, tested new hires generated significantly larger negative surprises than other employees. There is some evidence, however, that because the firm paid them a lower wage, the match generated larger quasi rents for the employer. The performance test coefficient in two of the profitability models is significant at the 10 percent level on a one-tail test, which suggests that the profitability advantage is a substantively important 11 to 13 percent of mean productivity at six months.

TRAINING CERTIFICATES. When controls are included for relevant work experience and the receipt of formal training from a previous employer, the possession of a training certificate had no effect on the surprise in productivity realizations, profitability, productivity, or willingness to work overtime.[31] Workers with training certificates were more likely to make innovative suggestions, but the effect is barely significant statistically.

31. If controls for relevant work experience and formal training are dropped from the model, the training certificate has large positive effects on initial productivity, innovative suggestions, and wage rates and negative effects on time spent in on-the-job training.

Table 7. Predictors of the Productivity Surprise, Worker Productivity, and Profitability of the Match

	Productivity at six months		*Profitability of match*					
Variable	*Surprise*	*Expect*	*Initial*	*Six months*	*Recent*	*Recent productivity*	*Number of ideas*	*Voluntary overtime*
Reference checks								
Previous supervisor	0.047*	0.010	0.029	0.033	0.031	0.070**	0.394***	0.198**
	(0.028)	(0.014)	(0.045)	(0.040)	(0.053)	(0.035)	(0.095)	(0.082)
Previously hired on this person's recommendation	0.038†	0.016	−0.015	0.077**	0.077†	0.032	0.054	−0.082
	(0.028)	(0.014)	(0.043)	(0.038)	(0.052)	(0.035)	(0.091)	(0.080)
Recommended by personnel office at previous job	−0.108*	−0.008	−0.008	−0.194**	−0.170†	−0.119*	−0.268†	−0.187
	(0.057)	(0.028)	(0.095)	(0.082)	(0.108)	(0.069)	(0.185)	(0.165)
Recommended by friend	0.007	0.005	−0.044	−0.019	−0.044	0.030	0.168*	0.205**
	(0.029)	(0.014)	(0.046)	(0.039)	(0.052)	(0.035)	(0.095)	(0.084)
Performance test	−0.088†	0.066**	0.128†	0.110†	0.109	0.031	0.069	0.094
	(0.055)	(0.028)	(0.087)	(0.074)	(0.102)	(0.064)	(0.192)	(0.166)
Training certificate	−0.007	0.026	0.070	0.004	0.014	−0.063	0.291†	−0.079
	(0.054)	(0.026)	(0.085)	(0.074)	(0.106)	(0.067)	(0.184)	(0.161)
Recruitment source								
Walk-in	−0.025	0.025**	0.007	−0.001	−0.009	−0.001	−0.031	−0.022
	(0.026)	(0.013)	(0.039)	(0.035)	(0.048)	(0.031)	(0.085)	(0.075)

Newspaper ad	−0.005	−0.014	−0.089**	−0.027	0.008	0.005	−0.175*	−0.045
	(0.027)	(0.013)	(0.042)	(0.036)	(0.051)	(0.033)	(0.091)	(0.079)
Referred by								
Public agency	−0.008	−0.022	−0.168**	−0.165**	−0.067	0.032	−0.074	−0.270*
	(0.055)	(0.027)	(0.083)	(0.071)	(0.101)	(0.066)	(0.178)	(0.163)
Private agency	−0.042	−0.019	0.040	0.007	0.019	−0.052	−0.081	0.034
	(0.067)	(0.033)	(0.102)	(0.089)	(0.121)	(0.088)	(0.225)	(0.191)
Vocational teacher	−0.024	−0.003	−0.031	−0.018	0.154*	0.047	−0.119	0.005
	(0.043)	(0.021)	(0.069)	(0.061)	(0.081)	(0.055)	(0.157)	(0.134)
Other teacher	−0.032	0.007	−0.056	−0.034	−0.079	−0.024	0.328*	0.174
	(0.052)	(0.026)	(0.085)	(0.071)	(0.097)	(0.069)	(0.190)	(0.162)
Background								
Years of school	0.012	0.004	−0.015	−0.006	0.004	0.031***	0.088***	−0.002
	(0.008)	(0.004)	(0.012)	(0.011)	(0.014)	(0.010)	(0.026)	(0.022)
Years of relevant classroom job training	0.009	0.002	0.014	−0.0008	−0.006	−0.005	0.059†	0.068**
	(0.011)	(0.006)	(0.017)	(0.015)	(0.019)	(0.014)	(0.037)	(0.032)
Relevant private occupational training	0.063*	0.040**	−0.009	0.033	−0.025	0.076*	0.008	0.109
	(0.038)	(0.018)	(0.059)	(0.053)	(0.073)	(0.044)	(0.122)	(0.108)
Relevant JTPA job training	0.060	0.066†	−0.114	0.156†	0.052	0.131	0.018	0.201
	(0.087)	(0.043)	(0.146)	(0.120)	(0.170)	(0.106)	(0.287)	(0.250)

Years of relevant work experience	0.011** (0.005)	0.011*** (0.003)	0.032*** (0.008)	0.009 (0.008)	−0.010 (0.012)	0.015** (0.006)	0.060*** (0.017)	0.025† (0.015)
Years of relevant work experience squared	−0.0002 (0.0002)	−0.0002** (0.0001)	−0.0008** (0.0003)	−0.0003 (0.0003)	0.0001 (0.0005)	−0.0002 (0.0002)	−0.0017*** (0.0006)	−0.0002 (0.0006)
Relevant formal on-job training	−0.055** (0.027)	0.036*** (0.013)	0.073* (0.042)	−0.051 (0.036)	0.010 (0.050)	0.005 (0.033)	0.078 (0.089)	−0.042 (0.078)
Relevant formal off-job training	0.019 (0.053)	0.025 (0.026)	0.032 (0.083)	0.101† (0.073)	0.144† (0.098)	0.107* (0.063)	0.330* (0.179)	0.067 (0.159)
Age 18	−0.0067* (0.0037)	0.0003 (0.0017)	−0.0125** (0.0053)	−0.0171*** (0.0049)	−0.0090† (0.0068)	−0.0020 (0.0044)	−0.0001 (0.0114)	0.0119 (0.0100)
(Age 18) squared	0.0001 (0.0001)	0.0000 (0.00005)	0.0003* (0.0001)	0.0003*** (0.0001)	0.0001 (0.0002)	−0.0001 (0.0001)	−0.0001 (0.0003)	−0.0004* (0.0003)
Female	0.003 (0.037)	−0.010 (0.018)	0.086 (0.056)	0.059 (0.053)	0.067 (0.063)	−0.036 (0.045)	−0.124 (0.123)	−0.139 (0.108)
Black	−0.020 (0.051)	−0.044* (0.025)	0.040 (0.078)	−0.024 (0.067)	−0.046 (0.084)	−0.056 (0.061)	−0.072 (0.166)	0.097 (0.152)
Hispanic	0.018 (0.055)	−0.038 (0.027)	0.056 (0.085)	0.018 (0.076)	0.045 (0.106)	−0.061 (0.072)	−0.163 (0.190)	0.042 (0.165)
Married	0.044 (0.027)	0.008 (0.013)	−0.043 (0.042)	0.060 (0.037)	0.113** (0.052)	0.054 (0.033)	0.213** (0.092)	0.114 (0.080)

Husband	−0.049	−0.005	−0.044	−0.162***	−0.141**	−0.082*	−0.216*	−0.105
	(0.035)	(0.017)	(0.054)	(0.047)	(0.067)	(0.043)	(0.117)	(0.102)
Temporary job	−0.004	−0.010	0.078	0.036	0.074	0.043	−0.117	0.145
	(0.035)	(0.017)	(0.053)	(0.048)	(0.065)	(0.041)	(0.112)	(0.100)
Potential/actual tenure	. . .	. . .	0.032	0.034	0.102**	0.202***	0.575***	. . .
			(0.033)	(0.029)	(0.041)	(0.025)	(0.067)	
Potential/actual tenure squared	. . .	. . .	0.0004	−0.0021	−0.0132**	−0.0216***	−0.059***	. . .
			(0.0041)	(0.0036)	(0.0053)	(0.0034)	(0.009)	
Adjusted R^2	0.0119	0.0927	0.0435	0.0592	0.0425	0.1199	0.1616	0.0212
F for reference check variables	1.94*	1.57	0.82	2.39**	1.31	1.92*	4.74***	2.47**
F for recruit source variables	0.34	1.21	1.61	1.03	0.74	0.27	1.27	0.71
F for background variables	1.45	5.92***	3.05***	3.50***	1.70*	2.31***	3.90***	1.68*
Root mean square error	0.323	0.167	0.520	0.426	0.439	0.375	1.131	1.047
Number of observations	829	909	869	764	449	756	905	1,026

Estimates of equation 3 predicting differences between person *A* and person *B*. Models predicting the innovative suggestions index and profitability and productivity at the time the questionnaire was completed included controls for actual tenure and its square. The other profitability models used potential tenure and its square to adjust for wage inflation. Productivity and profitability are measured relative to the mean realized productivity level of workers who have been at the firm for six months. Standard errors are in parentheses.

*implies Prob. LT .10 on a two-tail test.

**implies Prob. LT .05 on a two-tail test.

***implies Prob. LT .01 on a two-tail test.

†implies Prob. LT .10 on a one-tail test.

Table 8. Effect of Recruitment Source on Match Success

Outcome	*Walk-in*	*Newspaper*	*Referred by*					R^2	*Number of observations*
			Public agency	*Private agency*	*School*	*Union*	*Other*		
Productivity									
First 2 weeks	−0.02	−0.09*	−0.12*	−0.05	−0.10	0.50**	−0.03	0.227	493
	(0.4)	(1.7)	(1.8)	(0.6)	(1.6)	(2.2)	(0.4)		
Next 3–12 weeks	−0.02	−0.06	−0.08*	−0.13*	−0.04	0.13	−0.03	0.172	493
	(0.6)	(1.5)	(1.7)	(1.9)	(0.8)	(0.8)	(0.5)		
At interview or separation	0.00	−0.09*	−0.12**	0.01	0.00	0.19	−0.07	0.180	524
	(0.1)	(2.0)	(2.2)	(0.1)	(0.1)	(1.1)	(1.3)		
Other outcomes									
Training cost	0.02	0.03	0.06	0.08	0.05	−0.31	0.12	0.226	493
	(0.3)	(0.3)	(0.7)	(0.7)	(0.6)	(1.3)	(1.3)		
Log starting wage	0.01	−0.01	0.00	−0.03	−0.04	0.44***	−0.02	0.337	453
	(0.5)	(0.3)	(0.0)	(0.6)	(1.4)	(4.8)	(0.6)		
Log current wage	0.01	−0.04	−0.04	−0.02	−0.01	0.14	−0.03	0.240	524
	(0.5)	(1.3)	(0.9)	(0.3)	(0.3)	(1.1)	(0.7)		

Log tenure	−0.07	−0.24***	−0.32***	−0.17	−0.06	−0.29	0.02	0.657	510
	(1.0)	(2.8)	(3.0)	(1.0)	(0.1)	(0.6)	(0.2)		
Involuntary	0.074*	0.130***	0.260***	0.009	−0.030	0.168	0.010	0.073	510
turnover (0–1)	(1.8)	(2.2)	(3.4)	(0.1)	(0.4)	(0.5)	(0.1)		
Quit (0–1)	0.007	0.045	−0.090	0.130	0.047	−0.120	−0.074	0.064	510
	(0.1)	(0.6)	(1.0)	(1.1)	(0.6)	(0.8)	(0.8)		
Profitability									
First 3 months	−0.03	−0.10	−0.14**	−0.32**	−0.08	−0.67*	0.02	0.147	453
	(0.4)	(1.1)	(2.0)	(1.9)	(0.7)	(1.7)	(0.2)		
At interview	−0.01	−0.05	−0.12**	0.04	0.00	0.03	−0.05	0.066	524
or separation	(0.3)	(1.0)	(2.0)	(0.5)	(0.0)	(0.1)	(0.8)		
At interview	0.02	−0.03	0.06	0.02	0.06	−0.04	−0.06	0.101	229
(for stayers)	(0.3)	(0.6)	(1.0)	(0.2)	(1.3)	(0.2)	(1.0)		

Source: Estimations of the equation 3 difference model in EOPP data. The "t" tests, given in parentheses, are for a difference from the excluded category of friends, relatives, and employer referrals. The models all included the following control variables not shown: years of schooling, relevant vocational education dummy, private vocational education dummy, years of relevant work experience and its square, potential experience (Mincer definition) and its square, gender, whether the employee was a student when hired, whether the job was federally subsidized, whether individual was known when hired to qualify the firm for a job training tax credit, average hours worked per week, and whether the job was viewed as temporary. Models predicting outcomes at interview or separation contained controls for tenure and tenure squared. To deal with the tendency of nominal wages to rise over time, models predicting starting wage and profitability during the first three months contained controls for time since hire and its square. Models predicting tenure and turnover contained the logarithm of potential tenure and its square and excluded observations where one of the new hires had been hired for a temporary job.

RECRUITMENT SOURCE. With few exceptions recruitment source did not have significant effects on the success of a match. Informal recruitment channels that might provide screening services—friends, relatives, current employees, other employers (in EOPP) and "other" (in NFIB data)—were the excluded category, so negative coefficients were expected on most of the recruitment source dummies. Negative signs did predominate, but magnitudes were generally small, and consequently most coefficients were insignificant.[32] The exceptions to this generalization, however, are interesting and important.

The effect of referrals by unions could be examined in the EOPP data. Not surprisingly, such referrals had large positive effects on initial productivity and starting wage rates and a negative effect on initial profitability.

In the NFIB data, workers recruited through advertisements were significantly less profitable initially and less likely to suggest ways to improve sales or productivity. In EOPP data they were significantly less productive both initially and at the time of the interview and more likely to leave involuntarily.

Schools in Japan have long-standing referral relationships with local employers. James Rosenbaum has suggested that if such relationships were more prevalent in the United States, non-college-bound students would probably take their studies more seriously.[33] How well do such relationships serve American employers? Not too badly. In EOPP data, school referrals were not significantly different from the informal recruitment sources on any measure of outcome. In the NFIB survey,

32. Models were also estimated in which recruitment through friends, relatives, current employees, and "other" was compared to recruitment through labor market intermediaries (other than public agencies, which were represented by their own separate dummy variable), newspaper ads, and walk-ins. Those recruited through the first group did not do significantly better than the second group when the reference check variables were included in the model. Consistent with previous research, the probability of turnover in the first year was five percentage points higher for the second group than for the first group, but this difference was only significant at the 0.11 level on a two-tail test. The reference check variables are a direct measure of one aspect of the information exchange that tends to occur more frequently when workers are recruited through informal channels. Their inclusion in the model is one reason for the small effect of recruitment channels. Excluding these reference check variables does not, however, make the recruitment channel variables significant as a group.

33. Rosenbaum (1990).

information was obtained on the specific school official who made the referral—a high school vocational teacher or major professor in college or other teachers, principal, or school placement office. This distinction was suggested by McKinney and others' finding that placement rates for vocational students were significantly higher when the teacher, not a school counselor or placement office, handled the placement.[34] New hires recommended by a vocational teacher or major professor were significantly more profitable (by 15 percent of average productivity at six months) at the time of the interview or separation. The productivity and profitability of referrals made by other teachers were not significantly different from those recruited through informal channels with one exception—a significant positive coefficient on the innovative suggestions index.

The most important finding about recruitment sources, however, is the poor performance of new hires referred by public employment agencies. Holding the job, firm, and several background variables (including training by JTPA) constant, referrals from public agencies (the ES, vocational rehabilitation agencies, JTPA, and community-based organizations) were, in NFIB data, significantly less willing to stay late and less profitable (by about 16 percent of average productivity) both initially and after six months. In EOPP data they were significantly less productive than the average new hire (8 to 12 percent) in all three time periods, significantly less profitable (by 12 to 14 percent), and significantly (twenty-six percentage points) more likely to be dismissed and less likely to have long tenure. When separate dummy variables for ES referral and referral by other government agencies replaced the single public employment agency dummy, both types of government referrals had similar coefficients.

One would expect the least productive workers to quit or to be dismissed. This attrition may attenuate the correlation between recruitment source and worker productivity. The bottom row of table 8 calculates the effects of recruitment sources for paired new hires when both were still at the firm at the time of the interview. These comparisons show no connection between recruitment source and profitability. Although public agency referrals were significantly less profitable in some

34. McKinney and others (1982).

of the comparisons between retained and separated employees, those low productivity workers may have been dismissed by the time of the interview.

These results suggest that public labor market intermediaries do not serve employers very well. Do private employment agencies do a better job? Employers pay substantial fees to hire private referral agencies. Rees reported, for example, that in the Chicago labor market, fees were 60–72 percent of a month's salary.[35] Do private referrals warrant such fees? The regressions show that their referrals were not significantly better than new hires recruited through other channels. In fact, in EOPP data, referrals from private employment agencies were 13 percent less productive during the third through thirteenth week and 32 percent less profitable during the first three months. *Apparently, the problems of the ES cannot be solved by contracting out the function to private employment agencies.*

If the quality of the matches that result was poor, why do employers request any referrals from the public agencies. Why do they pay for the services of private agencies? Is it because they save on the direct costs of screening and interviewing job applicants? To answer those questions, I examine how the direct costs of recruiting, screening, and selecting a new hire vary across recruitment channels.

The Direct Cost of Hiring Through Alternative Channels

Hiring decisions impose two kinds of costs on the firm. If the position remains unfilled while the search is under way, output and profits may be forgone until the opening is filled. For the unskilled and semi-skilled jobs in the EOPP data set, most firms either received sufficient advance notice of forthcoming vacancies or filled the openings quickly, so the cost of delay does not appear to have been large. Holzer found that the vacancy was about four days shorter when the new hire was recruited through current employees, newspapers, or walk-ins rather than through friends and relatives of the employer.[36]

The second type of cost—the direct costs—are the opportunity cost of the time that the firm's owner and employees devote to recruiting,

35. Rees (1966).
36. Holzer (1987); and Baron and Bishop (1985).

Table 9. Hiring Cost by Recruitment Source

Recruitment source	*Hours recruiting, screening, and interviewing*	t *statistic for difference from walk-in*
Union	3.8	2.86
Relatives of current employee or owner	6.1	4.33
Friends of current employee or owner	8.3	1.24
Walk-in	9.0	—
School	11.2	1.67
Employer	12.0	2.59
Employment Service	14.8	3.71
Private employment agency	15.6	3.56
Other government agency	17.9	3.71
Newspaper ad	21.9	10.35

Source: EOPP survey.

screening, and interviewing job candidates. Public and private employment agencies offer to take over much of recruitment and screening function, so firms using these recruitment sources might expect to have lower direct hiring costs. This, however, does not appear to be the case (table 9). Data from the EOPP survey imply just the opposite. The decision process for new hires when the referral came from the ES took 14.8 hours, 17.9 hours when the referral was from some other government agency, and 15.6 hours when the referral came from a private employment agency. Decisionmaking took less time when informal channels were used; 6.1 hours, for example, when relatives of the owner or a current employee were hired. Decisions to hire a walk-in required only 9 hours.

It would appear that employers are unaware of how poorly they fare with public and private employment agency referrals, are forced into using them by an absence of other applicants for the job, or hire referrals from public agencies for altruistic reasons, such as giving a disadvantaged person a chance.

The Dilemmas of the Employment Service

The ES has multiple constituencies—different types of job applicants, employers, and the voting public—each with a different set of interests the agency must address. The ES has always had more clients

seeking jobs than employers seeking referrals. During the 1950s and early 1960s, the ES determined that it could best serve job seekers as a whole by taking employer preferences as given and attempting to fill job orders with the "best" possible applicants. According to David Stevens, who recently wrote a history of the agency, the standard practice during this period was "for a personnel representative in a firm to call a local office staff member with whom [the firm] had worked in the past, place a job order, and be confident that the local office staff person would only refer individuals in accordance with the employer's hiring requirements."[37] ES referrals during this period accounted for nearly 20 percent of the nation's new hires.

Changing Mission

Priorities shifted in the mid-1960s, and the ES "became one of the nation's public advocacy weapons for affirmative action on behalf of targeted populations."[38] According to Stevens,

> In the mid-1960s local office procedures were modified in several ways. First, discriminatory referral procedures, which had always been frowned upon were now more actively discouraged. And second, individual staff member control of job orders began to decline, which meant that the one-on-one relationships between employer representatives and State Employment Security Agency staff members were weakened. Both of these challenged the ability of the local office to offer a continuing guarantee of screening reliability. Many observers attribute the growth of private employment agencies coincident with the stagnation of the public employment service system to this social responsibility of the public agencies.[39]

Funding formulas were structured to emphasize placement of targeted groups rather than total placements. In 1978, for example, nearly 40 percent of the ES budget came from contracts with federal and state agencies serving welfare recipients and the disadvantaged.[40] Many of these contracts were performance-based, that is, the agency received a set fee for each member of the target group it placed. ES referrals fell

37. Stevens (1988, p. 30).
38. Stevens, personal communication, 1991.
39. Stevens (1988, p. 30).
40. U.S. Department of Labor (1979, p. 62).

to about 8 percent of new hires in 1971 and remained low throughout the 1970s and 1980s.[41] The public employment services of other nations generally have higher market shares. In the United Kingdom, the share is 30 percent; in Spain it is about 20 percent.[42]

The result is that in 1987–88 the ES placed only 17.5 percent of the 18,439,000 people who requested its help in finding work.[43] In addition, the quality of the positions for which it obtained job orders deteriorated.

By 1983 the reputation of the ES had deteriorated so much that most employers did not seek referrals from the agency even for tax credits worth nearly 50 percent of the wage.[44] The high priority the ES placed on serving one particular constituency appears to have cost the agency the support of its other constituencies: employers, voters, and nondisadvantaged workers. Because employers control job openings, the scarcest element of the job-matching process, employer disillusionment was particularly damaging. Without support from its traditional constituencies, the ES was unable to fend off substantial budget cuts during the 1990–91 recession, and it even began to lose its ability to serve the disadvantaged.

Effects on Job Seekers

How effectively does the ES serve job seekers? The share of job seekers using the ES fell from 30.2 percent in 1970 to 21.1 percent in

41. In 1987 the rate was about 7 percent. See Cohen and Stevens (1989).

42. OECD (1992, p. 128). Spain and the United Kingdom have semi-open self-service systems in which job openings are listed on bulletin boards in local offices of the public employment agency. Job seekers may review these listings, but addresses and phone numbers must be obtained through the employment agency. The act of obtaining the numbers turns the job applicant into a registrant, so counts of placements by the employment agency are comprehensive. In the fully open systems of Norway and Japan, employer names and addresses are listed along with the job openings, and walk-in job seekers can apply directly for these jobs. Administrative data on placements in these countries thus understate the number of job matches that have been assisted by the public employment agency, and estimates of market share consequently tend to be low—12 percent in Japan and 10 percent in Norway. In Spain the public employment agency's market share is high, in part because it has a legal monopoly on labor exchange services—all openings must be listed with the agency (although this requirement is not enforced)—and because all placements in subsidized jobs must go through the agency.

43. Cohen and Stevens (1989).

44. EOPP Employment Survey (1982).

1988.[45] Its share of job placements is substantially smaller. Only 5.5 percent of those employed in January 1973 reported they had found their current job through the ES.[46] The ES share of new hire activity was 3.6 percent in EOPP data and 2.8 percent in NFIB data. In 1984–85 the median wage of job seekers placed through the ES was only slightly above the minimum wage and about half the average wage of nonsupervisory workers.[47]

The low quality of the jobs for which the ES provides referrals is sometimes attributed to its mandate to give priority to economically disadvantaged and handicapped job applicants. In 1987–88, 19.7 percent of ES applicants were economically disadvantaged. The ES accounts for a considerably larger share of the jobs found by members of minority groups, and these groups were 32 percent of ES registrants.[48]

Do job seekers referred by the ES get better jobs than they would otherwise have gotten? Johnson, Dickinson, and West studied this question for the Department of Labor in the early 1980s. Baseline interviews were collected from a sample of 8,000 new applicants during 1980–81 in thirty ES offices spread across twenty-seven states, with follow-up interviews coming six to nine months later. The study estimated OLS models predicting wage rates, earnings, and employment in the six-month period following application to the ES, while controlling for earnings during the previous two years, occupation, a long list of background characteristics, and several indicators of the applicant's motivation to find work.

Women who received an ES referral found their first job a significant 2.8 weeks earlier than ES applicants who did not receive a referral and earned 23 percent more during the six-month period following their application to the ES. Remarkably, men who received an ES referral had no higher probability of employment than men who did not receive a referral. Males who received a referral obtained a first job about a half a week sooner, but earned 3.7 percent less during the six-month period and were 1.8 percent less likely to be employed at the end of the period. Receiving an ES referral also failed to improve the quality

45. Cohen and Stevens (1989, pp. 1027–70).
46. Rosenfeld (1975, pp. 39–43).
47. Cohen and Stevens (1989, pp. 1027–40).
48. Cohen and Stevens (1989).

of the job obtained. In fact, the estimates imply that new registrants who received referrals had slightly lower average wage rates—a 2 percent reduction for men and a 4 percent reduction for women.

Johnson, Dickinson, and West also reported that the regressions showed that "few applicant demographic characteristics and no work history characteristics significantly influence whether an individual receives a referral."[49] This is a surprising result, because employers prefer to hire experienced workers, so one would expect those workers to be the first to be referred by ES. If there is indeed no such relationship, employer complaints that the ES does not refer the most qualified worker possible may well have some basis in fact.

Whether one accepts the study's findings on referrals, it clearly offers no evidence that an ES referral helps a worker find a higher-paying job. Furthermore, the presumed finding that being referred to a job raises the probability of employment for women is considerably less impressive when looked at from a general equilibrium perspective. First, it is quite possible that the comparison group (the 60-plus percent of ES applicants who did not get referred) were made worse off by the process of applying and then being rejected. Applicants who are told they will be called if they qualify for any of the open jobs and who then never receive a call may despair of ever finding a job and end their own self-directed search efforts. Second, referred ES applicants may be displacing other job seekers. What would the employers who hired the ES referrals have done if there had been no ES? Surely, many (probably most) of these openings would have been filled some other way.

Policy Implications

The Department of Labor is seeking to reestablish the Employment Service's reputation for making high-quality referrals. The agency's

49. Johnson, Dickinson, and West (1985, p. 123). Several tests for selection effects were conducted. Regressions predicting the receipt of a referral found that "many of the measures of an applicant's motivation to find work are significantly related to the probability of receiving a referral. . . ." However, regressions predicting earnings during the six-month period prior to applying at the Employment Service found that men who received referrals had nonsignificantly lower earnings than men who did not receive referrals and that women with ES referrals had significantly lower earnings that women without ES referrals. The reader is urged to consult the article before deciding whether the authors' judgment is justified.

ability to respond is constrained, however, by its limited budget.[50] Job brokering is only one of the ES's responsibilities, and at current staffing levels, the agency cannot offer all employers the individualized brokering services that private employment agencies offer. The ES appears to see its niche as a highly automated, high-volume referral system that exploits the substantial economies of scale inherent in the labor exchange function.

The ES has been experimenting with various ways of improving the services it provides to employers and job seekers. A system of coding and matching occupational skills is under development. A new referral system bases most referrals on the General Aptitude Test Battery and, where it has been implemented, it has been popular with employers. Another change that should be considered is a shift to the European practice of listing all openings in convenient public places and on computer bulletin boards and to let job seekers contact employers directly. This approach would keep ES involvement to a minimum. Access to the system could be provided anywhere a computer terminal with a modem can be located—libraries, schools, shopping centers, community organizations, even private homes. Job seekers would describe their qualifications, the kinds of jobs they are seeking, and the geographic area within which they are looking.

The system would provide job seekers with a list of openings (along with a short description of the job) for which they appear qualified. Longer job descriptions and information on how to apply could be obtained simply by pressing a button. All public agencies and private employers with government contracts could be required to list most of their openings in the system. This alternative is attractive because it relieves the ES of the politically contentious burden of deciding who should have priority in applying for attractive jobs. Analysis and evaluation of these and other proposals for improving the agency's ability to match job seekers and employers is, however, beyond the scope of this paper.

50. Investments in public employment agencies are typically higher in Europe than in the United States. The ratio of employment service staff to population is 1 to 1,100 in the United Kingdom, 1 to 1,500 in Sweden, 1 to 1,250 in Germany, and 1 to 8,600 in the United States. See Carlson, Konig, and Reid (1986, pp. 36–37); and OECD (1992, p. 123).

Appendix A: The EOPP Employer Survey

The EOPP data comes from a survey of 3,412 employers sponsored by the National Institute of Education and the National Center for Research in Vocational Education (NCRVE) conducted between February and June 1982. The survey was the second wave of a two-wave longitudinal survey of employers from selected geographic areas across the country. The first wave was funded by the U.S. Department of Labor to collect data on area labor market effects of the Employment Opportunity Pilot Projects (EOPP). The survey encompassed ten EOPP pilot sites and eighteen comparison sites selected for their similarity to the pilot sites. The ES-202 lists of companies paying unemployment insurance taxes provided the sample frame for the survey. Because of the interest in low-wage labor markets, the sample design specified that establishments in industries with a relatively high proportion of low-wage workers be oversampled. The tax paying units were stratified by the estimated number of low-wage employees, and the number of establishments selected from each strata was roughly proportionate to the estimated number of low-wage workers at the establishments in that strata. The selection was random within strata. The telephone survey obtained a response rate of 75 percent.

About 70 percent of the original respondents completed surveys for the second wave. Seventy-two percent of the 3,412 respondents were single establishment firms, and the rest were parts of corporations with multiple establishments. Most of the respondents were the owners or managers of small establishments who were familiar with the performance of these employees. Seventy percent of the establishments had fewer than 50 employees, and only 12 percent had more than 200 employees. In large organizations the primary respondent was the person in charge of hiring, generally the personnel officer. If the primary respondent was unable to answer questions about the training received by newly hired workers in the sampled job, a supervisor or someone else with line responsibility completed that part of the interview.

The employers who received the full questionnaire were asked to select "the last new employee your company hired prior to August 1981 *regardless of whether* that person is still employed by your company." Only 2,594 employers had hired someone in the time frame requested,

and these employers constitute the sample used for estimating equation 2 models.

The sample of jobs for which paired data were available for estimating equation 3 models was generated in the following manner. The employers that provided information on one new hire were asked to provide data on a second new hire in the same job but with a different amount of vocational education. Of the 2,594 employers who provided data on one new hire, 1,511 had not hired anyone else in that job in the last two years, and 424 had not hired anyone with a different amount of vocational training for that position in the last two years. As a result, data are available for 659 pairs of individuals who have the same job at the same establishment. Missing data on specific questions used in the model further reduced the sample used for estimation to about 480.

The respondents were asked to report how much time typical new hires spend during the first three months of employment in four different kinds of training activities: (1) watching others do the job rather than doing it themselves, (2) formal training programs, (3) informal individualized training and extra supervision by management and line supervisors, and (4) informal individualized training and extra supervision by coworkers. For the sample of firms and jobs, the means for the typical worker were 47.3 hours watching others do the job, and 10.7 hours for formal training programs, 51 hours for informal training by management, and 24.2 hours for informal training by coworkers. A copy of the relevant portions of the questionnaire is available from the author.

Training time indexes were constructed by placing relative values on trainer and trainee time and then combining the time invested in training activities during the first three months on the job. The management staff members who provide training were assumed to be paid 1.5 times the wage of co-workers with two years of tenure. Formal training involves both trainer and trainee time. Sometimes training is one-on-one, and sometimes it is done in groups. It was assumed that, on average, each trainer had four trainees and that the value of the trainer's time (including the amortized cost of developing the training package) was four times the wage of a co-worker with two years of tenure. The value of the time trainees spent in formal training was assumed to eight-tenths of an experienced co-worker's wage. When supervisors and coworkers informally train a new employee, the trainee

is almost invariably directly involved in a production activity. Employers report that trainees are typically as productive during informal training as they are when working alone.[51] Consequently, informal training is assumed to involve only the investment of the trainer's time.

The survey asked the employer (or in larger firms, the immediate supervisor) to report on productivity of both new hires during the first two weeks, during the next eleven weeks, and at the time of the interview (or just before leaving for those who leave the firm). The rating was made on a "scale of zero to 100 where 100 equals the maximum productivity rating any of your employees in (*NAME's*) position can obtain and zero is absolutely no productivity by your employee." The fact that the nonresponse rate for this question was only 4.4 percent (it was 8.2 percent for previous relevant experience, 6.7 percent for education, and 5.7 percent for questions about starting wage rate) suggests that respondents felt capable of making such judgments and augurs well for the quality of the data that results. For the sample of firms that provided data, the mean values of these productivity indexes were 49.2 for the first two weeks, 64.7 for the next eleven weeks, and 75.4 at the time of the interview.[52]

Appendix B: Construction of the Profitability Measures

The questions asked in these two surveys about the productivity of particular individuals do not yield measures of productivity that are comparable across firms or across jobs within the firm. They are assumed, however, to be ratio scale measures of the relative productivity of two particular workers who have the same job. Measurement errors are assumed to be uncorrelated with the true ratio scale. Because the productivity indexes are used as dependent variables, not independent variables, measurement error only lowers the significance of hypothesis tests; it does not result in biased coefficients. If these assumptions are wrong and the variations in the productivity scores assigned by supervisors exaggerate the proportionate variations in true productivity, the

51. Hollenbeck and Smith (1984).

52. A more thorough description of the EOPP-NCRVE data is provided in Bishop (1990).

estimates of percentage differences in productivity between two workers will be biased upward. Even though it is possible for a worker's true productivity to be negative, the scale was defined as having a lower limit of zero. Floors and ceilings on a scale typically cause measurement errors to be correlated negatively with the true value. Furthermore, respondents who were not well informed about the relative productivity of their employees would probably tend to describe them as similar in productivity and not to exaggerate the differences between them. If that is the case, then the estimates of percentage differences in productivity between two workers will be biased downward. This latter type of bias appears to be more likely than the former.

Further evidence that the ratio scale assumption results in an understatement of percentage differences in productivity between individual workers doing the same job comes from comparing the coefficients of variation of productivity in this and other data sets. If pairs of workers who are still at the firm are used to construct a coefficient of variation in the EOPP-NCRVE data set, it averages 0.13 for sales clerks, clerical, service, and blue-collar workers. This estimate is smaller than the estimates for yearly output derived from analysis of objective ratio scale measures of output. These yearly output estimates were 0.35 for sales clerk jobs, 0.144 in semi-skilled blue-collar jobs, 0.28 in craft jobs, 0.164 in routine clerical jobs, and 0.278 in clerical jobs with decision-making responsibilities.[53] This means that the estimates of the effect of background characteristics on relative productivity growth reported in this paper are probably conservative. The fact that the employer is reporting on the past productivity of particular employees may also generate biases in data, but it is not clear how this problem might influence the estimated models.

Differentials in the *ex post* profitability of the two new hires were estimated by combining the data on the differentials in their wage, productivity, and training costs. Because data are not available on costs of training beyond the first three months at the firm, the *ex post* profitability variable for the date of the interview or separation is based solely on a comparison of the productivity and wage-rate differentials between the two new hires. In EOPP data the formula for profitability differential at the time of the interview was:

53. Hunter, Schmidt, and Judiesch (1988).

$$(A1) \qquad Y^C_{Aj} - Y^C_{Bj} = [(P^C_{Aj} - P^C_{Bj})/P^T_j] - \ln(W^C_{Aj}/W^C_{Bj})$$

The formula for the differential in *ex post* profitability during the first three months is:

$$(A2) \qquad Y^S_{Aj} - Y^S_{Bj} = [(P^S_{Aj} - P^S_{Bj})/P^T_j] - [(T^S_{Aj} - T^S_{Bj})/520] - [(W^S_{Aj} - W^S_{Bj})/ W^T_j],$$

where

Y^S_{ij}, Y^C_{ij} = Profitability (excluding any tax credits) of the i^{th} new hire in job j during the first three months (S), during the first week ($1W$), at the end of six months ($6M$), and at the time of the interview or separation (C),

P^S_{ij}, P^C_{ij} = Productivity index for person i during the first three months (S), during the first week ($1W$), at the end of six months ($6M$), and at the time of the interview or separation (C),

W^S_{ij}, W^C_{ij} = Wage of person i at the start (S) and at the time of the interview or separation (C),

P^2_j, W^2_j = Productivity index and wage of the typical worker in job j with two years of tenure (2), and

T^S_{ij} = Opportunity costs during the first three months of training person i. The units of the training index are hours of time of a worker with two years of tenure in job j.

Note that by dividing by P^2_j, the productivity differential, $(P^S_{Aj} - P^S_{Bj})$, is translated into the metric of the productivity expected from a worker with two years of tenure in job j. This is also the metric of the training cost differential, so the two terms may be summed. The starting wage differential, $(W^S_{Aj} - W^S_{Bj})$, is divided by the wage of a typical worker with two years of tenure in the job. The profitability proxy is constructed under an assumption that $P^2_j = W^2_y$. This implies that the third term need not be multiplied by an adjustment factor before being subtracted from the terms describing productivity and training differentials.

In NFIB data the formulas for *ex post* profitability differentials for the first week ($1W$), the next six months ($6M$), and at the interview (C) were:

$$\text{(A3)} \quad Y_{Aj}^{1W} - Y_{B}^{1W} = [(P_{A}^{1W} - P_{B}^{1W})/P_{j}^{6M}] - [(T_{Aj}^{1W} - T_{B}^{1W})/40] - [W_{Aj}^{S}/W_{B}^{S}] + 1,$$

$$\text{(A4)} \quad Y_{Aj}^{6M} - Y_{Bj}^{6M} = [(P_{Aj}^{6M} - P_{Bj}^{6M})/P_{j}^{6M}] - [(T_{Aj}^{6M} - T_{Bj}^{6M})/960] - [W_{Aj}^{S}/W_{Bj}^{S}] + 1,$$

$$\text{(A5)} \quad Y_{Aj}^{C} - Y_{Bj}^{C} = \ln(P_{Aj}^{C}/P_{Bj}^{C})] - [(T_{Aj}^{6M} - T_{Bj}^{6M})/960] - \ln(W_{Aj}^{C}/W_{Bj}^{C})],$$

where

T_{ij}^{1W} = Hours spent by person i in training during the first week, and
T_{ij}^{6M} = Hours spent by person i in training during the next six months.

These NFIB formulas assume that $P_{j}^{6M} = W_{j}^{S} = W_{j}^{C}$. Because workers with formal, off-the-job training from a previous employer are not paid more than other workers, other assumptions regarding the relationship among P_{j}^{6M}, W_{j}^{S}, and W_{j}^{C} (such as $P_{j}^{6M} = 1.4\ W_{j}^{C}$) will not change the statistical significance of the tests of the hypothesis that coefficient B in equation 3 is greater than zero. The tests of the profitability of hiring workers with relevant experience are, however, sensitive to these assumptions.

Comments and Discussion

Comment by Katharine G. Abraham: This paper's stated objective is to shed light on how government can facilitate better matching between jobs and workers. Much of the paper is devoted to the analysis of employer recruiting practices. The reported findings suggest that U.S. employers do a surprisingly bad job of predicting the performance of those they hire. The results also contain some intriguing clues concerning the worker traits that employers find most difficult to predict at the time of hiring and reveal a systematic association between the use of certain recruiting methods and the incidence of unpleasant hiring surprises.

My comments on the paper are of two sorts. First, I have several questions about the data and the econometric models that underlie the results reported. Second, and more important, even accepting the paper's empirical findings at face value, I would stress the need for care in their interpretation. The existence of cross-sectional associations between recruiting method and hiring outcomes does not imply that individual employers can improve the productivity of their new hires by altering their recruiting strategies. Moreover, strategies that raise the productivity of any individual employer's new hires need not translate into aggregate productivity improvements. Indeed, I would argue that the paper has little to say about the potential for improved job matching per se to enhance the performance of the U.S. labor market.

Let me begin by discussing briefly the data on which the paper's analysis rests. These data are very arguably the best available for the purpose at hand, but they are not without problems. One general concern is that the hiring experiences captured by both the National Federation of Independent Business (NFIB) and the Employment Opportunity Pilot

Projects (EOPP) surveys may not be representative of hiring experiences generally. The NFIB sample apparently excludes publicly held corporations; the EOPP survey design deliberately oversampled industries employing large numbers of low-wage workers. Perhaps more important, even after three follow-up mailings, the NFIB survey generated a response rate of only 24 percent. A larger share of surveyed employers completed the EOPP questionnaire, but the effective response rate was still just over 50 percent. (The data analyzed come from a follow-up survey that had a 70 percent response rate, but that survey was sent only to the 75 percent of those in the original random sample who responded to the survey on the first round.)

I raise these points primarily because I suspect that at least certain findings reported in the paper—most especially the finding based on NFIB data that new hires perform less well on average than expected at the hiring date—would not carry over to a more representative sample. If employers formulate their expectations concerning workers' performance rationally, new hires should perform about as well on average as expected at the date of hire. Employers who have had particularly disappointing hiring experiences might, however, be more likely to respond to a survey about hiring than those with uneventful hiring histories. Nonresponse bias thus might well have produced the finding in the NFIB data of poorer-than-expected average performance among new hires.

The analysis reported in the paper makes use of several outcome variables, including measures of productivity, actual productivity, the surprise in productivity, and profitability, along with other measures intended to capture match longevity. The various productivity measures feature most prominently in the paper, so it is important to think carefully about their underpinnings. In both the NFIB and the EOPP data, the productivity measures are based on employers' answers to questions about workers' relative productivity on a 0-to-100 scale. These answers are inherently subjective to some degree. I am willing to believe that employers can make a sensible judgment about whether employee A is more or less productive than employee B. I am less confident that employers can make sensible judgments concerning the magnitudes of the difference between the productivities of the two employees, and I am still less confident that different employers will make comparable use of the 0-to-100 scale in describing these quantitative differences or

that a given difference in numeric score can be meaningfully compared across jobs. Any problems with the productivity measure also will affect the more complicated profitability measure used in certain models; that measure also requires several other assumptions that I will not critique here. Measuring the productivity of individual workers is admittedly a difficult task, but I would have liked the paper to include some discussion of the reliability and validity of the measures used.

One aside here is that it might have been of interest to model not only the determinants of positive or negative productivity surprises, but also the determinants of absolute deviations between actual and expected productivity. Theoretical discussions commonly suggest that certain recruiting methods may provide both firm and worker with better information about the other. The most direct implication is that these recruiting methods should be associated with a lower incidence of both large positive and large negative productivity surprises. This implication could be tested directly using the absolute value of the surprise in productivity as an outcome variable.

A further methodological concern is the paper's somewhat cavalier treatment of categorical and qualitative variables. The employment outcomes analyzed include whether the person was involuntarily separated from the job and whether the person quit the job. In models fit for a single individual, this sort of categorical outcome might properly be analyzed using a logit or a probit model. These models are not directly applicable to the analysis of the difference in turnover outcomes between two individuals, but the ordinary least squares regressions reported in the paper also are clearly inappropriate. Similarly, treating ordinal values of an explanatory variable as cardinal measures, as is done in table 4, is certainly not strictly kosher.

Rather than dwelling on data and modeling issues, however, I would like to turn to the question of how the reported findings ought to be interpreted. One important question is what the findings have to say about whether and how individual employers might be able to screen their job applicants more effectively. The paper makes much of the findings reported in tables 7 and 8. These suggest, among other things, that new hires who received recommendations from former supervisors turn out, on average, to be more productive than those who did not, whereas workers hired through the Employment Service turn out to be less productive. Bishop implicitly attaches a structural interpretation to

these findings, arguing, for example, that his findings establish that "the extra time it takes to check a reference from a previous supervisor clearly pays off."

In fact, however, it is entirely possible that these associations have more to say about the effects of differences in labor market conditions on hiring outcomes than about the effects of differences in recruiting strategies. In a tight labor market, even an employer who makes a practice of checking references carefully and avoiding Employment Service referrals might experience difficulty in identifying attractive job candidates through the usual channels. Rather than leaving a job vacant and extending the period of search, such an employer might hire someone for whom a recommendation from a previous supervisor would not be obtained or accept an Employment Service referral. It would not be surprising if those hires turned out to be less productive or less profitable than the average hire, but in the case I have described, this would not be mean that the employer's recruiting strategy had been suboptimal but that there had been a shortage of more attractive candidates.

The paper itself provides some indirect support for the interpretation I have just suggested. As shown in table 9, the management time devoted to recruiting and screening for jobs ultimately filled through referrals from the Employment Service and other government agencies substantially exceeds that for jobs ultimately filled through more personal referrals. This is exactly what one would expect if employers turned to government agencies for referrals only after trying unsuccessfully to hire through other channels.

A second important question is what, if anything, the paper's findings imply about the potential for better matching of workers to jobs to raise aggregate productivity. Table 4 establishes that workers who receive favorable assessments of their occupational skills, learning ability, and work habits after six months on the job also tend to perform better after six months than had been expected at the time they were hired. This, it is suggested, implies that better *ex ante* assessments of these characteristics would "generate significant improvements in matching workers to jobs."

For this conclusion to follow, however, workers must exhibit different profiles of strength and weakness, and certain characteristics must be more valued on some jobs than on others. In practice, I would argue,

the potential for this sort of gain is likely to be limited. One relevant factor is that those who are strong along one dimension of performance tend to be strong along other dimensions as well. The higher the correlations among workers' standings along the various relevant ability dimensions, the lower the potential for raising productivity by matching workers with particular strengths to particular sorts of jobs.

Similarly, if having a particular strength raises a worker's productivity by a comparable amount in whatever job he or she accepts, there is little potential for improved matching to raise aggregate productivity. The estimates reported in table 4 do not directly address the question whether jobs differ in the value of having workers with particular abilities but establish only that new hires identified *ex post* as having particular strengths also have better-than-expected performance six months into their jobs. Table 4 reports the coefficients from both expected productivity models and comparably specified surprise-in-productivity models fit for each of four different occupational groups. The sums of the coefficients from these two models should equal the coefficients that would be obtained if an actual productivity regression were estimated using the same data. If it can be assumed that employers hiring for different kinds of jobs assess workers' abilities in a conformable fashion, the similarity of these sums across the four occupation-specific regressions can be taken as supportive of the view that those traits affect productivity in all four job groups in roughly the same way.

Putting things somewhat more strongly than is probably warranted, then, a reasonable summary of the findings reported might be that some workers are more capable than others, and more capable workers do better in most jobs. Although individual employers might benefit from being better able to identify the strongest performers among their applicants, the reported findings do not establish that there is any noteworthy short-term gain to be had from better matching of particular workers to particular jobs.

Having said this, I would readily concede that there are other arguments one might make for taking steps to improve employers' ability to assess potential job candidates. One appealing argument, advanced elsewhere by this paper's author, concerns the desirable incentives that potentially would be created for students and young workers if they knew that information about their school and work histories was readily available to employers. A full evaluation of this argument is well beyond

the scope of my comments today, but it is certainly an argument that merits attention.

General Discussion: Henry Farber raised a question about the model specifications used in the paper. Although data presented in the paper implied that people who were referred to their jobs through the employment service were less productive than other workers, he noted that people who obtained their jobs through informal contacts were the excluded category in the equations. He argued that using this baseline might be inappropriate, because informal contacts might be an efficient way to find a job by allowing for the transmission of private information not available to others. He said that the base should instead be random assignment of people to jobs.

Richard Schmalensee argued that one must not forget that the employment service is a government agency. Because of its public status, the employment service did not necessarily have the same incentives to improve its job-matching efficiency as a private employment service would have had. Schmalensee traced the changing historical mission of the employment service, noting that in its early days it had a high share of total job placements, which it subsequently lost when it decided that its main objective was affirmative action. Schmalensee suggested that the move to the GATB system was likely motivated by a desire to regain lost market share, although at the expense of the affirmative action objective. He argued that the changing function of the employment service might not have affected who received what jobs in the overall economy, but rather who received what jobs through the employment service. Given the current job placement share of the employment service, he wondered how relevant its move toward testing was to the overall labor market.

Zvi Griliches noted that using employment test results to make "better" job matches might not have a significant economic effect. According to Griliches, if productivity is simply personal productivity that an individual carries into any job he or she takes, then better allocation affects only income distribution, not total productivity. Although it is generally believed that better job matches would increase total productivity, he noted that this still needed to be proved empirically.

Gary Burtless agreed that some studies showed only small overall productivity gains from better job matches, but he noted that Ed Den-

ison's work on the slowdown in productivity growth in the 1970s attributed part of that phenomenon to the phasing out of employment tests.

Commenting on Alan Krueger's point that the private market would have fulfilled any demand for better employee-job matches, Burtless said that because an information exchange was involved, it was possible that the market was not functioning properly and that government intervention through the operation of an employment service might therefore be economically worthwhile. He said that a central location, where workers could obtain listings of job openings along with information on wage and work conditions and where employers could find a good cross section of the available labor pool, could provide substantial benefits. He also noted that in some European countries, public employment services had a monopoly right to knowledge about job openings that would be available to outside job applicants. Because they know where available jobs are located, these services can protect the unemployment insurance system against malingering on the part of the insured unemployed.

Would employment tests give students greater incentives to work harder in school? Burtless observed that American students reached age seventeen or eighteen generally knowing less math and science than students in other countries where they were required to take national standardized exams.

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