Volume Title: Training and the Private Sector

Volume Author/Editor: Lisa M. Lynch

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-49810-7

Volume URL: http://www.nber.org/books/lync94-1

Conference Date: December 15-17, 1991

Publication Date: January 1994

Chapter Title: The Impact of Previous Training on Productivity and Wages

Chapter Author: John Bishop

Chapter URL: http://www.nber.org/chapters/c8781

Chapter pages in book: (p. 161 - 200)

6 The Impact of Previous Training on Productivity and Wages

John H. Bishop

6.1 Introduction

Workers who are assigned to the same job and paid the same wage often differ greatly in productivity. Coefficients of variation of individual productivity in specific jobs, based on hard measures of physical output, average .144 for factory operatives, .35 for sales clerks, and .28 for craft workers (Hunter, Schmidt, and Judiesch 1988). This paper examines whether and to what extent variations in productivity (and other job outcomes) across workers doing the same job at the same firm can be predicted by information on the background and training of the individual worker.

Our primary goal in undertaking this analysis is to test for third-party benefits to employer-provided training. When employers are asked why they do not do more training, they often say that most firms find it is cheaper to poach trained workers from competitors than to train their own skilled workers. Since trained workers are paid more than untrained workers, these employers are saying that the wage premium is smaller than extra productivity net of the cost of training the worker. Put in economics jargon, what these employers are claiming is that "training generates third-party externalities." This claim will be tested in this paper. We consider five specific questions:

John H. Bishop is associate professor at the New York State School of Industrial and Labor Relations, Cornell University, and a senior research associate at the Center for Advanced Human Resource Studies and the National Center on the Educational Quality of the Work Force.

This paper is based on research that was funded in part by the U.S. Department of Education grant to the Center on Educational Quality of the Work Force, agreement R117Q00011-91, as administered by the Office of Educational Research and Improvement, U.S. Department of Education, and by grant USDOL J-9-M-3-0165 from the Employment and Training Administration, U.S. Department of Labor. The opinions and conclusions expressed herein are solely those of the author and should not be construed as representing the opinions or policies of any agency of the United States government.

- Does the time required to train a new employee tend to be less if the individual has already received relevant training at a school or in a previous job? By how much? Which type of training has the biggest effect?
- Is the reported productivity of a new employee higher if the individual has previous relevant training? By how much? Which type of training has the biggest effect?
- Are the probabilities of a quit or discharge related to whether the new employee has previous relevant training? Which type of training has the biggest effect?
- Is the wage paid a new employee higher if the individual has previous relevant training? Which type of previous training has the biggest effect?
- Does the firm obtain greater profits if it successfully recruits workers who have previous relevant training? In other words, is the productivity net of training, turnover, and wage costs consistently higher for new hires who have previous relevant training? What type of previous training increases profits the most?

The purpose is *not* to estimate the structural relationship between indicators of skill and job performance so that we may predict the performance of prospective new hires. The unknown character of the selection process by which job applicants are selected for and retrained in jobs makes unbiased estimates of structural relationships impossible.[1] We are examining instead what kind of relationship between personal characteristics and productivity survives the selection process which determines who is hired and who is retained in a job.

The issues raised by the first four questions are different from those raised by the last. Employees with equal tenure in a job are not always paid the same

1. We do not need to estimate a structural model of the relationship between background and job performance. Such models cannot be estimated using a sample of job applicants, without bias, because of the truncated nature of the sample (the applicants who were believed to have low productivity were not hired, so observations on their job performance are not available) (Brown 1982). If hiring selections were based entirely on worker characteristics included in the model, unstandardized coefficients would provide unbiased estimates of the structural relationship between these characteristics and job performance. Unfortunately, however, incidental selection based on unobservables such as interview performance and recommendations is very probable (Thorndike 1949; Olson and Becker 1983; Mueser and Maloney 1987). One cannot argue that, in a selected sample such as accepted job applicants, these omitted unobservable variables are uncorrelated with the included variables that were used to make initial hiring decisions and, therefore, that coefficients on included variables are unbiased. When someone with 10 years of formal schooling is hired for a job that normally requires 12 years of schooling, there is probably a reason for that decision. The employer saw something positive in that job applicant (maybe the applicant received a particularly strong recommendation from previous employers) that led to the decision to make an exception to the rule. Our data set does not include information on these compensating factors which may have induced the firm to hire the individual, so the coefficient on schooling is likely to be biased toward zero. This phenomenon also causes the estimated effects of other worker traits used to select workers for the job such as previous relevant work experience to be biased toward zero. Worker characteristics which were not used to select new hires will have either zero or positive correlations with the unobservable so their coefficients will probably not be subject to a downward bias.

wage, particularly at small firms. In the Employment Opportunity Pilot Projects–National Center for Research in Vocational Education (EOPP-NCRVE) employer survey—a sample dominated by small establishments—the standard deviation of the log of the wage paid to incumbents in a particular job was .146. Variation in the wage paid for particular jobs accounted for 4 percent of the total variation of starting wage rates in the sample and 5 percent of the variation in the current wage rates of job incumbents. When firms offer different wage rates to different hires, a perfectly competitive labor market is quite consistent with substantial differences in the expected productivity, training requirements, or turnover rates of new employees hired for a specific job.

6.2 Hypotheses

6.2.1 Are Employer Expectations of New-Hire Productivity Rational?

If assessments of differences in the expected productivity of job applicants grouped by traits such as schooling and training are generally accurate, we would expect wage differentials for visible worker traits to approximate productivity differentials. Thus, if expectations regarding the productivity of new hires are rational and if perfect competition prevails in the labor market, the ex post profitability of a new hire should not be predictable by information that is generally available to hiring-decision makers. Therefore, the null hypothesis is

> H_0 : When new hires for a particular job are compared, measures of the ex post profitability of the new hire and of the discrepancy between expected and realized productivity—the surprise in productivity realizations—should not be predictable by information on worker characteristics that is available to all participants in the market at the time the hiring decision is made.

Labor markets are not perfect, however. Information about job applicants and about alternative jobs is incomplete and costly to obtain. Even when good costless information on skills is available to all participants in the labor market, the null hypothesis that new-hire profitability is unpredictable may be violated if:

1. The size of the match-specific component of worker productivity and job attractiveness varies a good deal across jobs and this variation is predictable. Match specificity can result from skills which are useful at only one firm or at only a few local firms. This occurs when on-the-job training (OJT) or school-provided training develops industry- or occupation-specific skills and there are only a few firms in the locality that use these skills. Employers who do use these skills will not have to pay wages that fully reflect the high productivity of these workers at their firm. The attractiveness of a specific job to a particular worker—which is indicated by the worker's reservation wage for taking the

job—is also match specific. For example, mothers who are able to work only at certain times of the day or at a short commute from their homes will have lower-than-average reservation wages for jobs which meet these criteria. A good fit with coworkers and supervisors may also lower an individual's reservation wage. When match-specific rents are large, a whole range of wage rates may be consistent with preservation of the job-worker match. From the firm's point of view a wide gap between a worker's productivity and her reservation wage is a good thing, because it means turnover will be low and the expected profitability of the match will be high. Worker characteristics, such as having occupation-specific training and being a married women, which are associated with a larger gap between productivity and the worker's reservation wage should, therefore, have a positive relationship with the expected profitability of a match.

2. The quality of the new hires a firm is able to attract varies cyclicly and seasonally. When the economy is in recession, firms are able to hire workers with greater-than-average amounts of previous training and experience and higher-than-average levels of expected productivity. At the peak of the cycle, when labor markets are tight, employers are often forced to hire workers who have less training and experience and who are less productive. The result is that some of the firm's employees (those hired during a recession) are simultaneously more productive and better credentialed (i.e., have greater training and experience) than other employees. Thus, cyclical and seasonal variations in the tightness of labor markets can produce a positive within-firm correlation between credentials and the profit generated by particular employees, even if all new hires at any given point in time have identical expected productivity.

Information imperfections are a second major reason the profitability of new hires may be predictable.

3. Workers are not well informed about the wages they can command at another firm. The costs of a job search—travel costs, lost earnings, and mental anguish—are considerable. In unskilled and semiskilled labor markets, job seekers seldom have the chance to accumulate job offers and choose between them when a thorough search has been completed. Consequently, three-fourths of these job seekers accept the first job offer they receive. The result is considerable random variation in the expected productivity of new hires. Employers find that some of the time they are able to recruit and hire a worker with exceptionally strong credentials and higher-than-average expected productivity. On other occasions, the highly qualified applicants cannot be recruited and the firm must settle for someone with average credentials and average expected productivity. In this way, random variation in the expected quality of the new hires produces a positive correlation between productivity and credentials, even among people doing the same job who are paid the same wage.

4. Employers also lack good information on the occupational skills that job applicants have developed on previous jobs. At the National Federation of Independent Business (NFIB) firms surveyed for this paper, 60 percent of recent

hires had been selected without a single contact being made with a supervisor on a previous job. Only 24 percent had been asked to demonstrate their skills prior to being hired. Only 7 percent of the new hires had shown their prospective employer a certificate of training received on a previous job. When clerical, service, and blue-collar jobs are being filled, employers devote less than 10 hours on average to recruiting and selecting workers for each opening.

In many cases, employers learn of the existence of previous training and its relevance to their job *after* the employee has been working at the firm for a while. Under these circumstances, one might expect new information on previous training to be a good predictor of the relative productivity of workers, even while information that was publicly available during the hiring process is not predictive. One way to test specifically for this is to measure and then predict the difference between productivity realizations and employer expectations of that productivity held at the time the hiring decision was made. Such a test will be conducted in this paper. These four considerations lead me to propose the following hypotheses.

H_1 : When workers doing the same job are compared, the profitability of a new hire—realized productivity, net of training, wage, and turnover costs—should be positively related to indicators of occupation- and firm-specific skills, such as previous relevant work experience and relevant school-based occupational training.

H_2 : When workers doing the same job are compared, the profitability of a new hire should be negatively related to indicators of high reservation wages, such as schooling, total work experience, and being a married male, and positively related to indicators of low reservation wages, such as being a married women and being Hispanic (because of its association with being an undocumented worker).

H_3 : When workers doing the same job are compared, the surprise in the productivity realizations of new hires—realized productivity minus expected (at time of hiring) productivity—should be positively related to indicators of the relevance of previous work experience and training that may not have been available to hiring-decision makers at the time hiring decisions were made.

6.2.2 The Empirical Model

The best method of testing for relationships between worker characteristics and job performance and profitability is to compare two individuals at the same firm in the same job and see how differences in reported productivity, training costs, turnover, and wages are related to differences in background characteristics.

Let us assume that, in a sample of people who have been recently hired for the jth job, job performance outcomes ($\mathbf{Y}_{ji}$) depend on a vector of personal characteristics describing the individual's background and general education ($\mathbf{X}_i$), on a vector of individual skills and training relevant to this specific job

($\mathbf{S}_{ji}$), and on a vector of job characteristics ($\mathbf{Z}_j$). Real-world relationships in the levels of these characteristics are not, however, additive. Shop-floor practices and technology often constrain the degree to which individual differences in learning ability or competence can generate individual differences in productivity or training. If the workers of firm A are more adaptable and competent than those of firm B, firm A may be able to introduce profitable changes in technology and work assignments that firm B is unable to introduce. Similar differences in adaptability and competence between occupants of a particular job might generate much smaller effects on individual productivity.

Alternatively, the opposite might prevail. Work might be structured so that equipment breakdowns can be diagnosed and repaired by just a few highly skilled operatives. Once a few highly skilled operatives are recruited or trained, there may be little need to train others. Either way, the effects of individual characteristics and recruitment source on the productivity, turnover, and profitability of a new hire may differ depending on whether one is analyzing differences within firms or differences across firms. Processes by which individuals are selected and retained in particular jobs may also cause $\boldsymbol{\beta}$ coefficients to be different from $\mathbf{A}$ coefficients. A specification which takes this into account is

$$(1) \qquad Y_{ijk} = \boldsymbol{\beta}_k'(\mathbf{X}_i - \mathbf{X}_j) + \boldsymbol{\beta}_k(\mathbf{S}_{ij} - \mathbf{S}_j) + \mathbf{A}_k'\mathbf{X}_j + \mathbf{A}_k\mathbf{S}_j + \boldsymbol{\theta}_k\mathbf{Z}_j + u_{ijk} + v_{jk},$$

where

- Y_{ijk} is the kth outcome of the match between employee i and job j; the outcomes being modeled include turnover, wage rate, and supervisor reports of the worker's productivity and profitability;
- $\mathbf{X}_i$ is a vector of background characteristics of individual i which describe generic competencies (means of these characteristics for a job are $\mathbf{X}_j$);
- $\mathbf{S}_{ij}$ is a vector of characteristics of individual i, describing skills and training that effect performance in job j (means of these characteristics for a job are $\mathbf{S}_j$);
- $\mathbf{Z}_j$ is a vector of measurable characteristics of job j, including characteristics of the employer;
- u_{ijk} is a random error that is specific to the match between individual and job for the kth outcome; and
- v_{jk} is an error that is specific to the job or employer-respondent for the kth outcome.

The $\boldsymbol{\beta}_k$'s characterize the within-job relationship between individual characteristics and productivity; the $\mathbf{A}_k$'s characterize the relationship across jobs. Equation (1), however, can seldom be estimated, for two reasons. First, for many of the most interesting outcomes, such as supervisory ratings of skills

and job performance and measures of individual output, operational measures are inherently relative to others at the firm and not on a scale that is comparable from firm to firm or even from job to job within one firm. Second, data on the job-specific mean values of $\mathbf{X}$ and $\mathbf{S}$ are generally not available.

When $\mathbf{X}_i$'s and $\mathbf{S}_{ij}$'s are used to predict $\mathbf{Y}$ in population samples, $\mathbf{A}_k'$ and $\mathbf{A}_k$ are constrained to equal $\boldsymbol{\beta}_k$ and $\boldsymbol{\beta}_k$, and the estimated coefficients end up being a mixture of the two. This is fine in some applications, but it is a problem in others. A second problem is caused by unmeasured characteristics of the job and the firm ($\mathbf{v}_{jk}$) which influence wage rates, productivity, and turnover. When the covariance between $\mathbf{v}_{jk}$ and $[\mathbf{X}_i, \mathbf{S}_{ij}]$ is nonzero, biased estimates may result.

Since our interest is in the $\boldsymbol{\beta}_k$'s, not the $\mathbf{A}_k$'s, both of these problems can be finessed by estimating a model predicting the differences in the outcomes experienced by two people in the same job at the same firm as a function of differences in their background characteristics, as is shown in equation (2):

$$(2) \qquad Y_{1jk} - Y_{2jk} = \boldsymbol{\beta}_k'(\mathbf{X}_1 - \mathbf{X}_2) + \boldsymbol{\beta}_k(\mathbf{S}_{1j} - \mathbf{S}_{2j}) + (u_{1jk} - u_{2jk}) ,$$

where persons 1 and 2 both work in the same job j and matched pairs of new hires for each job j are the data. Estimating this model produces unbiased estimates of $\boldsymbol{\beta}'_k$ and $\boldsymbol{\beta}_k$ if the $\mathbf{X}_i$'s and the $\mathbf{S}_{ij}$'s are not correlated with the u_{ijk}'s.

6.2 Data on Training and Productivity Growth

The models described above will be estimated in two different data sets: the EOPP-NCRVE Employer Survey and a survey of a stratified sample of the membership of the NFIB.

6.2.1 The EOPP-NCRVE Employer Survey

The EOPP-NCRVE Employer Survey conducted in late spring 1982 provides a unique data set for examining how the education, training, and work experience of new hires affect the amount of on-the-job training they are given and the productivity they achieve during their first year or so on the job. It provides retrospectively longitudinal data on the time devoted to training and on the reported productivity of two new hires at 659 different firms.

The sample of jobs for which paired data are available was generated in the following manner. Telephone interviews were conducted with the owners/managers of 3,412 randomly selected establishments. Of these, 2,457 were single-establishment firms, and 930 were parts of corporations with multiple establishments. 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." A total of 818 employers could not provide information for a recent new hire. Most of these firms were small organizations that had not hired anyone in recent memory. The employers who 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.

Most of the establishments from which paired data are available are small. Seventy percent have fewer than 50 employees, and only 12 percent have more than 200 employees. Most of the respondents were owners/managers of small firms who were quite familiar with the performance of each of the firm's employees. At larger firms the personnel director provided information about the firm, and a line supervisor reported on the training costs and the productivity of the individual worker(s) sampled for the study.

Information was obtained on how many hours each of the two new hires for the particular job spent, 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 (T_{Wi}), (2) formal training programs (T_{Fi}), (3) informal individualized training and extra supervision by management and line supervisors (T_{Si}), and (4) informal individualized training and extra supervision by coworkers (T_{Ci}) (for relevant portions of the questionnaire, see Bishop, Barron, and Hollenbeck 1983).

A training-time index was constructed by first making assumptions regarding the relative value of trainer and trainee time and then combining the time invested in training activities by these various individuals during the first three months on the job. Expressed in coworker time units,

$$\text{(3)} \qquad \text{Training investment in the } i\text{th new hire} = 1.8T_{Fi} + 1.5{*}T_{Si} + T_{Ci} \\ .8T_{Wj} + 4 \, .$$

At the firms which supplied data on the training of a second employee, this index had an arithmetic mean of 168 hours and a geometric mean of 93 hours.

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 the worker left, for those who left 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 (while it was 8.2 percent for previous relevant experience, 6.7 percent for education, and 5.7 percent for the questions about starting wage rate) suggests that respondents felt capable of making such judgments and augurs well for the quality of the data that result. For the sample of firms which provided data on two new hires, the mean values of these indexes of reported productivity were 49.2 for the first two weeks, 64.7 for the next eleven weeks

and 75.4 at the time of the interview. A more thorough description of the EOPP-NCRVE data is provided in Appendix A.

6.2.2 The National Federation of Independent Business Survey

A survey was conducted of a stratified random sample of the 500,000 members of the NFIB during the first half of 1987. In order to increase the representation of larger firms, NFIB members 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 NFIB, so the sample does not contain data on employment outcomes at large multiestablishment firms. A four-page questionnaire was mailed to approximately 11,000 firms, and after three follow-up waves, 2,599 responses were obtained. Business owners who had no employees in the previous year, or who had not hired anyone in the past three years, were asked to check a box and send the questionnaire back completely blank; 569 blank questionnaires were returned. The questionnaire focused on the owners' experiences in hiring and training workers in a particular job. This job was selected by asking the owner the following question: "For which job have you hired the most people over the last two or three years. (If you have more than one job for which you have done a lot of hiring, please select the job requiring the greatest skill.) **All future questions refer to this job.**" After a series of general questions about the character of the job, the owner was asked to select two individuals who had been hired for this job and to answer all future questions specifically 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 two-and-a-half pages of questions about the two employees. Information of varying degrees of completeness were obtained on 1,624 person As and 1,403 person Bs. Nonresponse to particular questions reduced the sample further, so that the number of firms included in the estimation was 1,164 for starting wage rate and 1,121 for initial productivity.

Owners were asked about both starting wages and initial productivity at the beginning of the second week of employment and about current wage rates and current productivity. If one or both of the new hires had left the firm prior to the date of questionnaire completion, the owner was asked to provide information on the circumstances which prevailed "at the time of separation." Nevertheless, a number of respondents failed to provide data on outcomes at time of separation, so the sample size for analysis of current productivity was 833 and for current wage rates was 714.

The constraints of a mail questionnaire forced a simplification of questions about time devoted to training. Whereas the EOPP questionnaire distinguished formal training from informal training and further distinguished informal train-

ing by supervisors from informal training by coworkers, all three of these forms of training were combined in one very short question: "How many hours did you or an employee spend training or closely supervising A or B?" Two other types of training investment were distinguished. The questions were: "How many additional hours (beyond training and close supervision) did A/B spend learning the job by **watching others** rather than doing it?" and "How many hours did A/B spend reading manuals, etc., in order to learn the job?" Owners were asked to complete this question for the "first week" of employment and for the "next six months."[2] The training differential analyzed below is the logarithm of the ratio of the total number of hours spent in the three forms of training over the six-month period. The means and standard deviations of the variables used in the analysis of NFIB data are presented in Appendix C.

6.2.3 The Productivity Indexes: Validity of the Ratio-Scale Assumption

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 a 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 productivity level. Since 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, our 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 negatively correlated 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 this is the case, then our estimates of percentage

2. Unfortunately, respondents were not told what to do when they felt unable to estimate the time devoted to training. The result was that it was often not clear whether a blank response should be coded as a zero or as a "don't know." The following decision rules were adopted. Responses of "continuous," "DK," and "?" were coded as missing. If the employer had entered a "0" or "none" for one category of training and left other categories blank, blanks were coded as missing. If the employer had not answered the question about productivity at the end of six months, all training questions about the six-month period following the first week were coded as missing. Otherwise, a blank was coded as zero. This procedure probably errs on the side of retaining observations that should have been dropped, and this lowers calculated means for the sample. The resulting means for the first week on the job were 18.4 hours for trainer time, 5.7 hours for watching others, and 3.5 hours for reading manuals. For the next six months, the means were 54.6 hours for trainer time, 20.9 hours for watching others, and 12.0 hours for reading manuals.

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, the coefficient averages .13 for sales clerks and clerical, service, and blue-collar workers. This estimate of the coefficient of variation is smaller than the estimates of the coefficient of variation for yearly output derived from analysis of objective ratio-scale measures of output. These estimates were .35 in sales clerk jobs, .144 in semiskilled blue-collar jobs, .28 in craft jobs, .164 in routine clerical jobs, and .278 in clerical jobs with decision-making responsibilities (Hunter et al. 1988). 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 the estimated models might be influenced by this problem.

6.3 Results

Our hypotheses relate to the partial relationship between measures of previous training and experience and various indicators of job performance, while controlling for characteristics of the job that may vary within the pair of workers and for other background characteristics. Parallel analyses were conducted in the two data sets. Both data sets had measures of the following skill and training indicators—previous relevant work experience and its square, total work experience and its square, schooling, vocational education relevant to the job, training received at a private vocational/technical institution that is relevant to the job, and gender—which were entered simultaneously into the model. Characteristics of the job-worker match that might influence the outcome were also included in the model. When current or most recent reported productivity, current wage, and current profitability are predicted, tenure, tenure squared, and tenure during the first year were included as controls. For models predicting starting wage rates and initial profitability, the date of the hire and its square were controlled for. In the models estimated in the EOPP-NCRVE data, controls were entered for the following: hours worked per week, a dummy equal to one when the job was supposed to be temporary, a dummy equal to one when the new hire was subsidized by a Comprehensive Employment and Training Act (CETA)-OJT contract, a dummy equal to one when the employee was eligible for Targeted Jobs Tax Credit (TJTC) subsidy and the employer knew this when the hiring decision was made, and a dummy equal to one when the employee was going to school part-time while working.

An almost identical specification was estimated in the NFIB data. The dif-

ference was that the NFIB model contained no controls for receipt of subsidies for hiring particular workers, but it does contain controls for race and Hispanic ethnicity. Results for the EOPP and NFIB data sets are presented next to each other in columns 1 and 2, respectively, of tables 6.1 to 6.6. Column 3 presents the results of estimating a more complete model in NFIB data, which contains additional information on previous training received by the new hire. The additional variables are a dummy for having received relevant formal training at the work site on a previous job, a dummy for having received relevant formal off-site training sponsored by a previous employer, a dummy for having received relevant training from the military, a dummy for having received relevant training from a Job Training Partnership Act (JTPA) program, total num-

Table 6.1 **Log Training Time**

Variable	EOPP		NFIB		Augmented NFIB	
Previous employer training						
Relevant experience	−.064***	(5.22)	−.050***	(4.53)	−.045***	(3.97)
Relevant experience squared	.0013***	(3.04)	.00140**	(3.34)	.0012***	(3.02)
First-year's relevant experience	−.082*	(1.69)	−.125**	(2.21)	−.044	(.68)
Formal training on job					−.168***	(2.81)
Formal training off job					.070	(.64)
Schooling						
Years of schooling	.0084	(.69)	.005	(.38)	.006	(.43)
Relevant public vocational training	−.082**	(2.30)	.047	(.76)	.063	(.86)
Relevant private vocational training	−.108*	(1.33)	−.081	(1.01)	−.040	(.50)
Relevant training from military					.218*	(2.21)
Relevant training from JTPA					.150	(.59)
Years of occupational training					−.025	(1.05)
Total experience	.0041	(.69)	.0064	(.98)	.0041	(.61)
Total experience squared	−.00013	(.79)	−.00020	(1.13)	−.00018	(.95)
Demographic background						
Female	−.105*	(1.71)	−.083	(1.23)	−.139***	(1.92)
Married female					.109*	(1.99)
Married male					−.053	(1.08)
Black			.026	(.27)	.038	(.39)
Hispanic			.148	(1.30)	.145	(1.27)
Temporary job	−.239***	(3.32)	−.082	(1.14)	−.081	(1.13)
F-test on model	8.4***		6.2***		5.0***	
R^2	.209		.075		.094	
RMSE	.225		.701		.696	
N	494		939		939	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

Table 6.2 Productivity at End of First Week

Variable	EOPP		NFIB		Augmented NFIB	
Previous employer training						
Relevant experience	.029***	(4.38)	.045***	(7.36)	.042***	(6.80)
Relevant experience squared	−.00046**	(2.01)	−.00105***	(4.66)	−.00097***	(4.25)
First-year's relevant experience	−.020	(.76)	.047	(1.49)	.004	(.11)
Formal training on job					.095***	(2.83)
Formal training off job					.003	(.06)
Schooling						
Years of schooling	.0096	(1.50)	.0120	(1.49)	.0100	(1.20)
Relevant public vocational training	.042**	(2.10)	.044	(1.29)	.020	(.49)
Relevant private vocational training	.125***	(2.78)	.101**	(2.30)	.100**	(2.21)
Relevant training from military					−.032*	(.62)
Relevant training from JTPA					.080	(.74)
Years of occupational training					.015	(1.17)
Total experience	−.0097***	(2.98)	−.0019	(.53)	−.0023	(.60)
Total experience squared	.00026***	(2.91)	−.00004	(.42)	.00004	(.39)
Demographic background						
Female	.006	(.16)	.013	(.36)	.002	(.04)
Married female					.024	(.79)
Married male					−.007	(.25)
Black			.031	(.57)	.032	(.57)
Hispanic			−.062	(1.01)	−.058	(.94)
Temporary job	.078**	(1.97)	.008	(.21)	.008	(.21)
Intercept	.005	(.37)	.023*	(1.79)	.021*	(1.66)
F-test on model	8.9***		12.9***		8.8***	
R^2	.218		.123		.132	
RMSE	(.262)		.422		.421	
N	494		1,121		1,122	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

ber of years of school-based vocational training, and separate dummies for being a married female or a married male.

Despite differences in sampling, in selection processes, and in variable definitions, the two data sets generate remarkably similar findings. The data analysis strategy being employed in this paper has not been tried before, so it is quite heartening that the results turn out to be remarkably robust. For example, in both data sets initial productivity and required training are significantly influenced by relevant vocational education and years of previous relevant work experience but not by years of schooling or total work experience. Consequently, the discussion of the results will be organized not around partic-

Table 6.3 Starting Wage

Variable	EOPP		NFIB		Augmented NFIB	
Previous employer training						
Relevant experience	.016***	(3.69)	.026***	(7.13)	.023***	(6.40)
Relevant experience squared	−.00037**	(2.49)	−.00052***	(3.85)	−.00046***	(3.42)
First-year's relevant experience	.0010	(.06)	.025	(1.42)	.015	(.74)
Formal training on job					.019	(1.00)
Formal training off job					.001	(.04)
Schooling						
Years of schooling	.014***	(3.49)	.019***	(4.09)	.016***	(3.52)
Relevant public vocational training	.031**	(2.44)	.033*	(1.70)	.0.15	(.64)
Relevant private vocational training	.044	(1.55)	.068***	(2.70)	.069***	(2.71)
Relevant training from military					−.004	(.13)
Relevant training from JTPA					.0003	(.01)
Years of occupational training					.011*	(1.51)
Total experience	.0079***	(3.76)	.0116***	(5.72)	.0094***	(4.43)
Total experience squared	−.00014**	(2.35)	−.00052***	(3.85)	−.00020***	(3.63)
Demographic background						
Female	.024	(1.1)	−.074***	(3.43)	−.030	(1.33)
Married female					−.018*	(1.04)
Married male					.092***	(5.97)
Black			−.008	(.26)	−.015	(.49)
Hispanic			−.110***	(3.19)	−.119***	(3.51)
Temporary job	.035***	(1.36)	−.028	(1.30)	−.027**	(1.30)
Years before hired	.039***	(6.06)	.002	(.16)	.006	(.44)
Years before hired squared	.0020***	(3.78)	.0027*	(1.74)	.0023	(1.48)
F-test on model	10.8***		29.2***		22.2***	
R^2	.296		.263		.290	
RMSE	026		.244		.240	
N	454		1,164		1,164	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

ular data sets, not even around dependent variables, but around categories of right-hand-side variables:

- Work experience—contrasts between relevant experience and total experience;
- Firm specificity of skills—contrasts between the effects of tenure and of previous relevant work experience on current productivity;
- Schooling and relevant occupational training obtained at schools;
- Demographic characteristics—gender interacted with marital status and minority status.

Table 6.4 Current Productivity

Variable	EOPP		NFIB		Augmented NFIB	
Previous employer training						
Relevant experience	.0157**	(2.14)	.023***	(3.33)	.022***	(3.17)
Relevant experience squared	−.00004	(.18)	−.00046*	(1.85)	−.00043*	(1.74)
First-year's relevant experience	.033	(1.08)	−.026	(.74)	−.031	(.77)
Formal training on job					−.003	(.08)
Formal training off job					.159**	(2.36)
Schooling						
Years of schooling	.017**	(2.35)	.024***	(2.60)	.028***	(2.87)
Relevant public vocational training	.024	(1.09)	.039	(1.01)	.045	(.97)
Relevant private vocational training	.069	(1.39)	.082*	(1.69)	.103**	(2.06)
Relevant training from military					.098	(1.62)
Relevant training from JTPA					.154	(1.39)
Years of occupational training					−.021	(1.38)
Total experience	.0015	(.43)	−.0042	(1.01)	−.0046	(1.06)
Total experience squared	.00002	(.21)	−.000004	(.04)	.000004	(.03)
Demographic background						
Female	.028	(.72)	.024	(.56)	.009	(.20)
Married female					.020	(.58)
Married male					−.027	(.86)
Black			−.048	(.79)	−.047	(.77)
Hispanic			−.070	(.97)	−.069	(.96)
Temporary job	.031	(.68)	.076*	(1.79)	.076*	(1.80)
Tenure						
Years of tenure	−.108***	(2.62)	.0885**	(2.04)	.088**	(1.80)
Tenure squared	.0014***	(3.19)	−.0090**	(2.04)	−.0088**	(2.01)
Tenure first year	.430***	(6.86)	.328***	(4.98)	.324***	(4.92)
F-test on model	8.7***		8.7***		6.5***	
R^2	.234		.138		.150	
RMSE	.305		.412		.411	
N	534		833		833	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

6.3.1 Relevant versus Irrelevant Prior Work Experience

The effects of both relevant and irrelevant work experience on training costs, productivity, turnover, wage rates, and profitability are summarized for the EOPP data in table 6.7. Results from analysis of NFIB data are presented in table 6.8.

Relevant Work Experience

According to their employers, the new hires in the EOPP data had an average of 2.3 years of relevant work experience, and the new hires in the NFIB data

Table 6.5 Current Wage

Variable	EOPP		NFIB		Augmented NFIB	
Previous employer training						
Relevant experience	.011**	(2.13)	.026***	(5.22)	.025***	(4.86)
Relevant experience squared	−.00023	(1.33)	−.00050**	(2.55)	−.00047**	(2.36)
First-year's relevant experience	.031	(1.42)	.011	(.50)	.020	(.80)
Formal training on job					−.013	(.52)
Formal training off job					−.003	(.06)
Schooling						
Years of schooling	.016***	(3.12)	.018***	(3.23)	.016***	(2.66)
Relevant public vocational training	.034**	(2.17)	.025	(1.05)	.014	(.50)
Relevant private vocational training	.064*	(1.78)	.068**	(2.09)	.048	(1.42)
Relevant training from military					−.066*	(1.66)
Relevant training from JTPA					−.106	(1.52)
Years of occupational training					.019*	(1.86)
Total experience	.0050*	(1.95)	.0099***	(3.72)	.0103***	(3.71)
Total experience squared	−.00008	(1.14)	−.00026***	(3.51)	−.00026***	(3.44)
Demographic background						
Female	.008	(.26)	−.113***	(4.45)	−.088***	(3.22)
Married female					−.038*	(1.72)
Married male					.033*	(1.68)
Black			−.011	(.31)	−.010	(.28)
Hispanic			−.136***	(3.08)	−.131***	(2.99)
Temporary job	−.082**	(2.52)	−.060**	(2.17)	−.057**	(2.02)
Tenure						
Years of tenure	.045	(1.50)			.086***	(3.99)
Tenure squared	.0002	(.50)			−.0056**	(2.07)
Tenure first year	.074	(1.62)	−.0054**	(1.97)	.022	(.50)
Intercept			−.014	(1.40)	−.019	(1.83)
F-test on model	9.0***		18.9***		13.6***	
R^2	.240		.290		.302	
RMSE	.220		.242		.240	
*N*s2	534		714		714	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

had an average of 5.3 years of relevant work experience when hired. Relevant work experience significantly increased the productivity of new hires and significantly reduced the time required to train them (see cols. 1 and 2 of table 6.7). Substituting five years of relevant experience for an equivalent amount of irrelevant experience, while holding total experience constant, raised productivity by 25 percent in the first two weeks, by 15 percent over the course of the next ten weeks, and by 8–9 percent at the time of the interview. It also reduced training costs by one-third and raised productivity net of training costs by 44

Table 6.6 **Profit in First Months**

Variable	EOPP First Quarter		NFIB First Week		NFIB End of Six Months		Augmented NFIB First Week		Augmented NFIB End of Six Months	
Previous employer training										
Relevant experience	.0239***	(2.69)	.025***	(2.88)	.0124	(1.64)	.025***	(2.84)	.013*	(1.65)
Relevant experience squared	−.00030	(.97)	−.0008**	(2.39)	−.0004	(1.51)	−.0008**	(2.41)	−.0004	(1.50)
First-year's relevant experience	.044	(1.24)	.076*	(1.74)	−.060	(1.58)	.051	(1.01)	−.053	(1.22)
Formal training on job							.046	(.98)	−.011	(.27)
Formal training off job							.104	(1.20)	.138*	(1.84)
Schooling										
Years of schooling	−.015*	(1.79)	−.032***	(2.81)	−.013	(1.32)	−.030**	(2.52)	−.012	(1.16)
Relevant public vocational training	.047*	(1.82)	.025	(.54)	−.016	(.40)	.032	(.56)	.008	(.16)
Relevant private vocational training	.055	(.93)	−.004	(.05)	−.005	(.09)	−.009	(.14)	−.0005	(.09)
Relevant training from military							.026	(.34)	−.006	(.10)
Relevant training from JTPA							−.127	(.90)	.128	(1.04)
Years of occupational training							−.0088	(.46)	−.015	(.90)
Total experience	−.014***	(3.24)	−.012**			(4.77)	−.0093*	(1.71)	−.0196***	(4.17)
Total experience squared	.00035***	(2.74)	.00026*	(1.80)	.00046***	(3.96)	.00022	(1.48)	.0004***	(3.15)

(*continued*)

Table 6.6 (continued)

Variable	EOPP First Quarter		NFIB First Week		NFIB End of Six Months		Augmented NFIB First Week		Augmented NFIB End of Six Months	
Demographic background										
Female	.044	(.98)	.161***	(3.12)	.112**	(2.50)	.134**	(2.38)	.063	(1.30)
Married female							−.023	(.55)	.038	(1.05)
Married male							−.088**	(2.29)	−.087***	(2.64)
Black			.021	(.27)	−.062	(.93)	.026	(.34)	−.058	(.87)
Hispanic			−.076	(.88)	.020	(.26)	−.054	(.63)	.038	(.51)
Temporary job	.096*	(1.84)	.106*	(1.92)	.077	(1.60)	.110**	(2.00)	.078	(1.63)
Knew TJTC	.028*	(1.84)								
CETA-OJT contract	−.075	(.53)								
Subsidized hire	.079	(.67)								
Coop student	−.0016	(.03)								
F-test on model	4.0***		3.5***		3.6***		2.75***		3.07***	
R^2	.135		.058		.059		.068		.075	
RMSE	.328		.506		.440		.505		.438	
N	454		819		819		819		819	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

Table 6.7 Effects of Work Experience in EOPP Data (%)

Outcomes	Relevant Experience		Total Experience	
	1 Year	5 years	5 Years	R^2
Productivity net of training cost during first three months	10***	44***	−3.2*	.206
Productivity				
First two weeks	5***	25***	−6.0***	.209
Next twelve weeks	3.4***	15***	−3.4**	.159
Most recent for full sample	1.8***	8.2***	−.9	.163
Current for stayers	2.0***	8.9***	0	.182
Required training				
Formal training	−8*	−35*	.7	.075
Informal by management	−8***	−36***	3.4	.082
Informal by co-workers	−8***	−37***	−8.0	.056
Total training	−7***	−33***	−1.7	.213
Wages				
Starting	1.4***	6.4***	3.6***	.292
Most recent for full sample	1.3***	5.6***	2.3*	.230
Current for stayers	1.8***	9.8***	2.1*	.200
Profitability of hire during first three months	7***	30***	−12***	.127
Productivity minus wage				
Most recent for full sample	.8	3.9	−3.0*	.054
Current for stayers	.7	3.3	−2.7*	.078
Turnover				
Tenure	2	8	−.6	.646
Quit	3	15	−3.0	.054
Discharge or layoff	−15	−65**	10.0	.042

Note: Fixed effects regressions run on 455–524 pairs of new hires in the 1982 National Employer Survey. All models contained control variables for whether currently a vocational education student, years of schooling, vocational education interacted with years of schooling, private vocational education, sex, whether hired in a temporary job, whether known to be eligible for a subsidy when hired, and current average hours per week. Models for current or most recent wage, productivity, and profitability have additional controls for actual tenure and tenure squared. The turnover regressions are based on 510 pairs of new hires for nontemporary jobs and control the log of potential tenure and its square.

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

percent. Because workers with five years of relevant experience are so much more productive, their probability of discharge or layoff falls by 65 percent, from 12 percent to about 4 percent. Thus, despite their slightly higher quit rate, they have slightly greater expected tenure than new hires who lack relevant experience.

Table 6.8 **Effects of Work Experience in NFIB Data (%)**

Outcomes	10 Years of Relevant Experience	10 Years of Total Experience	Relevant Experience Replaces Irrelevant Experience	Formal Training	
				On Job	Off Job
Productivity					
End of first week	30.0	−2.7	32.7***	9.5***	.3
End of six months	13.4	−6.9*	20.3***	−.9	6.6
Current	9.8	−3.5	13.3***	.3	15.9**
Suggestions	43.5	−1.7	45.2***	13.6	37.3**
Required training	−29	2.4	−30.7***	−17.3***	7.2
Wage rates					
Starting	31.4	7.6***	22.1***	1.9	.1
Current or most recent	34.5	8.0***	24.6***	−1.3	−.2
Expected productivity	9.7	−.5	10.2***	4.2***	2.5
Surprise	.6	−6.6**	7.2**	−4.6	4.2
Profitability					
End of first week	22.6	−10.3*	32.8***	6.7	15.2
End of six months	12.3	−15.5***	3.2*	−1.1	13.8*
Current or most recent	−13.0	−6.5	−6.6	2.0	18.6*
Turnover					
Leave	−15.6	−4.5	−11.1	−8.4	−31.0
Quit	−49.4	−22.2	−27.2	−8.9	−.4
Discharge or layoff	31.6	20.0	11.6	−5.9	−68.8

Note: Col. 1 is the estimated effect of increasing both relevant and total experience by 10 years; no test of significance was calculated for this variable. Col. 2 presents the effect of increasing total experience by 10 years, while holding relevant experience constant. Col. 3 presents the estimated effect of 10 added years of relevant experience, while holding total experience constant. Percentage effects for required training and wage rates are anti-logs of 10-year effects calculated from logarithmic models for training and wage rates. The suggestions index ranges from 0 to 3 and has a mean of 1.027.

*Significant at the 10 percent level.

**Significant at the 5 percent level.

***Significant at the 1 percent level.

In the NFIB data, 10 years of relevant experience, with total experience held constant, increased productivity by 32.7 percent in the first week, by 20.3 percent at the end of six months, and by 13.3 percent at the time of the interview. Workers with an extra 10 years of relevant experience required 30.7 percent less training time during the first six months and were significantly more likely to make suggestions which improved sales or productivity.

Irrelevant Work Experience

Total work experience was defined as the total number of years since completing school or reaching the legal working age, whichever is smaller. The mean of this variable was 8.2 years in EOPP data and 10.2 years in NFIB data. The models contained controls for relevant experience, so the coefficient on total experience measures the effect of experience that was not relevant to the job. *Irrelevant* experience has effects on productivity and training costs dramatically different from those of relevant experience. In the EOPP data, five years of experience considered irrelevant by the employer was associated, during the first three months on the job, with new hires being 3–6 percent less productive. Productivity net of training costs was also about 3 percent lower. Irrelevant experience did not have significant effects on time devoted to training or on turnover. The analysis of the NFIB data yields similar results. Ten years of irrelevant experience had no significant impact on initial productivity, training requirements, or total turnover, but it reduced productivity after six months of tenure by a statistically significant 6.9 percent.

There are probably two reasons why irrelevant experience often has a negative association with productivity, in this data. Older workers who lack occupationally relevant experience may have a type of experience that produces habits and skills that must be unlearned when the individual enters a very different setting. This is certainly the view taken in Japan. A second possible reason is the obsolescence and forgetting of skills and knowledge gained in school that might be relevant to the job (Kohn and Schooler 1983). When relevant experience is held constant, total experience measures the time period over which potentially relevant skills that were gained in school have been depreciating through lack of use. Apparently these two effects outweigh beneficial effects from general OJT that is not relevant to the job at the new firm.

The contrast between relevant experience's large positive impact on productivity and irrelevant experience's negative impact has some important implications. When one looks across new hires in a specific job, it is the occupation- or industry-specific skills that have the greatest impact on productivity. Thus the key to making work experience pay off is gaining experience and training that are relevant to the career one plans to pursue and entering that career path immediately after leaving school. Changes in a career that do not make use of the occupation- or industry-specific skills that have been accumulated necessarily involve large sacrifices of productivity and income. The longer a particular career path has been pursued, the greater the sacrifice will be.

Even though it is associated with lower productivity, irrelevant experience is also associated with higher wage rates relative to co-workers. The effect of irrelevant experience on wage rates is about one-third of the size of the effect of relevant experience on wage rates.[3] In the NFIB data, the first 10 years of irrelevant experience lowers profitability by 10.3 percent of the wage in the first week and by 15.5 percent of the wage after six months on the job. In the EOPP data, five years of irrelevant experience lowered profitability by 12 percent of the wage in the first three months and by 3 percent of the wage at the time of the interview. Older workers who lacked relevant work experience were less profitable new hires partly because (a) their higher reservation wages (better alternatives at other firms) forced employers to pay them more and (b) employers tend to expect older workers to be more productive than they turn out to be.

Productivity Surprises

Evidence on this last issue can be found in table 6.9, which presents a regression model predicting the discrepancy between realized productivity at the end of six months and the respondents' expectation of that productivity at the time the individual was hired. Our hypothesis that expectations were generally rational is supported by the prevalence of insignificant coefficients and the insignificance of the F-test for the model as a whole. But there are exceptions. Employers were pleasantly surprised by the productivity of workers with relevant work experience and unpleasantly surprised by the productivity of those with irrelevant work experience. These findings support our hypothesis H_3. Profitability can be predicted by *relevant* work experience, because many employers were not aware of the relevance of the new hire's previous work experience until long after the hiring decision. Since total work experience is easy to measure prior to hiring, the combined effect of the two variables should have been foreseen by employers, but since the two variables are strongly correlated, a positive coefficient for relevant experience in the model predicting the productivity surprise tends to cause the coefficient on total work experience to become negative.

6.3.2 Spillovers from Employer Training

We will now compare the impact of previous relevant training on wage rates with its impact on productivity. Holding total experience constant in the EOPP data, starting wage rates were 6.4 percent higher for those with five years of

3. Note that the effect of five years of relevant experience which is not offset by a decline in irrelevant experience is obtained by adding the predicted effect of a simultaneous increase in both relevant experience and total experience. Alexander's (1974) analysis of longitudinal data on earnings from social security files and Hollenbeck and Wilke's (1985) analyses of 1983 CPS data obtained similar results. Holding the amount of experience at the firm constant, past experience in one's current industry or occupation had larger positive effects on earnings than experience in other industries or occupations.

Table 6.9 **Surprise and Actual Productivity at Six Months**

Variable	Surprise Productivity at Six Months (actual minus expected)		Actual Productivity at Six Months		Current Wage Minus Alternative Wage	
Previous employer training						
Relevant experience	.015**	(2.53)	.029***	(5.23)	.0003	(.21)
Relevant experience squared	−.0003	(1.47)	−.0006***	(3.11)	−.00007	(1.25)
First-year's relevant experience	−.044	(1.36)	−.027	(.86)	.0004	(.06)
Formal training on job	−.046	(1.48)	−.009	(.30)	−.005	(.61)
Formal training off job	.042	(.75)	.066	(1.21)	−.012	(.83)
Schooling						
Years of schooling	.044	(.52)	.013*	(1.79)	−.0037**	(2.01)
Relevant public vocational training	−.011	(.31)	−.031	(.85)	.005	(.55)
Relevant private vocational training	.080*	(1.90)	.123***	(2.98)	−.010	(1.01)
Relevant training from military	.096*	(1.92)	.106**	(2.15)	−.013	(1.10)
Relevant training from JTPA	.089	(.99)	.157*	(1.70)	−.031	(1.45)
Years of occupational training	.003	(.23)	.005	(.45)	−.004	(1.29)
Total experience	−.0076**	(2.14)	−.0079**	(2.30)	.0012	(1.40)
Total experience squared	.0001	(1.03)	.00009	(.99)	−.00002	(.89)

(*continued*)

Table 6.9 (continued)

Variable	Surprise Productivity at Six Months (actual minus expected)		Actual Productivity at Six Months		Current Wage Minus Alternative Wage	
Demographic background						
Female	−.005	(.13)	.007	(.20)	.006	(.70)
Married female	.039	(1.39)	.024	(.90)	−.0004	(.06)
Married male	.002	(.08)	−.005	(.22)	.013**	(2.19)
Black	−.028	(.56)	−.065	(1.32)	−.0067	(.57)
Hispanic	.002	(.04)	−.049	(.90)	.005	(.41)
Temporary job	.004	(.11)	.008	(.23)	−.0128	(1.56)
F-test on model	1.38		3.79***		.53	
R^2	.028		.068		.038	
RMSE	.35		.36		.08	
N	937		872		872	

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

relevant experience. The additional pay seems to be considerably smaller than the benefit—a 44 percent increase in productivity net of training costs during the first three months—the firm derives from hiring a worker with five years of relevant experience. This hypothesis was tested by defining for each new hire a measure of relative profitability—productivity net of the wage and training costs—during the first three months and then analyzing how worker characteristics influence profitability of the new hire. The definition of the profitability variable is described in Appendix B. Hiring workers with five years of relevant experience reduces losses, or increases profits, during the first three months by an amount equal to 30 percent of the typical new hire's productivity net of training costs (see row 13 of table 6.7).

Holding total experience constant in the NFIB data, 10 years of relevant work experience increased starting wage rates by 22 percent and current wage rates by 24.6 percent. It also increased profitability by 32.8 percent of the wage in the first week and by 3.2 percent of the wage at the end of six months. Both of these effects are significant at the 5 percent level on a one-tail test or better. Clearly the firm benefits when it is able to hire workers trained by other firms.

How long does this spillover benefit last? Five years of such experience is apparently associated in the EOPP data with an increase in the profit margin, at the time of the interview, that is equal in magnitude to 3.3–3.9 percent of the worker's potential productivity and associated in the NFIB data with a decrease by an equivalent amount.[4] Neither of these effects is statistically significant, however. The spillover benefit of hiring relevantly trained workers diminished with tenure, apparently approaching zero after a year or so. In addition, turnover is lower for workers who had relevant work experience, though here again the finding is not statistically significant. These results suggest that firms hiring workers with relevant experience retain for themselves much of the greater productivity and lower training costs of these workers during the first few months on the job. Since members of the sample had fewer than two years of tenure at the time of the interview, it is not possible to say what happens to the relative profitability of experienced and inexperienced hires in the third and subsequent years at the firm.

Formal Training

The NFIB survey also has data on formal training received on and off the job. Formal training received on the job from a previous employer has no effect on starting wage but increases initial productivity by 9.5 percent of the wage

4. The measure of profitability at the interview date was obtained by subtracting proportionate differences in wage rates from proportionate differences in productivity. Differences in the costs of training the worker were not measured beyond the first three to six months, so this variable captures only part of the variations across people in their current profitability to the firm. The positive effects of relevant training and experience on profitability are probably understated as a consequence.

and reduces training requirements by 17.3 percent. It has no effect, however, at the time of the interview.

Formal training received off the job, on the other hand, has no initial effect on anything, but it increases the index of suggestions by 37 percent and current productivity by 15.9 percent. Formal off-the-job training does not increase current wage rates however, so profitability increased by 13.8 percent of the wage at six months of tenure and by 18.6 percent of the wage at the time of the interview.

These results suggest that OJT sponsored by firm A not only benefits the employee and the employer (as implied by Becker's theory of OJT), but also sometimes benefits other employers in the industry, who hire workers who quit or are laid off by firm A. In other words, OJT often creates externalities—benefits that are not appropriated by either the trainer or the trainee. Formal off-the-job training generates substantial long-lasting externalities, and the informal training captured by the relevant experience variable appears to generate externalities only in the first year or so of a worker's tenure at a firm. The market failure that is implied by this finding appears to justify some modest governmental efforts to stimulate the externality-creating activity—general OJT in general and off-the-job employer-sponsored training in particular. The lack of long-term data on the magnitude of spillovers is a problem however, for it is always possible that the profits generated in the first year or two by hiring an experienced/trained worker are offset by losses in the out years. Clearly, more research on the issue is needed.

6.3.3 Effects of Vocational Education

The proportion of new hires who are reported to have received relevant occupation-specific training from a school is quite high: about 20 percent in the EOPP data and 37 percent in the NFIB data. The effect of this school-based training on performance outcomes is summarized in table 6.10 for EOPP data and in table 6.11 for NFIB data.

Effect of Vocational Training from Public Institutions—EOPP Data

New hires who received *relevant* vocational training required smaller amounts of OJT and were more productive in their first few months on the job. Analysis of the EOPP data set (not reported here) found that employees who have had vocational training that is *not relevant* to the job were slightly less productive in the first two weeks and required slightly more training than people who have had no vocational training.

The impact of relevant vocational training varied considerably by level and by provider. Consequently, the EOPP analysis offers separate estimates of the effects of training received at private and at public institutions and of the effects of training received by workers with different levels of schooling (a high school diploma or less, some college, and a 4-year-college degree or higher). The impacts of relevant vocational training received at a public institution are

reported for each of the three categories of educational attainment in columns 1–3 of table 6.10. The additional impact of receiving one's training at a private institution is reported in column 4. The impact of an additional four years of schooling is reported in column 5.

The EOPP data suggests that the effects of relevant vocational training were largest for those with one to three years of college. It increased productivity in the first two weeks by 13 percent, reduced management training time by 35 percent, and reduced overall training time by 22 percent. Vocational training at these institutions appears to have increased tenure slightly though not significantly. Overall productivity net of training costs during the first three months increased by a significant 22 percent. Starting wage rates were a significant 8 percent higher. The fact that productivity net of training cost increased more than wage rates implies that for those with one to three years of postsecondary education, vocational training benefits the employer as well as the new hire. The magnitude of the spillover benefit during the first three months is estimated to be 16 percent of productivity net of training costs, in the EOPP data. This estimate is not significantly different from zero, however, and the point estimate was very close to zero by the time of the interview.

Vocational training obtained in high school apparently has smaller effects on productivity, training requirements, and wage rates than vocational training obtained at two-year postsecondary institutions. The difference is statistically significant for initial productivity, for informal training by management, and for starting wage rates. College graduates with vocational training get significantly more training than other vocationally trained workers in the same job, but, in other respects, are not significantly different from those with only some college. Their overall productivity net of training costs during the first three months is no higher than that of workers with no vocational training.

By the date of the interview, however, the productivity advantage of workers with vocational training from a public institution, over others in the same job, had greatly diminished.

Effect of Vocational Training from Public Institutions—NFIB Data

Vocational training at public institutions has no statistically significant effects on performance outcomes in the NFIB data.

Training from Private Vocational/Technical (Voc/Tech) Institutions

High productivity and significant reduction in training costs result from hiring employees who have been trained at privately controlled voc/tech schools or colleges. Compared to students who received their vocational training at public institutions, privately trained students are 20 percent more productive, initially, in the EOPP data, and 7 percent more productive at the time of the interview and require 20 percent less training. Their overall productivity net of training costs during the first three months is 22 percent higher. In the EOPP

Table 6.10 **Effects of Relevant School-based Vocational Training in EOPP Data (%)**

Outcomes	Vocational Training with 12 or Fewer Years of School	Vocational Training with Some College	Vocational Training with Four or More Years of College	Extra Impact of Private Vocational Training	Impact of Four Years of General Education
Productivity net of training costs during first three months	7	22**	0	22*	1
Productivity					
First two weeks	3*	13**	3	20***	0
Next twelve weeks	2	4	4	7	2
At time of interview	3	1	−10	7	5*
Required training					
Formal training	−9	25	73	−37	−10
Informal by management	−8*	−35***	−19	−9	8
Informal by co-workers	4	−26	−2	−36*	24**
Total training	−9	−22**	12**	−20**	3
Wages					
Starting	10***	8***	2	4	0
At time of interview					

Profitability of hire during first three months	6	16	−17	16	−5
Productivity minus wage (at time of interview)	1	1	−4	2	0
Turnover					
Tenure	−6	10	11	7	−4
Quit	−18	10	29	−7	−21
Discharge or layoff	23	−24	−54	−34	33

Note: Fixed effects regressions run on 435 pairs of new hires in the 1982 EOPP Employer Survey for all models included control variables for whether currently a vocational education student, was hired in a temporary job, was known to be eligible for a subsidy when hired, and current average hours per week. Models for current or most recent wage, productivity, and profitability have additional controls for actual tenure and tenure squared. Models for starting wage and profitability in the first three months control for date of hire and for log of potential tenture and its square. In cols. 1 and 3 the significance levels report on a hypothesis test of differences between the effect of high school (col. 1) or four-year college (col. 3) vocational training and the effect of vocational training received at a community college or technical institute.

*Significant at the 10 percent level (two-sided).

**Significant at the 5 percent level (two-sided).

***Significant at the 1 percent level (two-sided).

Table 6.11 **Effects of School-based Vocational Training in NFIB Data (%)**

Outcomes	Relevant Public Vocational Training	Private Vocational Training	Relevant Military Training	Relevant JTPA Training	Years of Vocational Training	Years of Schooling
Productivity						
End of first week	2.0	10.0**	−3.2	8.0	1.5	1.0
End of six months	−3.1	12.3***	10.6**	15.7*	.5	1.3*
Current	4.5	10.3**	9.8	15.4	−2.1	2.8***
Suggestions	2.7	2.3	10.1	−6.0	4.7	7.8***
Required training	6.5	−4.0	24.4**	11.1	−2.5	.6
Wage rates						
Starting	1.5	7.1***	−0.4	0.0	1.1	1.6***
Current or most recent	1.4	4.8	−6.6*	−10.6	1.9*	1.6***
Expected productivity	−1.4	4.4**	3.9	7.7*	0.5	0.7*
Surprise	−1.1	8.0*	9.6*	8.9	0.3	0.4
Profitability						
End of first week	3.2	−0.9	2.6	−12.7	−0.9	−3.0**
End of six months	0.8	−0.1	−0.6	12.8	−1.5	−1.2
Current or most recent	5.3	−7.6	11.8	1.7	−3.8*	1.4
Turnover						
Leave	−11.8	−11.9	5.8	−40.5	−1.5	−0.8
Quit	−30.6	17.2	20.0	−8.1	−1.2	7.1
Discharge or layoff	17.5	−54.0*	−4.3	−90.4	−2.2	−9.4

Note: Col. 1 is the estimated effect of relevant vocational training obtained at a public institution. Col. 2 presents the additional effect of obtaining training at a private voc/tech institution. Col. 3 presents the additional effect of receiving relevant training from the military. Col. 4 presents the additional effect training obtained through the Job Training Partnership Act. Col. 5 presents the effect of the length (in years) of vocational training. Percentage effects for required training and wage rates are anti-logs of ten-year effects calculated from logarithmic models for training and wage rates. The suggestions index ranges from 0 to 3 and has a mean of 1.027.

*Significant at the 10 percent level.

**Significant at the 5 percent level.

***Significant at the 1 percent level.

data their starting wage rates are only 4 percent higher, so the firm benefits significantly when it is able to hire a graduate of a private voc/tech institution.

In the NFIB data, hiring a graduate of a private voc/tech institution has a smaller effect on training requirements but a larger effect on initial and current productivity and wage rates. The wage increase roughly corresponds to the productivity benefit so profitability is not significantly affected by hiring a graduate of a private voc/tech school.

Military Training

About 3.8 percent of the new hires had received relevant training in the military. These workers typically must receive extra training from their new employer, but at six months they are 10.6 percent more productive than workers who have been trained at a public voc/tech institution, and at the time of the interview they were 9.8 percent ($p = .105$) more productive. Since they typically received below average pay at the time of the interview, they are probably very profitable hires in the long run. Employers appear to be surprised by the performance of those with training from the military.

JTPA Training

Only 2 percent of the new hires in the sample had received training funded by the Job Training Partnership Act. This was too small a number to produce findings which are statistically significant at conventional levels. Nevertheless, point estimates tell a fascinating and very positive story. In a previous paper (Bishop 1989) analyzing EOPP data, I found evidence that disadvantaged individuals who participate in TJTC and CETA were stigmatized by the signal of their disadvantaged status generated by their participation in these programs and consequently performed better than they were expected to. Analysis of data on JTPA trainees yields similar results; JTPA graduates started out 8 percent more productive ($p = .47$ on a two-tailed test) than other vocationally trained workers and received/required 10 percent more training ($p = .56$), but they were 15.7 percent more productive ($p = .089$) at six months and 15.3 percent more productive ($p = .165$) at the time of the interview and are 12 percent ($p = .224$) less likely to be fired. Despite the positive impacts on productivity, current wage rates were 10 percent below ($p = .129$) that of the other occupant of the job. As in the earlier study, these findings suggest that standard evaluations which focus on the wage and earnings outcomes of programs like JTPA and CETA are biased by the stigma generated by signaling the trainee's disadvantaged status and thus substantially understate the social benefits of such training.

Years of Schooling

In the EOPP data, additional years of schooling generally did not have statistically significant effects on initial productivity, required training, and turnover but were related to receiving more informal OJT from co-workers. Schooling

is, however, positively related to productivity at the time of the interview in both data sets and to expected productivity in the NFIB data. These results contradict the claims of Ivar Berg (1971) in *Education and Jobs: The Great Training Robbery.* The fact that years of schooling have zero impact on initial productivity but a significant impact on productivity after a year suggests that schooling helps the individual learn the job.

Schooling is also positively related to both starting and current wage rates. Since starting wages respond positively to schooling even though initial productivity does not, years of schooling is negatively related to profitability in the first quarter in the EOPP data and to profitability in the first week in the NFIB data. With time, however, this effect disappears. Schooling has no effect on profitability at the end of six months or at the time of the interview.

6.3.5 Demographic Characteristics

The productivity, training requirements, and turnover of black and Hispanic employees are not significantly different from those of other employees. Black employees receive the same wage rates. Hispanic employees are, however, paid significantly less—12–13 percent less—than others at the same firm. Why? The profitability models provide a clue, for in these estimations Hispanics are not significantly more profitable for the firm. Hispanics did require some additional training and were about 5 percent less productive than other workers, but the differences were not statistically significant. When a profitability variable is constructed, these factors offset the lower wage, and the result is that the reduced wage appears to be justified by productivity differences. The alternative explanation of this phenomenon is hypothesis H_2 from section 6.2.1. It suggested that undocumented workers of Hispanic background have almost no bargaining power when they negotiate with employers about wages and thus are paid less. This hypothesis receives no support in this data set.

Gender and marital status have no significant impact on productivity or tenure. In the NFIB data, single women require/receive 13.9 percent less training than single men, but married women require/receive 16 percent more training than married men. The big gender differences are in wage rates. Wage rates for single and married women are significantly below the wages received by married men. Relative to married men, single women are paid 12 percent less, both at the start and at the time of the interview, and married women are paid 14–16 percent less. Single men fall in the middle. This appears to be a result of differences in bargaining power and reservation wages. There are no significant effects of gender and marital status on the surprise component of productivity realizations, and point estimates of the gender-related surprises are small. Consequently, we can rule out the hypothesis that the lower wages were caused by employers having expected women to do a poorer job (see table 6.9). The profitability findings are similar to the wage findings, and the differences between married men and women are highly significant. In the NFIB data, single and married women generate substantially higher profits for their em-

ployers than do married men. The additional profit from hiring a single women rather than a married man is 22 percent of the wage in the first week and 15–16 percent of the wage after six months and at the time of the interview. The additional profit from hiring a married women rather than a married man is 19 percent of the wage in the first week and at six months and 23 percent at the time of the interview.

In summary, the analysis finds no support for a bargaining power (more commonly called a discrimination) explanation of lower wages for blacks and Hispanics. Blacks do not receive lower wages than others in the same job, and the lower wages received by Hispanics appear to be due to lower productivity and higher training costs. The analysis offers support, however, for the bargaining power hypothesis as an explanation of differences between married men and both single and married women.

6.4 Summary and Conclusions

This paper examined the relationship between the prior training of new hires and their productivity, training costs, wages, and turnover, for two new employees hired for the same job. Nonrandom selection for and retention in these jobs makes unbiased structural estimates of underlying population relationships between background characteristics and productivity infeasible. Analysis of the reduced-form relationships, however, reveals a significant tendency of new hires with relevant previous work experience, relevant employer-sponsored formal training, and relevant vocational education (particularly when obtained from a private voc/tech institution) to require less training, to be more productive, and to be paid higher wages both initially and after one year.

The paper also provides a unique test of the rationality of the expectations which are the basis of wage offers to new hires and of the competitiveness of these labor markets. The rationality of expectations about the future productivity of new hires was tested by measuring and then predicting the surprise in productivity realizations—the difference between relative productivity at six months and employer expectations of that productivity held at the time the hiring decision was made. Worker characteristics known to hiring-decision makers at the time of hiring had no significant relationship with the surprise in productivity realizations. The R^2 of the model was only 2.8 percent. On the other hand, worker characteristics which are frequently not known to hiring-decision makers when they are hiring—e.g., the relevance of previous work experience—were significantly related to the surprise in productivity realizations.

The paper also examined the efficiency and competitiveness of the market for new hires. If expectations are rational and the market competitive, we would expect that (a) wage differentials for visible worker traits would approximate productivity differentials and (b) ex post profitability of new hires would not be predictable by information that is generally available to hiring-decision

makers. This hypothesis must be rejected, however, because a number of significant predictors of the ex post profitability of new hires were uncovered. Prior relevant work experience and formal off-the-job training were positively associated with the profitability of the new hire, and total potential work experience and being a married male were negatively related to profitability. These results suggest that many employers are not aware of the exact character of the training and experience their new hires bring to their firm and, consequently, that new hires who have training and experience from previous jobs often do not receive commensurately higher wage rates. This suggests that training sometimes generates third-party externalities when trainees do not stay with the employer who trains them. If this conclusion is correct, modest governmental efforts to stimulate general OJT and employer-sponsored formal off-the-job training would appear to be in order.

Appendix A
The EOPP Employer Survey and the Measurement of Training and Productivity Growth

The analysis is based on data from a survey of 3,412 employers sponsored by the National Institute on Education (NIE) and the National Center for Research in Vocational Education (NCRVE) conducted between February and June 1982. The survey was the second wage 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 10 EOPP pilot sites and 18 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 in proportion to the estimated number of low-wage workers at the establishments in that strata. Within strata the selection was random. The survey was conducted over the phone and obtained a response rate of 75 percent.

The second wave attempted to interview all of the respondents from the first-wave survey. About 70 percent of the original respondents completed surveys for the second wave. Most of the respondents were owners/managers of small firms who were quite familiar with the performance of each of the firm's 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, that part of the interview was completed by talking to a supervisor or someone else with line responsibility.

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 in the study.

The respondent was asked to report how much time typical new hires for this job spent, 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 co-workers. For the sample of firms and jobs, the means for the typical worker were 47.3 hours watching others do the job (T_W), 10.7 hours in formal training programs (T_F), 51 hours in informal training by management (T_S), and 24.2 hours in informal training by co-workers (T_C) (relevant portions of the questionnaire appear in Bishop et al. 1983).

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 provided formal and informal training were assumed to be paid 1.5 times the wage of co-workers with two years of tenure. Formal training involves both the trainer's and trainee's time. Sometimes such training is one-on-one, and sometimes it is done in groups. It was assumed that the average ratio of trainees to trainers was four 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 time of trainees engaged in formal training was assumed to have a value of eight-tenths of an experienced co-worker's time. When supervisors and co-workers give informal training to a new employee, the trainee is almost invariably directly involved in a production activity. Employers report that for informal training, trainees are typically as productive while being trained as they are when working alone (Hollenbeck and Smith 1984). Consequently, informal training is assumed to involve only the investment of the trainer's time.

Appendix B
Measures of the Profitability Differentials for New Hires

Estimates of differentials in the ex post profitability of the two new hires were made by combining the data on their wage, productivity, and training costs differentials. Because EOPP data is 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 the EOPP data, the formula for profitability differential at the time of the interview was

$$(A1) \qquad Y^{C}_{1j} - Y^{C}_{2j} = [(P^{C}_{1j} - P^{C}_{2j})/P^{2Y}_{j}] - \ln(W^{C}_{1j}/W^{C}_{2j}).$$

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

$$(2A) \qquad Y^{S}_{1j} - Y^{S}_{2j} = [(P^{S}_{1j} - P^{S}_{2j})/P^{2Y}_{j}] - [(T^{S}_{1j} - T^{S}_{2j})/520] - [(W^{S}_{1j} - W^{S}_{2j})/W^{2Y}_{j}],$$

where

Y^{S}_{ij}, Y^{C}_{ij} = profitability (excluding any tax credits) of the ith 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^{2Y}_{j}, W^{2Y}_{j} = productivity index and wage of the typical worker in job j with two years of tenure (2Y); 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^{2Y}_{j}, the productivity differential $(P^{S}_{1j} - P^{S}_{2j})$ 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 $(W^{S}_{1j} - W^{S}_{2j})$ 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 $P^{2Y}_{j} = W^{2Y}_{j}$. This implies that the third term need not be multiplied by an adjustment factor be-

fore being subtracted from the terms describing productivity and training differentials.

In the 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

$$(A3) \qquad Y_{1j}^{1W} - Y_{2j}^{1W} = [(P_{1j}^{1W} - P_{2j}^{1W})/P_j^{6M}] - [(T_{1j}^{1W} - T_{2j}^{1W})/40] - (W_{1j}^{S}/W_{2j}^{S}) + 1,$$

$$(A4) \qquad Y_{1j}^{6M} - Y_{2j}^{6M} = [(P_{1j}^{6M} - P_{2j}^{6M})/P_j^{6M}] - [(T_{1j}^{6M} - T_{2j}^{6M})/960] - (W_{1j}^{S}/W_{2j}^{S}) + 1,$$

$$(A5) \qquad Y_{1j}^{C} - Y_{2j}^{C} = [\ln(P_{1j}^{C}/P_{2j}^{C})] - [(T_{1j}^{6M} - T_{2j}^{6M})/960] - [\ln(W_{1j}^{C}/W_{2j}^{C})],$$

where

T_{ij}^{1W} = hours spent by person i in training during the first week;
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 between P_j^{6M}, W_j^{S}, and W_j^{C} (such as $P_j^{6M} = 1.4W_j^{C}$) will not change the statistical significance of the tests of the hypothesis that coefficient $\boldsymbol{\beta}$ in equation (1) is greater than zero. The tests of the profitability of hiring workers with relevant experience are, however, sensitive to these assumptions.

Appendix C
Means and Standard Deviations of NFIB Data

Characteristic	Mean Level	Standard Deviation of Level	Standard Deviation of Difference
Schooling			
Years of schooling	12.6	1.75	1.57
Years of relevant vocational training	.67	1.28	1.29
Relevant public vocational training	.217	.41	.38
Relevant private vocational training	.132	.34	.315
Relevant training from JTPA	.020	.14	.123
Relevant training from military	.037	.19	.25
Previous employer training			
Relevant experience	5.28	5.65	5.86
Formal training on job	.067	.25	.22
Formal training off job	.49	.50	.48

Characteristic	Mean Level	Standard Deviation of Level	Standard Deviation of Difference
Demographic background			
Age	28.86	9.26	10.76
Female	.40	.49	.34
Married	.54	.50	.64
Husband	.32	.47	.52
Black	.03	.18	.24
Hispanic	.03	.18	.20
Temporary job	.17	.37	.34
Hours worked per week	38.5	8.55	–
Years since hired	1.96	1.27	1.55
Tenure	1.29	1.27	1.51
Wage rates			
Starting wage ($)	6.46	4.25	–
Log starting wage differential	–	–	.281
Current wage ($)	7.52	4.43	–
Log current wage differential	–	–	.289
Premium over outside wage	.01	.09	.08
Productivity relative to mean at six months			
End of second week	.72	.37	.321
End of six months	1.00	.33	.338
Current or most recent	1.085	.35	.400
Suggestions (scale 0–3)	1.03	1.11	1.234
Surprise in six-month productivity	−.13	.28	.325
Log training time	4.06	1.24	.858
Profitability relative to mean productivity at six months			
Initial	–	–	.532
End of six months	–	–	.439
Current or most recent	–	–	.449
Turnover			
Leave	.288	.45	.628
Quit	.162	.37	.549
Discharge or layoff	.117	.32	.430

References

Alexander, Arthur J. 1974. Income, experience, and internal labor markets. *Quarterly Journal of Economics* 80 (1): 63–85.

Berg, Ivar. 1971. *Education and jobs: The great training robbery.* Boston: Beacon Press.

Bishop, John. 1989. Toward more valid evaluations of training programs serving the disadvantaged. *Journal of Policy Analysis and Management* 8, no. 2 (Spring): 209–29.

Bishop, John, John Barron, and Kevin Hollenbeck. 1983. *Recruiting workers.* Columbus: National Center for Research in Vocational Education, Ohio State University.

Brown, Charles. 1982. Estimating the determinants of employee performance. *Journal of Human Resources* 18 (Spring): 177–194.

Hollenbeck, K., and B. Smith. 1984. The influence of applicants' education and skills on employability assessments by employers. National Center for Research in Vocational Education, Ohio State University.

Hollenbeck, K., and R. Wilkie. 1985. The nature and impact of training: Evidence from the Current Population Survey. In *Training and human capital formation,* ed. John Bishop et al. Columbus: National Center for Research in Vocational Education, Ohio State University.

Hunter, John E., Frank L. Schmidt, and Michael K. Judiesch. 1988. Individual differences in output as a function of job complexity. Department of Industrial Relations and Human Resources, University of Iowa, June.

Kohn, M. L., and C. Schooler. 1983. *Work and personality: An inquiry into the impact of social stratification.* Norwood, N.J.: Ablex.

Mueser, Peter and Tim Maloney. 1987. Cognitive ability, human capital and employer screening: Reconciling labor market behavior with studies of employee productivity. Department of Economics, University of Missouri-Columbia, June.

Olson, Craig A. and Brian E. Becker. 1983. A proposed technique for the treatment of restriction of range in selection validation. *Psychological Bulletin* 93 (1): 137–48.

Thorndike, R. L. 1949. *Personnel selection: Test and measurement Techniques.* New York: Wiley.

This Page Intentionally Left Blank