1-1-1997

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Citation

Barron, John M., Mark C. Berger, and Dan A. Black. 1997. On-the-Job Training. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

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Kalamazoo, Michigan

Library of Congress Cataloging-in-Publication Data

Barron, John M.
On the job training / John M. Barron, Mark C. Berger, Dan A. Black.
p. cm.
Includes bibliographical references and index.
ISBN 0–88099–178–X (cloth : alk. paper). — ISBN 0–88099–175–5 (pbk. : alk. paper)
1. Employees, Training of—Evaluation. 2. Organizational learning. I. Berger, Mark C. II. Black, Dan A. III. Title.
HF5549.5.T7B2917 1997
658.3'12404—dc21 97–10295
CIP

W. E. Upjohn Institute for Employment Research
300 S. Westnedge Avenue
Kalamazoo, Michigan 49007–4686

Cover design by J. R. Underhill
Index prepared by Shirley Kessel.
Printed in the United States of America.

Acknowledgments

We would like to express thanks to the many individuals and organizations that have helped make this research possible. We wish to thank the W.E. Upjohn Institute for Employment Research for financial support for conducting this research. We also thank the U.S. Small Business Administration for financial support to conduct the SBA survey (Contract No. SBA-6640-OA-91). The SBA and Upjohn Institute Surveys were conducted ably by the Survey Research Center at the University of Kentucky. We especially thank Jim Wolf for his help with questionnaire design and Melissa Huffman for assistance in the initial processing of the survey data. We thank Kevin Hollenbeck for many useful comments and suggestions throughout the entire project and Don Parsons for his thorough review of the completed draft of the manuscript. We also received helpful comments from seminar participants at the Upjohn Institute for Employment Research. We appreciate the patience and endurance of Steve Allen and Amitabh Chandra, who read and provided comments on countless versions of various chapters. We thank Judy Gentry for her editing of the final manuscript.

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ON-THE-JOB TRAINING

CHAPTER 1

Introduction

In 1964, Gary Becker noted the important role of on-the-job training in *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education* by observing that:

> Theories of firm behavior, no matter how they differ in other respects, almost invariably ignore the effect of the productive process itself on worker productivity. This is not to say that no one recognizes that productivity is affected by the job itself; but the recognition has not been formalized, incorporated into economic analysis, and its implications worked out.... Many workers increase their productivity by learning new skills and perfecting old ones while on the job. Presumably, future productivity can be improved only at a cost, for otherwise there would be an unlimited demand for training (1964, p. 8).

In the decades following Becker's classic text, researchers have made substantial progress in achieving Becker's goal of fully incorporating the role of on-the-job training into economic analysis.

Researchers now widely accept that there are two key aspects of training. First, there is the recognition that on-the-job training is an important example of an "investment" in human capital.[1] Like any investment, there are initial costs. For on-the-job training, these costs include the time devoted by the worker and co-workers to learning skills that increase productivity plus the costs of any equipment and material required to teach these skills. Like any investment, the returns to these expenditures occur in future periods. For on-the-job training, these future returns are measured by the increased productivity of the worker during subsequent periods of employment.

The second key aspect of on-the-job training is the distinction between "general" and "specific" on-the-job training, a distinction emphasized by Becker in his early works. While all training increases the productivity of the worker at the firm providing the training, general training also increases the productivity of the worker at firms other than the one providing the training. For example, a secretary who learns the use of a standard word-processing program or a doctor who interns at a specific hospital both receive general training, as these skills are transferable to other workplaces. On the other hand, specific on-the-job training increases the productivity of the worker at the firm providing the training, but not at other firms. Resources spent orienting new employees to the practices of their new employer, or teaching employees how to contribute to a unique assembly process or work team, are examples of specific training.

Chapter 2 presents the standard theoretical framework for assessing the impact of on-the-job training on productivity, wages, and turnover. This sets the stage for our investigation in subsequent chapters of the magnitude and effects of on-the-job training, an investigation that focuses on three employer-based surveys of the training received by newly hired workers. We start by considering two questions: Exactly how much training do employers provide their workers? Who receives this training? Chapters 3 and 4 address these two issues: the extent of training and the characteristics of the recipients of on-the-job training. Our focus is on the extent of training provided to new workers during their first three months of employment. We find that a substantial amount of on-the-job training takes place at the beginning of a job, that most of this training is informal training, and that participation in this training depends on such variables as an individual's level of education and experience.

The findings reported in chapters 3 and 4 rely solely on employer-based surveys. This raises the issue of whether the patterns of on-the-job training reported by employers are similar to workers' perception of the extent of training. One way to examine this is to identify a particular position and compare the employer's response concerning the training involved with the responses of the worker who is the recipient of this training. An analysis of such a "matched" survey is the subject of chapter 5. We find substantial measurement error in the training variables, and also that firms tend to report more training than workers. But there appears to be no systematic variation in reporting errors based on firm or worker characteristics, and aggregate reported measures of the incidence of training are similar.

The theory of on-the-job training developed in chapter 2 involves several key predictions concerning the effect of training on the starting wage and on wage and productivity growth. Chapter 6 investigates the evidence supporting such predictions. We find that training does increase wage and productivity growth as anticipated, but there appears little evidence that training substantially reduces the starting wage as predicted.

Chapter 7 investigates evidence consistent with the possibility that there is a matching of positions with more training to more "able" individuals. To do so, we examine employer recruiting activities. Here, we find evidence of a systematic attempt by employers through their hiring activities to link high training positions to higher-ability workers. In short, employers do spend substantially more time searching for a new employee if the position to be filled involves greater training. These findings provide a rationale for our failure in chapter 6 to detect a strong, inverse relationship between the level of training and the starting wage. Chapter 8 summarizes all of our findings and offers some policy recommendations.

NOTE

1. Human capital investments have been classified as ". . . activities that influence the future money and psychic income by increasing the resources in people" (Becker 1964, p. 1). Human capital investments include not only schooling but also on-the-job training, migration, medical care, and searching for information about wages and prices. All these activities are engaged in at some cost and yield future returns, often in the form of higher wages.

CHAPTER 2

On-the-Job Training as an Investment in Human Capital

A 1992 survey by the Small Business Administration indicates that workers' starting wages in 1992 increased by 10 cents for each additional month of formal education. This finding is consistent with an extensive literature documenting the fact that firms pay higher wages to workers with greater formal education, consistent with the assumption that such workers are more productive. It is not clear, however, to what extent formal education can be viewed as an investment in human capital that directly increases a worker's productivity. An alternative view is that formal education acts as a signal—that more productive workers, because they can acquire additional formal education at a lower cost, do so to signal their higher productivity to employers. In this view, increased formal education does not increase the inherent productivity of workers, but it does reveal those workers who are more able.

It is not our intent to discern the relative importance of formal education as a human capital investment versus formal education as a signal of ability. Rather, we seek to focus on a different, less-studied type of human capital investment—on-the-job training. The potential impact of on-the-job training on worker productivity can be substantial. For instance, the SBA survey of employers that we later analyze extensively indicates that if an employer spends an additional month providing on-the-job training to a particular worker, that worker's hourly wage will rise by 6.5 cents. This 6.5 cent figure likely underestimates the actual increase in worker productivity that can be attributed to on-the-job training. The reason for this is that part of the return to on-the-job training is reaped by the employer as higher profits, as productivity increases by more than the wage paid.

The extent of on-the-job training varies widely across different occupations and industries. What patterns in terms of differences in wage and productivity growth, as well as hiring activity and turnover, should we expect to see across positions? To answer this question, we introduce the standard theory of the effects of on-the-job training, following our presentation of a simple training model. In doing so, we introduce the important distinction between "general" and "specific" on-the-job training. We also explore the implications of training for turnover.

On-the-job training is but one approach to explaining why wages grow with job tenure. This chapter reviews other theories that provide alternative explanations. The job-matching/learning approaches suggest that wage growth reflects the revelation of information concerning the productivity of particular workers assigned to particular tasks, not on-the-job training that increases productivity. The incentive-based thesis suggests that the higher wages paid to long-term employees indicates a long-term contract that incorporates appropriate incentives to minimize shirking by newly hired workers. The final section of this chapter provides an overview of the available evidence concerning the major predictions of on-the-job training theory.

A Simple Model of On-the-Job Training

When a worker is hired, there is a match between a particular position and a particular worker. Firms' positions differ with respect to a variety of factors including the extent of on-the-job training required, formal educational requirements, the capability of the employer to monitor workers' effort, and the safety or attractiveness of the workplace. Workers also vary widely with respect to such factors as innate ability, formal education attainment, and the propensity for future turnover.

To focus on the role played by on-the-job training, let us consider a simple situation in which a worker is hired by a firm for two periods. The worker comes to the firm with a level of general human capital acquired through formal education E and a level of ability denoted by the Greek letter α. A worker with no training but with education E and ability α has productivity in the first, or beginning, period of work denoted by the term $p(E, \alpha, 0)$, where the zero in this expression indicates that the worker has received no training by the current employer in prior periods. Naturally, increases in formal education or ability make the worker more productive. In terms of first derivatives of the productivity function, this means that $\partial p/\partial E > 0$ and $\partial p/\partial \alpha > 0$.

During the first period, the employer provides the total training to the worker denoted by the vector T. The training provided take two forms. General training, denoted by T_g, is training that increases the worker's productivity not only at the firm providing the training, but also at other firms. Specific training, denoted by T_s, is training that increases the worker's productivity only at the firm providing the training. Thus, the training vector $T = (T_g, T_s)$. Training increases the productivity of the worker at that firm in the future. In our simple example, the future is the second period. Thus, a worker with ability α and education level E who has received general training T_g and specific training T_s at the firm will have a productivity in the period "after" training given by:

$$(2.1) \quad f_a = p(E, \alpha, T)$$

where $\partial p/\partial T > 0$. We assume that increased ability α not only increases worker productivity, but also affects the return to training. In particular, it is assumed that $\partial^2 p/\partial T \partial \alpha > 0$. In words, the return to increased training is greater for more able workers (workers with a higher α).

Training increases future productivity, but it comes at a cost. Otherwise, as Becker notes, "there would be an unlimited demand for training." (1964, p. 9) One way of introducing costs of training is to view part of the output of a worker being consumed by the training activity. That is, output produced for sale is reduced as the worker takes time out to learn how to increase productivity by observing others. Training costs also include the lost output of co-workers and managers who take the time to show the new worker techniques for improving productivity.[1] Let $c(E, \alpha, T)$ denote the total training costs in terms of lost output that the employer incurs during the first period to provide training T to an individual with formal education E and ability α.[2] Naturally, $\partial c/\partial T$ is greater than zero, such that there are greater costs to increased training. The net productivity of a beginning worker during the training period is thus given by:

$$(2.2) \qquad f_b = p(E, \alpha, 0, 0) - c(E, \alpha, T).$$

If there were no training provided ($T = (T_g, T_s) = (0, 0)$), then training costs are zero i.e., $c(E, \alpha, 0, 0) = 0$.

As expressions (2.1) and (2.2) indicate, with no training the net productivity of the worker during the first and second periods of employment would be identical. If the worker receives some training (i.e., $T_g > 0$ or $T_s > 0$), then the productivity of this worker after training (f_a) is higher than his or her net productivity during the first period (f_b) for two reasons. First, there is the productivity enhancement of training in that $p(E, \alpha, T) > p(E, \alpha, 0, 0)$ if $T_g > 0$ and/or $T_s > 0$. Second, there is the cost of training $c(E, \alpha, T)$ in terms of the reduced contribution to output of the worker during training along with reduced output by co-workers.

Let w_b denote the "beginning" wage paid the worker during the first period and w_a denote the wage paid a worker during the second period of employment, "after" training. Let q denote the probability a worker quits the employer at the end of the first period of employment. With probability $1 - q$, the worker remains at the employer for a second period of employment. Then the employer's expected net present value to hiring a worker and providing training levels T_g and T_s during the first period of employment, NPV, is given by:

$$(2.3) \qquad \text{NPV} = f_b - w_b + \beta\,(1 - q)\,(f_a - w_a)$$

where β is the discount factor ($1 > \beta > 0$). The second term in expression (2.3) indicates that with probability $1 - q$, the worker does not quit, and the firm reaps the net return $f_a - w_a$ from the trained worker during the second period of employment.

We start by comparing the wages paid beginning and trained workers. Competition across employers in the form of the creation or destruction of various positions means that, in equilibrium, all three types of positions will have zero net present value to employers. Setting NPV, as defined by expression (2.3), equal to zero, wages satisfy the following zero profit condition:

(2.4) $w_b + \beta\,(1-q)\,w_a = f_b + \beta\,(1-q) f_a\,.$

Substituting (2.1) and (2.2) into (2.4), and noting that total training, T, is equal to the vector of general and specific training, (T_g, T_s), the zero profit condition implies the following two-period wage function for newly hired workers:

(2.5) $w_b + \beta\,(1-q)\,w_a = p(E, \alpha, 0, 0) - c(E, \alpha, T_g, T_s)$
$+ \beta(1-q)\,p(E, \alpha, T_g, T_s)\,.$

The next two conditions place restrictions on the wage paid to trained workers. The first condition, a result of competition among employers for trained workers, requires that employers pay an after-training wage w_a at least as great as the trained worker's potential contribution to output at other firms.[3] Otherwise, such workers will be bid away. As the potential contribution of workers at alternative employers depends on the extent of general, but not specific, training, we thus have the following expression for the wage paid to workers after the training period:

(2.6) $w_a \geq p(E, \alpha, T_g, 0)\,.$

Condition (2.6) indicates that competitive forces provide a lower bound to the wage an employer pays a trained worker.

There are other forces that impose an upper bound on the after-training wage w_a. Specifically, the employer will have an incentive to unilaterally dismiss a trained worker if the wage paid the trained worker in the second period exceeds his or her productivity. Such moral hazard considerations on the part employers imply that an incentive-compatible wage agreement will require a second period wage that does not exceed the worker productivity. That is, we have the following conditions concerning the wage paid a trained worker:

(2.7) $w_a \leq p(E, \alpha, T_g, T_s)\,.$

The danger of an employer paying a wage in the second period below that previously agreed upon is not considered. It is assumed that the potential for the worker to sabotage production in retaliation against such actions is a sufficient deterrent.

By invoking conditions (2.5) - (2.7), we can identify reasons for differences in wages across various positions that differ solely in the extent and type of training. For instance, let w_o denote the per-period wage for a worker in a position that offers no training and w_{ag} denote the wage paid a worker after receiving general training T_g but no specific training. Conditions (2.6) and (2.7) imply that w_o will exactly equal the worker's productivity in the position, $p_a(E, \alpha, 0, 0)$, while w_{ag} will exactly equal $p_a(E, \alpha, T_g, 0)$. Thus, the difference in the second-period wage paid to workers of identical ability α and education E across these two positions is:

$$(2.8) \quad w_{ag} - w_o = p(E, \alpha, T_g, 0) - p(E, \alpha, 0, 0) \equiv r(E, \alpha, T_g) > 0$$

where $r(E, \alpha, T_g)$ is the gross return in terms of increased productivity from an investment in general training T_g by a worker with ability α and formal education E.

If the worker reaps the entire return to general training, then from the zero profit condition (2.5) it follows that the worker must bear the entire cost of general training. In other words, the first period wage of workers who receive general training, w_{bg}, is reduced by the cost of providing the training. That is,

$$(2.9) \quad w_o - w_{bg} = c(E, \alpha, T_g) > 0$$

where $c(E, \alpha, T_g)$ is the total cost in terms of lost output from an investment in general training T_g by a worker of ability α with education E.

Equations (2.8) and (2.9) illustrate the well-known prediction that workers reap all the returns to general training and bear all the costs of such training. What is not indicated is whether the return to general training, fully captured by workers through higher wages after training, more than compensates workers for the costs in terms of the reduced wages received during the training period. That is, we have not yet placed any restrictions on the net gain to an individual investing in the level of general training T_g. For an individual of ability α with formal education level E, the net value to such an investment is:

$$(2.10)\quad V_g(E, \alpha, T_g) = \beta\, r(E, \alpha, T_g) - c(E, \alpha, T_g).$$

Clearly there are differences across workers in terms of ability α and differences across positions in terms of training requirements. Assuming that V_g is increasing in ability, it follows that workers will be sorted across positions according to ability, with the higher ability workers assigned to positions with increased training.[4] An equilibrium in which some positions offer general training while others do not then requires that there exist a marginal worker with level of ability α_{mg} and formal education E_{mg} who will be just indifferent between the two positions. For such a worker:

$$(2.11)\quad V_g(E_{mg}, \alpha_{mg}, T_g) \equiv \beta\, r(E_{mg}, \alpha_{mg}, T_g) - c(E_{mg}, \alpha_{mg}, T_g) = 0\,.$$

For this worker, the fact that the present value of the return to general training exactly matches the cost of such training implies that the two-period return to accepting a position with general training T_g is identical to a position that offers no training.

With regard to specific training, the restrictions on the wage paid to trained workers as represented by conditions (2.6) and (2.7) do not, by themselves, determine who bears the costs of, or reaps the return to, specific training. For the moment, let us assume that workers reap a constant fraction δ $(1 \geq \delta \geq 0)$ of the total return to specific training in the form a higher wage after training. If the worker receives the fraction δ of the return to specific training, then the difference between the wage w_{as} paid to a worker after receiving only specific training T_s and the wage w_o paid a worker with no training is:

$$(2.12)\quad w_{as} - w_o = \delta\, [p(E, \alpha, 0, T_s) - p(E, \alpha, 0, 0)] \equiv \delta\, r(E, \alpha, T_s) > 0\,.$$

As with general training, the zero profit condition across positions that vary in training implies that if a worker reaps some of the return to specific training, the worker must bear some of the costs in terms of a reduction in the beginning wage. From equation (2.12) and the zero profit condition (2.5), for workers who receive the fraction δ of the return to specific training, the cost of training in terms of a lower wage equals:

$$(2.13) \quad w_o - w_{bs} = c(E, \alpha, T_s) - (1-\delta)\,\beta\,(1-q)\,r(E, \alpha, T_s)\,.$$

Equations (2.12) and (2.13) illustrate the well-known point that if workers receive a greater fraction of the return to specific training (a higher δ), they will bear a greater cost in terms of a reduction in the starting wage. Naturally, for a given $\delta < 1$, both a lower quit rate q or greater return to specific training $r(E, \alpha, T_s)$ will reduce the training costs borne by the worker as each change increases a firm's reward to providing the training. This reflects the fact that the overall net return to specific training, given by:

$$(2.14) \quad V_s(E, \alpha, T_s, q)\,/ \equiv (1-q)\,r(\alpha, E, T_s) - c(\alpha, E, T_s)$$

depends inversely on the quit rate.

Traditional Predicted Effects of Training on Wages, Productivity, and Turnover

Below we summarize the above discussion of on-the-job general and specific training with respect to the commonly cited implications for wages and productivity. We then turn to the implications of specific training for turnover. With respect to the effect of general and specific training on the pattern of wages, we have the following two propositions.

Proposition 1: Comparing a position that offers general on-the-job training with one that offers no training, if workers of similar ability and level of formal education were to be employed in both positions, then the following predictions hold:

- The wage paid after training to a worker in the position offering the general training would be higher to reflect the increased productivity of the trained worker at the employer offering the training as well as at other employers (see condition (2.8)).
- The starting wage for a worker at the position that offers the train-

ing would be lower to reflect the cost of training (see condition (2.9)).

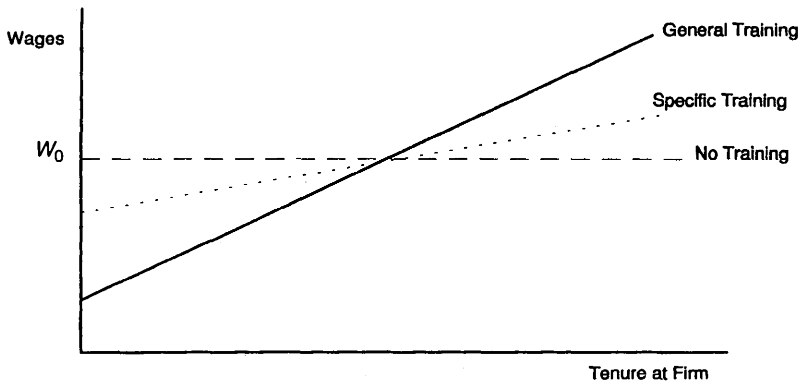
- The growth in wages and productivity for a worker at the position offering general training would exceed that of a worker at the position not offering such training (see conditions (2.8) and (2.9)).
- The growth in wages and productivity for a worker who starts in a position offering general training would be identical whether the worker quits after training to work for another employer or remains with the employer (see conditions (2.6) and (2.7)). Thus, controlling for the effect of work experience on wage and productivity growth, there would be no additional effect on wages and productivity growth for workers who have a longer tenure at a particular firm.

Proposition 2: Comparing a position that offers specific on-the-job training with one that offers no training, if workers of similar ability, level of formal education, and quit propensity were to be employed in both positions, then the following predictions hold:

- The wage paid to workers after training would be higher to reflect the increased productivity of the trained worker at the employer offering the training (see condition (2.12)). The extent of the increase will depend directly on the sharing rule for total returns (δ) and total return to the investment (see condition (2.12)).
- The starting wage for a worker at the position that offers specific training would be reduced if the worker (a) reaps a greater share δ of the return to such training, or (b) has a higher quit propensity q (see condition (2.13)).
- The growth in wages and productivity for a worker who starts in a position offering specific training would be lower if the worker quits after training to work for another employer. The lower growth occurs because the worker forfeits his or her share of the return to the investment in specific capital. Thus, controlling for the effect of work experience on wage and productivity growth, specific training suggests an additional effect on wages and productivity growth for workers with a longer tenure at a particular firm.

Figure 2.1 illustrates the potential wage profiles for individuals with no training, with a given amount of training that is all general, and with the identical level of training that is specific. For specific training, it is assumed that the worker and firm share both the cost to and return from the training. This explains the less steep wage profile for the specific training compared to an identical level of general training. Either type of training, however, provides a rationale for an upward sloping wage profile.

Figure 2.1 Effect of Training on Wage Profiles



Wage Equations Implied by the On-the Job Training Model

Propositions 1 and 2 suggest the estimation of the following wage equations for beginning and trained workers:[5]

(2.15) $\ln (w_b) = b_0 - b_1 \ln T_g - b_2 \ln T_s + b_3 \ln \alpha + b_4 \ln E - b_5\, q + \varepsilon$

and

(2.16) $\ln (w_t) = e_0 + e_1 \ln T_g + e_2 \ln T_s + e_3 \ln \alpha + e_4 \ln E - e_5\, q + \varepsilon$

where b_i , $e_i > 0$, i = 0, ... , 5. Propositions 1 and 2 imply that, controlling for individuals' levels of ability (α), formal education (E), and quit propensities (q), a cross section of beginning workers' wages should reveal the pattern of lower starting wages at positions with more training, with a greater negative impact on the starting wage for general training, as workers bear the entire cost of such training (i.e., $b_1 > b_2$).[6] Conversely, a cross section of trained workers' wages should reveal the pattern of higher wages at positions that offer more training, again with general training having a greater impact (i.e., $e_1 > e_2$).

Often measures of differences in training across positions are not directly available. One alternative approach to test the theory of on-the-job training, an approach that circumvents this lack of data, uses data on the lengths of time a worker has been in the labor market and with a particular employer as proxies for the extent of general and specific training. The argument is that, as training takes place over time, the extent of general training should be directly correlated with the length of time an individual has been in the labor force, or what is generally referred to as his or her length of work "experience." Similarly, with regard to specific training, a greater length of time a worker has remained at a particular employer, or what is generally referred to as his or her "tenure" at an employer, is interpreted as indicative of a worker who has acquired greater specific training. As we discuss later in this chapter, these proxies have been used to test for the predicted returns to training.

The Implications of Specific Training for Turnover

A unique aspect of specific training as an investment in human capital is that, for such an investment to pay off, an employer and employee must continue in their employment relationship. Equation (2.14) illustrates this by noting that the net return to specific training, V_s, increases with a reduction in the quit propensity. In a more general model, not only the propensity of workers to quit but also the propensity of employers to terminate the employment relationship through a unilateral discharge influences the net return to specific training. Such turnover, whether initiated by employer or employee, imposes costs on the other party when the costs and returns to specific training are shared ($1 > \delta > 0$). There are at least two ways to minimize the adverse effects of turnover decisions on the joint return to specific training: contract choice and the sorting of workers across positions based on their quit propensities. We consider contract choice first.

To minimize the adverse effects of turnover decisions, an optimal employment contract will seek an arrangement in which the individuals—workers or employer—who make turnover decisions after training—quit or discharge—consider the entire lost return to specific training arising from a termination of the employment relationship. For instance, let's say that the sole source of turnover is quit decisions by workers who discover new, more attractive alternatives. If one were to restrict the analysis to the appropriate sharing rule (i.e., optimal choice of δ), then the optimal sharing rule would be to set δ equal to one. In this case, the worker contemplating whether or not to quit would bear all the costs, in terms of the forgone return to specific training, if the decision were to quit.[7]

If one were to consider more flexible contractual forms, then the optimal contract would specify that a worker who quits must compensate the employer for any lost return to specific training. Similarly, an employer who discharges a worker must compensate the worker for any lost return to specific training. Such payments are not uncommon. For instance, the practice of granting severance pay to a dismissed worker can be interpreted as a contingent terminal payment that forces employers to compensate workers for lost returns to specific training. Similarly, the fact that, if workers quit shortly after receiving training, they give up future pension payments or paid vacations, illustrates terminal payment by workers to employers that compensates the employer for the lost returns to specific training.

The above discussion leads to the following proposition concerning the nature of contracts that arise at positions with specific on-the-job training.

Proposition 3: Comparing a position that offers specific on-the-job training with one that offers no training, if workers of similar ability and level of education were to be employed in both positions, then the following predictions hold:

- If workers are to receive new information on the value of alternatives to continued employment, it is optimal for workers to bear at least part of the cost and reap at least part of return to specific training as higher posttraining wages. The outcome is a wage for the experienced worker that exceeds that available from other employers. As a consequence, the worker will be less likely to quit. This result is strengthened if optimal contingent terminal payments (e.g., vested pension plans, paid vacation days based on seniority) are considered.

- If employers are to receive new information on the value of alternatives to continued employment, it is optimal for employers to bear at least part of the cost and thus reap at least part of the return to specific training as posttraining wages below the productivity of the worker. As a consequence, the employer will be less likely to discharge the worker. This result of a "quasi-fixed" labor input is strengthened if one considers optimal contingent terminal payments (e.g., severance pay).[8]

A second way to minimize the adverse effects of turnover decisions on the joint return to specific training involves the sorting of workers across positions based on their quit propensities. For instance, let's presume that the labor market is populated with two types of individuals. Stayers (*S*) have a low-quit propensity, q_S, while movers (*M*) have a high-quit propensity, q_M, with $q_M > q_S$. For positions that differ in the extent of general training alone, this difference in quit propensities would be irrelevant, as quit propensities do not affect the return to such an investment.[9] As the expression for the net return to specific training indicates (expression (2.14)), however, individuals with a lower-quit propensity will find such positions more attractive. To the extent that the employer shares in the costs of and return to specific training, employers as well will place a greater value on individuals with the lower-quit propensity.

The fact that the joint gains to specific training are greater for workers with the lower-quit propensity implies a sorting of workers. To see why, let's assume that there are two types of positions, those that require no specific training and those that require substantial specific training. Further, let us assume that employment contracts are initially allocated randomly across the two types of workers (those with high-quit propensity q_M and those with low-quit propensity q_S). Given such an allocation, the wage profiles of the two types of positions are such that expected wages and profits are identical across the two positions for the mean quit propensity, with $q_M > \bar{q} > q_S$. In this case, the stayers would prefer to be located in positions with specific training, as they would be more likely to reap a return to the training in the second period that takes the form of a higher wage. Conversely, the movers would prefer the positions with no specific training. Similarly, employers filling the positions that require specific training would prefer the stayers, as they would then be more likely to reap their portion of the return to specific training. Search and screening activities by both workers and employers to induce such a sorting of workers provide us with the following proposition.

Proposition 4: Comparing a position that offers specific on-the-job training with one that offers no training, the sorting of workers of similar ability and level of formal education but differing quit propensities provides the following predictions:

- Positions with specific on-the-job training would be populated with individuals who have a lower inherent propensity to quit. As a consequence, we would observe less turnover (quits) in positions that require more specific training.
- Workers with low-quit propensities would on average be more productive in the labor force (e.g., they would be equally productive in positions requiring no specific training but more productive in positions with specific training). As a consequence, their compensation should be greater than that of high-quit propensity individuals.

The importance of the above propositions is suggested by Lazear and Rosen (1988), Kuhn (1993), and Barron, Black, and Loewenstein (1993). These papers, among others, consider the sorting of women into jobs with low turnover costs (e.g., positions with low specific training) that can arise if turnover rates are inherently higher for females than males. If women have a weaker attachment to the labor force, then as proposition 4 indicates, efficiency, and hence labor market equilibrium, requires that women be assigned to jobs in which turnover is less costly. Becker (1985) suggests that this sorting of women into such jobs that offer less training may reflect explicit decisions by women to take such positions because of their specialization in home production and weaker labor force attachment. Along similar lines, O'Neill (1985) argues that part of the gender wage gap is created by women's preferences for part-time work and flexible work schedules.

Thus, the traditional model of on-the-job training provides predictions concerning the starting wage, wage growth, turnover, and even gender differences in the labor markets. At this point, however, it is worth noting the informational assumptions necessary for this model. First, both firms and workers must be able to agree on before-training productivity (f_b) and after-training productivity (f_a). Obviously, firms have an incentive to understate the productivity of a worker before the receipt of training so as to lower the starting wage. In addition, both firms and workers must agree on what are the costs of training (our $c(\cdot)$ function). Unfortunately for the parties, there is no market that will efficiently provide a price quote for the training services. Again, firms would appear to have an incentive to overstate the cost of training to lower the starting wage.

Perhaps more important, workers and firms must agree on what training is general and hence should be funded by workers, and what training is specific, for which the firms and workers should share the investment. Clearly, firms would like to describe all the training as general training, and workers would like to specify all the training as specific. The division of training between specific and general may not be as obvious as it first seems. Much training may be specific to an industry, and if that industry's employment is declining, should we count it as general training? When the market for trained workers is thin, it becomes difficult to determine what fraction of the training is truly general. In addition, because the gains to specific training are a function of the likelihood of job turnover, workers and firms must agree to the probability of job turnover. Finally, because labor contracts seldom explicitly determine the wage profile, workers and firms generally agree to an "implicit contract." Such implicit contracts inherently cannot rely on third-party enforcement mechanisms to insure that both parties honor their commitments and hence are often difficult to enforce.

Other Models of Compensation, Productivity, and Turnover

On-the-job training models provide important insights into patterns of wages, productivity, and turnover. An investment in training today raises a worker's future productivity and consequently his or her future compensation. As we have seen, since the initial contributions of Becker (1962) and Oi (1962), economists have drawn a key distinction between general training, which has value at alternative firms, and specific training, which has value only at the firm offering the training. There are, however, alternative interpretations of the observed wage patterns that are predicted by on-the-job training. This section briefly considers two such alternatives: learning/job matching models and incentive-based compensation models. As we examine the extent and impact of on-the-job training, we must keep in mind the role these theories could play. Otherwise, we may incorrectly attribute wage growth or productivity growth to on-the-job training when such growth actually reflects these other phenomena.

Learning/Job Matching Models

The human capital literature stresses the productivity-enhancing effects of on-the-job training. As this training is acquired during the first period of employment, it is predicted that productivity and wages will be directly correlated with experience. It has been suggested that other activities unrelated to traditional human capital investment also occur during the first period of employment and imply similar outcomes. Specifically, employers gather information concerning a new workers' ability during the initial period of employment. Similarly, workers gather information concerning the nonpecuniary benefits of an employer. As discussed below, the acquisition of information can affect wage growth and task assignment. The key assumption of these learning/job matching models is that information on the value of a match between an employer and new hire increased over time rather than productivity growth due to training. Below we consider three types of information that can be acquired over time.

The first type of information acquisition is known as the "learning model." In such models, the employer acquires information on the true ability of the worker. Those who are identified as high ability are rewarded by an increase in wage, for the employer seeks to reduce the likelihood of turnover by such individuals. Thus, the wages of some workers will rise over time as these workers are identified as the higher ability workers. Others will experience a decline in wages. This prediction is distinct from human capital theory. For human capital theory, wages do not fall with tenure; zero training implies identical wages across time, while positive training implies rising wages. In contrast, if information revelation on ability occurs during the first period of a worker's employment, then wages can fall for those workers revealed to be below average. In fact, Farber and Gibbons (1991) find that real wage declines do occur. They estimate that as many as 20 percent of workers experience a real wage decline with experience on the job.

The "job matching" literature focuses on a second type of information acquisition. Unlike the learning model, the information acquired does not reveal the productivity of the worker at other firms. Rather, each employer regards all prospective employees as identical *ex ante*. In other words, the realized value of a match between any given worker with any given employer can be viewed as a random variable drawn from a common distribution. In some of the models (e.g., Johnson (1978), Viscusi (1979), Lippman and McCall (1981), and Holmlund and Lang (1985)), the realized value depends on information the worker gathers during the first period of employment about working conditions and other non-pecuniary aspects of the employment relationship. In other models, e.g., Jovanovic (1979b), the realized value of a match between a firm and a worker is the discovered productivity of the worker. If the additional information suggests that the joint value to the match is a good one, the worker remains at the firm. If the realized joint value includes in part a good draw in terms of productivity, the worker who remains with the firm will receive a higher wage, for only the more productive matches continue.

A third type of information acquisition by employers during the initial period of employment concerns the tasks for which the new employee has a comparative advantage (that is, the tasks for which the new employee is the low-cost producer). As discussed by Barron and Loewenstein (1985), such information will allow the employer to efficiently assign workers across tasks. The ability to assign workers efficiently is valued by employers as it reduces production costs. Thus, employers will pay workers whose abilities they have identified higher wages to discourage turnover. Here it is possible that all identified workers will receive higher wages, as each could be equally capable at the task for which they have a now-identified comparative advantage.

Incentive-based Compensation Models

There are numerous examples of incentive-based compensation models. A key feature of such models is that employers cannot readily identify an employee's work effort. For example, there might not be a clear link between observable current output and the worker's effort, which is not directly observable. If it takes time to discover the extent of shirking by a worker, then an optimal compensation scheme would delay payment until it is revealed that the worker provided appropriate effort. If it turned out that the worker had shirked, termination of the employment agreement would deny the worker these anticipated large payments toward the end of his or her tenure at the firm. This could create a powerful incentive for workers to provide substantial work effort during the early periods of employment. Lazear (1979) describes this incentive-based compensation scheme as follows:

> By deferring payment a firm may induce a worker to perform at a higher level of effort. Both firm and worker may prefer this high wage/high effort combination to a lower wage/lower effort path that results from a payment scheme that creates incentives to shirk. Thus, it may pay the firm and worker to set up a scheme such that the worker is paid less than his marginal product when he is young and more than his marginal product when he is old to compensation (p. 1264).

There are several pieces of evidence that suggest that incentive-based compensation models can complement training models in providing an explanation for wages rising with tenure at a particular employer. First, as Lazear notes, they are consistent with the institution of mandatory retirement, as older workers lack incentives to provide work effort and are paid a wage above their marginal product as a reward for providing substantial work effort in their youth. Second, as noted by Lazear and Moore (1984), the age-earning profile is less steep for self-employed workers, and these are the workers for whom the problems in inducing the appropriate level of effort do not exist.

Evidence Concerning the Standard Predictions of On-the-Job Training

Tests of the predictions of the theory underlying on-the-job training have typically taken one of two approaches, depending on the availability of data. The first, and more common, approach is adopted when direct measures of on-the-job training are not available. As discussed above, by making the assumption that the extent of general on-the-job training varies directly with the time in the labor force (labor market experience) and that the extent of specific on-the-job training varies directly with time on the job (job tenure), economists have relied on measures of labor market experience and job tenure to proxy for on-the-job training. The second approach to testing the theory of on-the-job training relies on explicit measures of training.

Inferring Training from Wage Data

In his path-breaking research, Mincer (1974) established the standard specification for the effect of on-the-job training earnings over the life-cycle. Mincer assumed that on-the-job training investment was directly related to work experience, and suggested that individual (log) earnings appeared to be a quadratic function of experience.[10] This reasoning leads to the following specification for the typical wage equation that includes both a worker's labor market experience and job tenure:

$$\text{(2.17)} \quad \ln(w) = \beta x + \phi_1 (\exp) + \phi_2 (\exp)^2 + \gamma_1 (\text{ten}) + \gamma_2 (\text{ten})^2 + \varepsilon$$

where x is a vector of control variables that includes measures of worker demographics and formal education, exp is the worker's total labor market experience, ten is the worker's tenure at the firm, ε is the error term, and β, ϕ's, and γ's are parameters to be estimated.[11]

We may estimate equation (2.17) using standard regression analysis and claim that the ϕ's provide a measure of the returns to general human capital while the γ's provide some evidence about the returns to specific training. To see why, consider the experiment of two workers who have just completed their first year in the labor market: John and Carol. John has just left his previous employer, so while his experience is one year (exp = 1), his tenure at his current employer is zero (ten = 0). Thus, John's expected log wage is:

$$\text{(2.18)} \quad E(\ln(w)) = \beta x + \phi_1 + \phi_2$$

because John now has one year of labor market experience, but his tenure at his current employer is zero. In contrast, Carol's expected wage is:

(2.19) $E(\ln(w)) = \beta x + \phi_1 + \phi_2 + \gamma_1 + \gamma_2$

because she has both one year of experience (exp = 1) and one year of tenure (ten = 1).

If jobs offer general on-the-job training, both John and Carol will earn more after they have spent one year in the labor market (that is, $\phi_1 + \phi_2 > 0$). In addition, if jobs offer firm-specific training and if workers reap at least a portion of the returns to that specific training, Carol will earn an additional premium for her tenure at the firm ($\gamma_1 + \gamma_2 > 0$). Because John has left his previous employer, any firm-specific skills that he may have picked up no longer increase his productivity and hence no longer have any impact on his wage. By examining the difference in the returns to Carol's labor market experience and John's labor market experience, we can identify the increase in wages due to firm-specific training. By examining the difference in John's wage to a worker without any previous experience (or if available, comparing John's wage a year ago to his wage today), we can identify the increase in wages from general human capital.[12]

The above approach to estimating the impact of on-the-job training has a long history. Although the approach is simplistic, the data seem to support many of its implications. First, literally hundreds of studies find that wages increase with labor market experience, as one would expect if on-the-job training increases worker productivity and the extent of on-the-job training is directly related to the span of time in the labor force. Second, the literature also finds substantial increases in earnings when job tenure increases, suggesting that the increase in worker productivity from specific training is also, at least partly, reflected in the wages paid. Moreover, as the theory suggests, diminishing returns to experience and tenure with respect to their impact on wages is typically found.

Holzer (1990a, 1990b), however, cites recent evidence of mixed support for OJT theory's claim that increased worker experience and tenure raises wages by increasing worker productivity.[13] One may question whether the finding that wages are significantly correlated with experience and tenure measures of on-the-job training is a true test of on-the-job training theory. There is, we believe, a sample selection problem. This notion, which dates back in economics to Roy (1951), emphasizes the non-experimental nature of most economic data.[14] Consider the experiment with John and Carol that we described above. Our interpretation is valid if John's decision to leave his employer were a random event. In economics, however, it is often thought that agents' decisions are not random; they instead are the outcome of rational agents attempting to maximize their utilities subject to the appropriate constraints. John may be unmotivated and may find it difficult to hold a job while Carol may be a highly motivated, loyal employee. Unfortunately, most data sets do not provide researchers with the necessary data to measure such differences. One way to model such unobservable characteristics is to assume they are a part of the error term, or

$$\varepsilon_{jt} = \eta_j + u_{jt} \tag{2.20}$$

where ε_{jt} is the error term for the *jth* worker (in our example, John or Carol) and t is the time period. The term η_j represents the individual's "fixed effect," because it does not change over time while u_{jt} is the standard error term that varies each period. If we replace the error term in equation (2.17) with this more detailed error term, we can easily see some of the statistical problems that this selection problem generates. If the worker's η is correlated with his or her tenure and labor market experience, OLS provides biased and inconsistent estimates of the parameters ϕ's and γ's. For instance, suppose that η measures the worker's motivation and suppose that more motivated workers are more productive (hence earn higher wages), more likely to stay at the current employer, and more likely to remain in the labor market. In this case, the ϕ's and γ's will be upwardly biased; we will be mistakenly ascribing the returns of the worker's motivation to training.

We can avoid this problem if we have panel data that provide repeated observations on the wage of individual workers. For instance, one strategy would be to focus on the difference in wages between two years, or from equation (2.17):

$$\begin{aligned} \Delta(\ln(w)) = {} & \beta\,\Delta x + \phi_1\,\Delta(\exp) + \phi_2\,\Delta(\exp)^2 + \gamma_1\,\Delta(\text{ten}) \\ & + \gamma_2\,\Delta(\text{ten})^2 + \Delta u \end{aligned} \tag{2.21}$$

where the Δ refers to the difference of the variable between time t and $(t-1)$. Equation (2.21) is independent of the η's because we have removed them by "differencing" the data. If a variable does not change over time, we cannot identify the corresponding parameter.

Estimation of such fixed-effect models does eliminate the potential bias previously identified, but such estimation is clearly impossible in cross-sectional data. Fortunately for the human capital model, panel data sets have confirmed the presence of large experience and tenure affects on wages. As our discussion of the matching literature in the previous section suggests, however, fixed-effect models may not be sufficient to assure a clean test of on-the-job theory. To see why, consider a slight generalization of equation (2.20):

$$(2.22) \qquad \varepsilon_{jt} = \eta_j + \varphi_{ji} + u_{jt}$$

where φ_{ji} is a match-specific error term between the *jth* worker and the ith firm.[15]

As the matching literature suggests, workers can differ in their productivities because of idiosyncratic differences in the matches among workers and the firms. For instance, a worker may complement the unique skills of existing workers, or, conversely, a worker may not get along with current employees. The presence of this added match-specific effect, however, creates a major problem in the estimation of the returns to tenure. If a worker is well matched (has a large η_{ji}), he or she is more likely to remain at the firm. Thus, OLS estimation of equation (2.21) will result in biased estimates of the γ's because workers who have remained with their current matches have higher φ's than workers who leave. Moreover, as Jovanovic emphasizes, we might expect the value of φ to be learned over time, which further complicates the estimation. While there have been numerous studies that attempt to control for these matching considerations, their results remain controversial. See Garen (1988) for a review of that literature.[16]

While the job-matching argument does provide a challenge to the on-the-job training model in the interpretation of the returns to tenure, the job matching model does not challenge the interpretation of the returns to experience. The large return to labor market experience would appear to be good evidence of the returns to on-the-job training, but the relative importance of firm-specific training would appear very much in doubt. Moreover, we are unable to test the prediction that on-the-job training lowers the starting wage from this indirect measure of training. Clearly, a direct measure of training would be useful.

Evidence from Direct Measures of Training

Until recently, one of the key difficulties in testing on-the-job training theory has been the lack of explicit information on training activities. As Brown (1990) observes, "obtaining information on the extent of training of the workforce is complicated both by conceptual problems and by difficulty in actually measuring those aspects of training that seem relatively well-defined" (p. 98). There now exist data sets that offer a variety of direct measures of various types of on-the-job training.

Lynch (1992), Levine (1993), and Brown (1989) consider whether the observed positive correlation between wages and tenure can be interpreted as the return to on-the-job training. With regard to this issue, the results are mixed. If measures of training are included in wage equations, such measures (a) have no effect on the estimated returns to tenure according to Lynch, (b) have some effect on the returns attributable to tenure according to Levine, or (c) account for almost all the returns to tenure according to Brown. With regard to the predicted negative effect of training on turnover, Mincer (1988) reports that training and turnover are indeed inversely related. In contrast, however, Levine (1993) finds no evidence that establishments with high levels of training have low levels of turnover.

There appears to be more agreement concerning the impact of training on productivity and wage growth. For instance, Mincer (1989b) reports that the "range of estimates (on the rate of return to training) based on several data sets generally exceeds the magnitude of rates of return usually observed for schooling investments." (p. 20) Holzer (1990b) and Bartel (1992) find that training increases performance as well as wage growth. Booth (1993) also establishes that some types of employer- provided training affect earnings, although training is generally found to be greater and more portable across jobs for men than women.

Differences that do exist in the above analysis of the effects of on-the-job training can be attributed to at least two factors. One is that the various studies cited differ substantially in the measures of on-the-job training. Some training measures derive from worker surveys, other from surveys of firms. Some studies represent a national survey, while others focus on a single large firm. Some studies focus on formal training measures, while others include informal training activity as well. A second factor that can explain differences in results across studies is the importance of confounding hypotheses. For instance, Kaestner and Solnick (1992) suggest that the upward-sloping wage/tenure profiles attributed to on-the-job training-induced productivity differences may instead reflect the deferred payment scheme suggested by Lazear (1979, 1981). Simon and Warner (1992) view their analysis of the relationship among wages, experience, and job tenure as support for Jovanovic's job-matching model, not the on-the-job training model. Finally, Barron, Black, and Loewenstein (1989) suggest that the matching of high ability workers to positions with high training can bias the estimated impact of training on wages.

In light of the above discussion, a key contribution of the chapters to follow will be to provide thorough analyses of the various effects of on-the-job training that (a) rely on a common set of on-the-job training measures across data sets, and (b) attempt to control for other hypotheses that can confound estimations of the predicted effects of on-the-job training.

NOTES

1. Training costs can be broadly defined to include the "hiring costs" associated with resources devoted to interviewing and screening potential new employees, as well as losses due to the position being vacant during this hiring process.

2. The training cost function includes the worker's formal education and ability, as either could influence the cost of additional training.

3. Our discussion ignores any "search" costs associated with workers locating alternative employers. In addition, our analysis also assumes that other employers know at zero cost the extent of general training received by a worker.

4. The increase in the return to training for more able individuals reflects our prior assumption that $\partial^2 p/\partial T_g \partial\alpha > 0$.

5. The term "ln" stands for the natural logarithm of the variable.

6. As we discuss in more detail below, estimation of these two wage equations is not as clear-cut as it may first appear. Difficulties arise from the assumption that ability affects the return to training. For instance, this assumption suggests that higher ability individuals will be matched to positions with greater training, such that differences in measured training be closely related to differences in worker ability. The estimated coefficient on training may then capture not only the return to training but also compensation for increased ability. If for some reason workers of differ-

ent ability are assigned to positions with the same amount of training, so that training and ability are not perfectly correlated, other issues arise. For instance, the assumption that the return to training depends on ability then suggests the inclusion of an ability/training interaction to capture the effect of ability on marginal return to training.

7. Hashimoto (1981) considers this problems.

8. The view that specific training can result in labor being a "quasi-fixed" factor of production was first emphasized by Oi (1962), who coined this term.

9. This statement assumes that a difference in the quit propensity of two workers simply reflects a difference in the likelihood a worker changes employers. However, the outcome of some quits is that the worker exits the labor market. If a higher-quit propensity reflects a reduced likelihood of continued participation in the labor force, then the return to general training, which is reaped only by those who remain in the labor force, would be lower for those with a higher-quit propensity.

10. The quadratic form allows for decreasing returns to experience, indicated by a negative coefficient on the squared term. Recently, Murphy and Welch (1992) show that a quadratic specification tends to understate wage growth early in workers' careers. While they recommend the use of quadratic specifications, we will for convenience of exposition continue to use the quadratic specification.

11. In cross-sectional studies, Mincer suggested that researchers use age minus years of schooling minus six as a proxy for experience, with the six subtracted to account for the first six years of life when the individual is not in school and with schooling subtracted to reflect that the individual is (presumably) and working full time when enrolled in school.

12. Our informal presentation of this material belies the considerable theoretical underpinnings of the wage equations that labor economists estimate. See Mincer (1974) for a rigorous justification of the standard wage equations.

13. For instance, Hanushek and Quigley (1985), in probing the relationship between wage growth and investments in on-the-job training, find that the restrictions imposed on wage growth by OJT are not supported for substantial portions of the labor force.

14. Roy was concerned with worker's nonrandom selections of jobs and the implications for the distributing of wages.

15. As in Willis and Rosen (1979), this also could be a match-specific parameter between the jth worker and the ith job.

16. Lazear (1979, 1981) also notes that presence of hours restrictions and mandatory retirement provisions seems inconsistent with the basic human capital model. He argues that upwardly sloped wage profiles result, at least in part, from incentive contracts that firms design to avoid having workers shirk.

CHAPTER 3

Measures of On-the-Job Training

For many years, economists seeking to test the theory of on-the-job training presented in chapter 2 have relied on proxy variables such as job tenure and labor force experience to measure the extent of on-the-job-specific and general training. In the past fifteen years, however, data sets with direct measures of training from both employees and employers have become available. In this chapter, we focus on employer-provided data. The first section introduces two employer training surveys. We then compare the employer-provided data of the 1982 Employment Opportunity Pilot Project (EOPP) and 1992 Small Business Administration (SBA) surveys with employee-provided data from other surveys.

Employer Survey Questions on Training

This section analyzes two of the three data sources used in this book: the 1982 Employment Opportunity Pilot Project survey and the 1992 Small Business Administration survey. The third data set, the 1993 Upjohn Institute Survey, is considered separately in chapter 5, as this data set asked both employers and employees about the training activities of newly hired workers. We start with a brief description of the training questions contained in the 1982 EOPP and 1992 SBA surveys.

The 1982 EOPP Survey

In 1980, the Department of Labor funded an extensive survey of employers to study the labor market effects of the Employment Opportunity Pilot Projects. This 1980 EOPP survey interviewed employers at

23 sites across the country; approximately 5,700 employers were involved in the survey. In 1982, the National Institute of Education and the National Center for Research in Vocational Education funded a follow-up survey of the employers who participated in the original 1980 EOPP survey. For the second wave, 70 percent of the original respondents completed surveys. The 1982 EOPP data set improved on the 1980 EOPP survey by obtaining more detailed information on the training activities of the most recently hired employee.

Researchers have used the 1982 EOPP data set extensively (e.g., Barron, Black, and Loewenstein 1987; Bishop 1990; Holzer 1990a, 1990b; and Holzer, Katz, and Krueger 1991). One key advantage of the survey is that it asked establishments detailed questions about the on-the-job training provided to the last worker hired at the establishment. In particular, the survey asked employers the following sequence of three questions:

> During the first three months of work, what was the total number of hours spent on formal training, such as self-paced learning programs or training done by specially trained personnel?
>
> During the first three months of work, what was the total number of hours management and line supervisors spent away from other activities giving informal individualized training or extra supervision?
>
> During the first three months of work, what was the total number of hours co-workers who are not supervisors spent away from their normal work giving informal individualized training or extra supervision?

In a different section of the questionnaire, the employer was asked:

> During the first three months of work, how many total hours does the average new employee spend in training activity in which he or she is watching other people rather than doing it himself or herself?

Answers to the above four questions provide information concerning four types of training. Answers to the first three questions provide the employer's measure of the number of hours of formal training, of informal training that management provided, and of informal training that nonmanagerial co-workers provided to the newly hired worker in the first three months of employment. The fourth question sought to measure the number of hours during the first three months that the worker spent watching others in order to learn how to do the job. A final training question asked by the survey, similar to the worker-based Panel Study of Income Dynamics measure of training, was:

> How many weeks does it take a new employee hired for (name's) type of position to become fully trained and qualified if he or she has no previous experience in this job, but has the necessary school-provided training?

Because it asks respondents to calibrate their responses to workers without any previous experience, this question provides an indication of the total human capital that the job requires from nonschooling sources. As such, it is a distinct concept from the actual training received on the job. For example, workers with considerable previous experience may become fully trained and qualified much more quickly than this response indicates. Because of this distinction, we refer to the answer to this question as the total human capital required for the job that the worker holds. This question allows us to control for differences in the requirements or complexity of jobs across workers in the EOPP and SBA surveys.[1]

The 1992 SBA Survey

In 1992, the Small Business Administration funded a survey to examine training at large and small firms. Survey Sampling, Inc. of Fairfield, Connecticut constructed the sample of businesses for this survey. Survey Sampling drew a stratified random sample of 3,600 businesses from the Comprehensive Business Database, oversampling large establishments to ensure statistically meaningful comparisons between large and small firms.[2] The authors designed the survey and the Survey Research Center (SRC) at the University of Kentucky conducted the survey in the summer of 1992.[3]

While based on the survey methodology of the EOPP data, the SBA-funded survey had several innovations. First, unlike the EOPP data set, the SBA data set did not oversample low-income workers, nor was it targeted only at sites where new government programs were planned. The survey also differed from the EOPP data in that the questions concerning the total hours of training over the first three months of employment were divided into separate questions concerning the average number of hours of training per week and the number of weeks of training. This approach allowed us to discover to what extent training was complete by the end of the first three months of employment. Finally, in addition to the EOPP's four measures of training (formal training programs offered by the firm on site, informal training by the worker's supervisor, informal training by co-workers, and time that the worker spent watching others perform tasks during the first three months), the SBA survey added a fifth measure, the number of hours spent at off-site formal training programs during the first three months of employment.

To check how the set of completed SBA surveys compares to the initial stratified national sample, we estimated a probit equation with the dependent variable equal to one if the establishment was in the sample, and zero otherwise. For independent variables, we used a set of one-digit industry dummies, Census region, a dummy variable indicating whether the establishment was located in a Metropolitan Statistical Area, and a vector of establishment-size variables.[4] Establishments from SIC code 7 (a portion of the service industry [10.2 percent in sample versus 16.6 percent universe]) and SIC code 5 (retail trade [30.8 percent versus 33.0 percent]) are somewhat underrepresented in our sample. Similarly, there are too few establishments from the Northeast Census region (13.7 percent versus 17.4 percent), and there are too few urban establishments (77.6 percent versus 85.1 percent). In addition, the probability of inclusion in our sample monotonically increases with the size of the establishment. However, the differences are not large, and one can conclude that the sample of firms that completed the survey is generally representative of the underlying national population of firms.

Employer Measures of On-the-Job Training

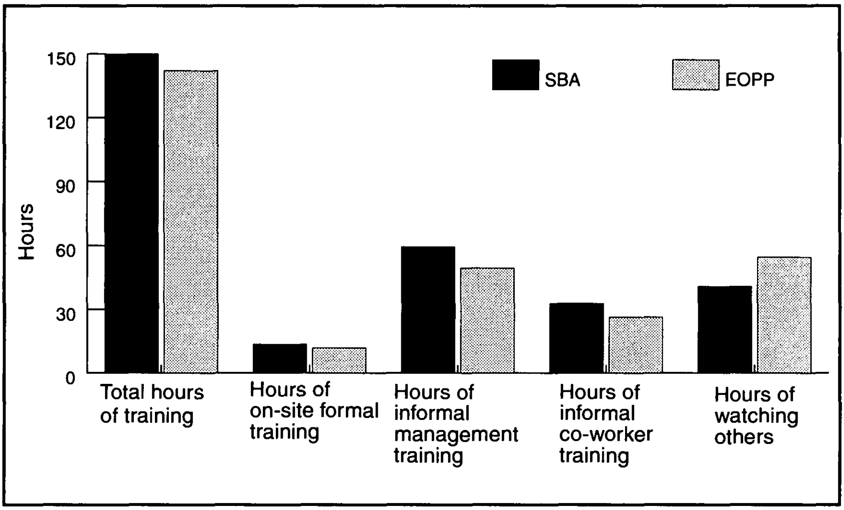
Table 3.1 reports the magnitude and incidence of training for the two surveys. Figure 3.1 illustrates the differences in the means of the various measures of training between the EOPP and SBA data sets. Note that the overall means are quite similar. For the EOPP data, newly hired workers receive about 142 hours of training in the first three months, with about 95 percent of all workers receiving some form of training; for the SBA data, newly hired workers receive about 150 hours of training with 98 percent of all workers receiving some training.[5] For the individual training measures, the off-site formal training (which is unique to the SBA) is a relatively minor form of training; average newly hired workers receive about three hours of this training while only about 7 percent of all workers receive this form of training. Thus, if we subtract the three hours of off-site formal training from the SBA mean, the two data sets indicate a remarkable degree of similarity.

Table 3.1 Means and Incidence Rates of Training Measures, 1992 SBA Data and 1982 EOPP Data

	SBA	EOPP
Total hours of training	149.9	141.9
Incidence rate	0.978	0.948
Hours of off-site formal training	3.4	---
Incidence rate	0.069	---
Hours of on-site formal training	13.6	11.9
Incidence rate	0.205	0.151
Hours of informal management training	59.4	49.3
Incidence rate	0.906	0.872
Hours of informal co-worker training	32.8	26.3
Incidence rate	0.605	0.628
Hours of watching others	40.7	54.5
Incidence rate	0.645	0.803
N	1,123	1,916

Within specific categories of training, table 3.1 reveals some differences in the average level and incidence of training. For instance, incidence of formal training is higher in the SBA data than in the EOPP data. In contrast, the EOPP reports more training by watching others than does the SBA data. For the EOPP, the newly hired worker has an average of about 54 hours with an incidence rate of 80 percent, but for the SBA data, the average number of hours is only about 41 hours with an incidence rate of 65 percent.

Figure 3.1 EOPP and SBA Measures of Average Hours Spent in On-the-Job Training During First Three Months



A comparison of means and incidence rates, however, can be somewhat deceiving. The length of training in the first three months can be a very skewed variable, and the mean is somewhat sensitive to large observations in the right tail of the distribution. Therefore, in table 3.2, we provide the mean, and the 5th, 10th, 25th, 50th, 75th, 90th, and 95th percentiles for each training measure conditional on the measure being positive. In each case, the mean of the distribution is considerably larger than the median of the distribution, indicating that the distributions are skewed to the right.

A weakness of both the EOPP and SBA data is their reliance on training received in the first three months of employment. We have just seen the importance of the right-hand tails of the data in generating differences in unconditional means in training between the SBA and EOPP data. To understand the significance of the three-month truncation of training, table 3.3 lists, for each type of training, the probability that a training spell lasts at least 12 weeks (both the unconditional

Table 3.2 Comparison of 1992 SBA and 1982 EOPP Training Measures

	Formal Training					Informal Training			
Percentile	**SBA** Off-site	**SBA** On-site	**EOPP** On-site	**SBA** Management	**EOPP** Management	**SBA** Co-worker	**EOPP** Co-worker	**SBA** Watching others	**EOPP** Watching others
5th	4	4	4	1	2	2	2	2	3
10th	4	6	6	4	4	3	4	4	5
25th	16	16	16	12	10	10	8	8	10
50th	30	35	40	30	30	21	20	24	32
75th	40	80	100	65	70	60	48	65	80
90th	120	180	200	160	120	120	100	160	160
95th	240	240	240	240	200	240	145	360	240
Mean	48.1	66.9	78.6	65.5	56.6	54.2	41.8	63.1	67.9
N	110	318	252	1,012	1,645	766	1,157	765	1,512

probabilities and the more revealing probabilities conditional on receiving the type of training) for only the SBA data. We cannot construct a similar table for the EOPP data because that set only contains information on the total number of hours, not length, of training. We choose 12 weeks because both 12 and 13 weeks are mass points. While a three-month period contains 13 weeks, we thought many respondents believed that period to contain 12 weeks. We refer to an incidence of training that lasts at least 12 weeks as a truncated incidence, although it is possible that the incidence would end exactly on the 12th week. From table 3.3, we see that 29 percent of training spells are truncated at three months. Conditional on receiving each type of training, the truncation rates are very high, with each measure having more than one in five cases truncated. Thus, the three-month frame of reference used by both the SBA and EOPP data appears to understate the training that newly trained workers received.

Table 3.3 Rates of Training Spells Lasting at Least 12 Weeks, 1992 SBA Data

Training measure	Overall rate	Conditional rate
Total training	0.291	0.298
Off-site training	0.016	0.230
On-site training	0.042	0.204
Informal management training	0.217	0.239
Informal co-worker training	0.157	0.259
Watching others	0.146	0.226

Finally, in table 3.4 we compare the reported time it takes to become fully trained and qualified. For the EOPP data, this measure of total human capital has a mean of 20.2 weeks, but the median is only 6.75 weeks. The SBA data set reveals a similar mean for total human capital of 22.2 weeks but a median of just 6 weeks. Again, both measures are highly skewed to the right. In sum, the two distributions of total human capital look very similar despite the ten-year difference in the time periods.

Another issue that could cloud comparisons between the 1982 EOPP data and the 1992 SBA data is the effect of the business cycle on training. The EOPP survey was conducted in the first half of 1982 just prior to the trough of the cycle in November 1982. The SBA survey was conducted in the summer of 1992, just over a year past the trough of the cycle in March 1991. The training data for the last worker hired precedes the date of the survey by at least 3 months by construction. Thus, much of the EOPP training data precedes the 1982 trough of the cycle by several months, and much of the SBA data is very close to the 1991 trough. It could be that training decisions of firms are different at the trough of the cycle from those before or after it. If so, comparisons made between the SBA and EOPP data could be affected. Unfortunately, the EOPP and SBA cannot be used to provide evidence on this question. However, tabulations by James Spletzer using the National Longitudinal Survey of Youth (NLSY) provide evidence on the incidence of formal training over the cycle that had its trough in 1991. Spletzer finds that the incidence of training is the lowest in 1991 at the trough of the cycle. Using a probit model, the probability of receiving training in 1990, one year before the trough, is only slightly higher.[6] This suggests that any contamination from business cycle effects is likely to be small when comparing the EOPP and SBA data.

Table 3.4 Length of Time to Become Fully Trained and Qualified, 1992 SBA Data and 1982 EOPP Data

Percentile	SBA	EOPP
5th	.55	1
10th	1	1
25th	2.25	2.8
50th	6	6.75
75th	24	22.1
90th	52	52
95th	104	94
Unconditional mean	22.2	20.2
Fraction nonzero	.991	0.969
N	1,193	1,921

In other words, the EOPP and SBA were both conducted close enough to the trough of a business cycle to avoid any contamination from business cycle effects. However, comparisons between the EOPP, SBA and surveys conducted further into an expansion or at the peak of the cycle may be affected. The Spletzer results run counter to the argument that as firms stockpile their most capable workers during a downturn, they increase their training activities. Of course, Spletzer's results are for the incidence of training and suggest that firms do not train more extensively during economic downturns, but they may train some workers more intensively. However, if his results hold for the provision of training in general, then the EOPP and SBA surveys would underpredict the incidence of training at other points in the business cycle.

Overall, the 1982 EOPP data and the 1992 SBA data give very similar answers on the provision of training. In some respects, these similarities of the data sets surprised us. The 1980s was a decade of fundamental change in the distribution of wages in the U. S. economy.[7] During that time, the distribution of wages became much more dispersed, and little overall growth in real wages occurred. For some groups—in particular, those below the median earnings of the economy—there was a decline in real wages. For others, especially workers in the upper quintile of the earnings distribution, there was a substantial increase in real wages for high-wage workers. Given the apparent importance of on-the-job training in wage determination, one may have anticipated a similar increase in the dispersion of on-the-job training. Comparing these two data sources, however, we find only limited evidence of such an increase in the dispersion of on-the-job training. The SBA data appear to have a somewhat greater dispersion for management training and co-worker training, and have a somewhat greater incidence of formal training than the EOPP data, although the differences are not substantial. One must bear in mind, however, that both data sets are truncated at the first three months of employment and may hide some important changes in the distribution of on-the-job training. Further, the sample for the EOPP data is not nationally representative.

Worker Measures of On-the-Job Training

Another approach to measuring on-the-job training asks employees about their training experiences. Obviously, most data will not have a collection of newly hired workers, so worker surveys will include a considerable number of workers with a significant amount of tenure. If these workers receive less training than newly hired workers, and if workers are asked about their training activities over a relatively short period of time, very low incidence rates for training are likely to occur. As a result, surveys of workers tend to ask very general training questions; however, one still finds very low incidence rates of training from worker surveys.

The first commonly used data on training was the Panel Study of Income Dynamics (PSID), which in 1976 asked a question similar to the EOPP and SBA question about the time to become fully trained and qualified. The PSID asked:

> On a job like yours, how long would it take the average new person to become fully trained and qualified?

Duncan and Hoffman (1978, 1979) were the first to analyze these data; they note that the instrument used the phrase "the average new person" rather than "you" in order to "minimize reported training differences due to skills or experience unique to the respondent" (1979, p. 596).

This wording, however, creates a potential difference between the frames of reference of the PSID data and the EOPP and SBA data. Namely, the PSID does not explicitly tell the respondent to assume that the new person has no previous experience, while both the EOPP and SBA do explicitly ask the respondent to make this assumption.[8] Keeping this potential difference in mind, Duncan and Hoffman report that the mean response to this question was 1.66 years, or about 86.32 weeks. The PSID sample, however, contains many individuals with a great deal of tenure at their employers. Black, Garen, and Loewenstein (1988) report that there is a very strong concave relationship between tenure at the firm and the length of time to become fully trained and qualified; therefore, we should not be surprised that the PSID mean is so much larger than the EOPP and SBA means.

The National Longitudinal Survey of the High School Class of 1972 (NLSHS72) provides a more detailed set of questions concerning training. Altonji and Spletzer (1991) provide an excellent description and analysis of these data. In 1986, the survey asked for the number of weeks and the number of hours per week of four different forms of training in the worker's current job or last job held by the worker. These forms of training included employer-provided formal training (which Altonji and Spletzer suggest corresponds to the SBA on-site formal training), informal training, off-site training, and any tuition or financial aid that the employer offered to employees attending an educational institution. The sample of workers is limited to individuals who graduated from high school. The training questions ask about any training received at any time on the last job, and some workers may have considerable tenure at the job.

Altonji and Spletzer report that 45.7 percent of workers receive some employer provided training—that is, some formal, informal, or off-site training. For the individual training measures, 27.8 percent of all workers report that they received formal on-site training. This figure is somewhat higher than the 20.4 percent reported in the SBA data and the 15.1 percent reported by the EOPP data. The mean hours of formal training is 52.7 hours, but unlike the SBA and EOPP data, the NLSHS72 data is truncated at 93 weeks of training, so the means are not directly comparable. The mean duration of formal training, conditional on receiving this type of training, is 10.9 weeks, and more than a quarter of the sample has a duration lasting 12 weeks or longer, which is somewhat higher than the SBA figure of 20.4 percent. Given that these workers are more educated than workers in the two other data sets and given also that many of them have considerable tenure on the job, these differences do not appear to be overly large.

The informal training, however, has a much lower incidence (19.7 percent) than the other two data sets. In contrast, the incidence rates of informal training by management in both the EOPP and the SBA data are more than 85 percent. Thus, it would appear that the incidence of informal training is underreported in the NLSHS72 data. In our view, this finding is not particularly surprising. Formal training is probably much easier to remember than informal training, especially after several years have elapsed. Moreover, the worker may not recognize as "real" training much of the informal training that occurs. A newly hired worker may not consider asking a co-worker for assistance or some advice to be training, but the employer may count this as training, especially if it detracts from the performance of the co-workers. The mean number of hours of this type of training is 45.8 hours. The mean conditional on receiving informal training, however, is 233 hours, and half the sample reports the training lasting at least 20 weeks. To us, this seems to indicate that workers are more likely to remember the very long spells of informal training. As SBA data indicates that most spells of informal training last less than 12 weeks, this may account for the relatively low incidence rate of informal training in the NLSHS72 data.

Another large difference between the NLSHS72 and the SBA data lies in the incidence rate of off-site training. For the NLSHS72, Altonji and Spletzer report an incidence rate of 20.0 percent, whereas the SBA data indicates only a 6.9 percent incidence rate. While some of this difference may be explained by variances in the education levels of the two samples, the disagreement is still too large. Off-site training often involves a considerable expense because it may involve substantial direct costs in fees or tuition, and because it may be necessary to send the worker to a different location for training, incurring transportation, food, and lodging expenses. Firms and workers may wish to ensure a beneficial employment match before incurring such costs. Thus, we might anticipate that the off-site training incidence is somewhat higher for more senior workers because the likelihood of a separation declines with the worker's tenure. The mean hours of training, conditional on receiving this type of training, is 101.3 hours, which is considerably larger than the SBA's conditional mean of 48 hours.

Finally, Altonji and Spletzer note that the incidence of a worker taking advantage of tuition plans is 9.4 percent, with a mean of 18.2 hours. The mean conditional on an incidence of this type of training is 193 hours with a mean duration of 27 weeks. This suggests that formal schooling continues to be a rather important part of the training programs of many firms. Loewenstein and Spletzer (1993) report a similar rate for the CPS data (13.6 percent).

A comparison of the incidence rates and hours of training measures from the NLSHS72, EOPP, and SBA data provides four interesting observations. First, there is somewhat more formal training among workers as a whole than among a sample of newly hired workers. Second, this difference for off-site formal training is even more pronounced than for on-site formal training. These two observations suggest that formal training is more prevalent for workers with some tenure than for newly hired workers. Third, there are many incidences of on-site formal training and informal training that last longer than 12 weeks, which suggests that the EOPP and the SBA three-month frame may understate the training that newly hired workers received. Fourth, employees seem to understate the incidence of informal training, with workers more likely to remember particularly long spells of informal training. Given the prevalence of informal training that we find in the EOPP and SBA data, this suggests that relying on surveys of workers will lead to an understatement of the importance of informal on-the-job training.

To examine further the differences in formal training programs, we consider the National Longitudinal Survey of Youth, which is a survey of over 12,500 youths ranging in age from 14 to 22 years in 1979. Lynch (1992), Veum (1993), and Bartel and Sicherman (1993) provide analysis of this data set for different time periods. Concentrating on data from 1979 to 1985, Lynch focuses on a sample of workers who did not complete college and who received formal on-the-job training lasting at least four weeks. These limitations result in a sample of 3,064 workers. The four-week length of a training spell is a sampling feature of the early NLSY training measures. Lynch reports that only 4.2 percent of the workers in her sample had this type of on-the-job training, but that the average length of this type of training was 31 weeks. In contrast, in the SBA data 38 percent of all training incidences lasted at least four weeks so the incidence rate of formal training programs lasting at least four weeks is about 7.8 percent (20.5 percent of all workers receive any on-site formal training and 38.0 percent of these incidences last at least four weeks). Given that Lynch excludes college graduates from her sample and that, as we shall later see, college graduates are more likely to receive formal training programs, these two incidence rates appear very similar. This suggests that her conditional mean for formal training spells is a reasonable estimate of the conditional mean, although it is a bit low because it excludes college graduates. Lynch's results suggest that the distribution of time in formal on-the-job training spells has extremely thin but very long tails. By focusing on the training received in the first three months, surveys such as the SBA survey and the EOPP survey cut off that long tail, which may cause researchers to underestimate the training that newly hired workers receive.

As to the importance of the three-month truncation, we may use Lynch's estimate of the conditional mean to infer what the SBA measure of training would have been without the truncation. For the SBA data, our unweighted mean weeks of training, conditional on receiving formal training, is 4.1 weeks. We may separate this mean by

$$E(x) = Pr(x \leq 3 \text{ weeks})\, E(x \mid x \leq 3 \text{ weeks}) + Pr(x > 3 \text{ weeks})\, E(x \mid x > 3 \text{ weeks}). \quad (3.1)$$

The mean number of weeks of on-site training conditional on the number of weeks being less than or equal to 3 weeks is 1.51 weeks with 0.6541 of the spells being less than 4 weeks. Using Lynch's conditional mean estimate of 31 weeks, the estimated untruncated mean number of weeks of on-site formal training is about 11.7 weeks, suggesting that both the NLSY and the SBA and EOPP truncations lead to a very biased estimate of the sample mean. The NLSHS72 data has a mean weeks of formal training of 10.9 weeks, but this measure is truncated at 93 weeks.[9]

Veum (1993) uses data for the period between the 1986 and 1990 surveys of the NLSY. For these years, the data no longer require that the training spell last four weeks, and it is interesting that Veum reports that about 18.4 percent of all workers reported some company-provided training, which we interpret to be formal on-site training. This estimate is reasonably consistent with the estimates of other data, especially when the differences in samples are considered (the other incidence estimates were 20.4 percent from the SBA, 15.1 percent from the EOPP, and 27.8 percent from the NLSHS72). Given Veum's means, we may calculate that the mean number of hours, conditional on receiving some company training, is approximately 135 hours, which is considerably larger than the truncated EOPP (78 hours) and the SBA (67 hours) data, but is smaller than the corresponding estimate from the NLSHS72 (190 hours). Veum's measure is for all incidences of formal training that occurred between 1986 and 1990.

Using data from 1988 to 1990, Bartel and Sicherman (1993) report that the incidence of formal training was 12.1 percent with a conditional mean of 260 hours, which is considerably larger than the mean reported by Veum and larger than the mean from the NLSHS72 data (190 hours). Bartel and Sicherman report on annual data, which partly explains their lower incidence rates for the total training received over a four-year period. We are still unclear why Veum's estimates of the hours trained are so much smaller than Bartel and Sicherman's estimates.

The Current Population Survey (CPS) provides cross-sectional estimates of the incidence of training that workers received before or on their current job both in 1983 and 1991. Lillard and Tan (1992) and Pergamit and Shack-Marquez (1987) analyze the 1983 data. The CPS on-the-job training data may be divided into formal and informal training programs. Lillard and Tan report that 11.7 percent of men needed formal (what they call "company") training on their previous job to obtain their current job, and 11.6 percent of men needed formal training while on their current job. If we combine those two estimates, we obtain an incidence rate of approximately 23 percent, although clearly we may have some double counting. Pergamit and Shack-Marquez (1987) report that 30 percent of their sample, which differs from Lillard and Tan's sample, received training both *before* and *during* their current jobs. Thus, a back-of-the-envelope calculation to correct for the double counting suggests an incidence rate of about 20 percent. For women, the sum of the two training measures was 20.6 percent and fell to 18 percent when we corrected for double counting.

If one takes the more conservative estimates of training on the current job—13.1 percent for women and 11.6 percent for men—we do see a lower incidence rate than data from the other samples. It is important to keep in mind, however, that the CPS sample is representative of all workers in the economy. Previous data sets discussed thus far have not sampled all workers, and this variation may account for some of the differences in the data sets. Most important, unlike the EOPP and SBA data, the CPS is not a sample of newly hired workers, who receive most of the training among all workers. Using a different CPS sample, Pergamit and Shack-Marquez report that the incidence rate of formal training on the current job is 14.2 percent.

For informal on-the-job training, Lillard and Tan report that 15.1 percent of men and women received informal training on their current job, and 30.8 percent of the men and 26.0 percent of the women said they received training on their previous job that was necessary to obtain their current job. Thus, the incidence rates from the CPS are well below the incidence rates of the SBA and EOPP data. This again may reflect the fact that workers are less likely to remember incidences of informal training.

Loewenstein and Spletzer (1993), using the 1991 CPS data, also report an extremely low incidence rate of informal training—16.3 percent. While this figure is somewhat higher than the 1983 CPS estimates, it is much lower than the employer-reported estimates from the SBA and the EOPP data. Indeed, the estimate is quite similar to the 19.7 percent figure Altonji and Spletzer (1991) obtained from the NLSHS72. Therefore, the CPS data also seem to suggest that workers may underreport the incidence rate of informal training.

In contrast, Loewenstein and Spletzer report an incidence of 17.2 percent for formal training, which is reasonably similar to the employer-reported incidence rates in the EOPP and SBA data (15.1 percent and 20.5 percent, respectively), and the employee-reported incidence rates from the NLSY and NLSHS72 (12.1 percent to 18.4 percent and 27.8 percent, respectively). While the 1983 CPS data gives somewhat lower incidence rates, and the NLSHS72 shows slightly higher incidence rates, employee- and employer-reported incidence rates appear reasonably similar for formal training. Moreover, comparing the 1982 EOPP survey and 1992 SBA survey of employers, and comparing the 1983 CPS and the 1991 CPS of employees, there is some modest evidence of an increase in formal training. For the employer surveys, training increased from 15 to 20 percent, and for the employee surveys, training increased from 12 to 17 percent.[10]

The 1991 CPS data also reported the duration of the training that the worker received. Loewenstein and Spletzer report that the mean duration of formal training, conditional on receiving formal training, is 9.6 weeks. This estimate approximates the NLSHS72 estimate of 10.9 weeks and the SBA estimate (adjusted for the truncation using Lynch's conditional mean) of 11.7 weeks. Thus, measures of the duration and the incidence rates of formal training appear similar.

Finally, the Survey of Income and Program Participation (SIPP) in 1984 provides another data source to measure training. But because the incidence rates are extremely small, we are suspicious of the data. Flynn (1993) and Haber (1988) report that the SIPP survey asked in 1984, "did [you] receive training designed to help [you] find a job, improve job skills or learn a new job?" Although this appears to be a very broad question encompassing formal and informal job training as well as vocational and technical schooling, Haber estimates that only 23 percent of workers in his sample had ever received any training, and only 9.4 percent of all workers had ever received on-the-job training. In our view, these estimates are much too low and indicate that the SIPP data missed a great deal of training.

Conclusions

We conclude with the following five observations concerning employee- and employer-reported measures of training.

1. Employee- and employer-based data sets appear to provide similar estimates of the incidence of on-site formal training, and there is at least some evidence that the measures of the duration of training estimates are not dissimilar.
2. The EOPP and SBA data, which measure training only for the first three months of employment, appear to understate training substantially. Evidence from the SBA and NLSHS72 studies suggests that the difference matters for both formal and informal training. Similarly, the early NLSY estimate that only measures formal training programs of at least four weeks in length also appears to miss a great deal of the formal training.
3. A comparison of the SBA and NLSHS72 data suggests that the incidence of off-site training may increase with the worker's tenure.
4. Comparing the EOPP and SBA data with the PSID data, there is some evidence that employees may understate (relative to employers) the length of time to become fully trained and qualified.
5. The incidence rates for informal training reported by employees are dramatically below the incidence rates for informal training reported by employers.

There is one caveat: Our examination of the means of various training measures from different surveys may not provide a good indication of how precisely we are measuring training. Even if employees and employers provide estimates of training that have identical means, there may be considerable measurement error in the reports of both employers and employees. If the measurement error is completely random, the means may be quite similar, but the training measures themselves are very inaccurate.

NOTES

1. Part of these differences may also reflect the fact that the skills required of more complex jobs are more complementary with physical capital such as computers or with high-performance workplace transformations.

2. The sample was stratified by establishment size in the following manner: 1,250 establishments with 0-19 employees, 1,250 establishments with 20-99 employees, 550 establishments with 100-499 employees, and 550 establishments with 500 or more employees. We excluded Agriculture, Forestry, and Fisheries (SIC 0-99) and Public Administration (SIC 900 and above). Except for these exclusions, we sampled establishments randomly within each size stratum, providing a representative distribution by industry and region.

3. We first sent a letter to each establishment describing the survey. SRC attempted to track down establishments with undeliverable letters using directory assistance and attempted to contact each of the 3,600 establishments for a telephone interview. Of the original sample of 3,600 establishments, 2,561 were eligible to complete an interview. The 1,039 ineligible establishments were out of business, had disconnected phones, did not answer in any of 15 attempts, could not be reached because of Hurricane Andrew, had other miscellaneous problems, or had no employees. We had 1,288 establishments complete the survey. The 1,273 noncompletions consisted of refusals, those who reported that answering surveys was against company policy, those who stated that the appropriate person was repeatedly unavailable, and those who rescheduled the interview six or more times.

4. The breakdowns for firm size were 1-4 employees, 5-9 employees, 10-19 employees, 20-49 employees, 50-99 employees, 100-199 employees, 200-499 employees, and 500 or more employees.

5. To enhance comparability of the samples, EOPP and SBA sample weights are used in the training computations reported in this chapter. The SBA weights account for the stratification by establishment size and the oversampling of large firms. The EOPP weights account for the oversampling of smaller, single establishments firms within each target metropolitan area.

6. Relative to the reference year of 1993, the 1991 coefficient is -.0303 ($t = 4.53$) and the 1990 coefficient is -.0306 ($t = 4.44$). We thank James Spletzer for kindly providing these estimates.

7. See Juhn, Murphy, and Pierce (1993) or the special issues of the *Quarterly Journal of Economics* (February 1992) for references.

8. See Sicherman (1990) for a discussion of this point and related issues concerning the PSID training measure.

9. The reader should keep in mind, of course, that combining these two estimates makes heroic distributional assumptions, but we believe that it does indicate a very important limitation of both data sets. The NLSY misses a majority of the spells of formal training, and the SBA and EOPP data truncate the very long spells of training that are an important determinant of total training.

10. In a remarkably thorough study of all forms of training, Lillard and Tan (1992) also report incidence rates from the National Longitudinal Surveys of young men and mature men, which were conducted in various years ranging from 1967 to 1982. They report that the incidence of formal training in a two-year period for young men is 10.4 percent and for mature men, 5.6 percent. It is difficult, however, to directly compare these data with either the CPS or employer-provided data.

CHAPTER 4

Who Receives On-the-Job Training?

With the development of data sets containing explicit measures of on-the-job training, economists have been able to identify the recipients of on-the-job training. Because of the importance of on-the-job training in determining wages, this question is of considerable interest. According to traditional on-the-job training models developed in chapter 2, workers with jobs that offer little or no training should not anticipate large increases in wages, since work experience does little to increase their stock of human capital. If workers with different personal characteristics differ in their access to on-the-job training, wage differences across workers may be attributed, in part, to these differences in access.

This chapter examines the recipients of on-the-job training from the perspective of two employer-reported data sets: the 1982 Employment Opportunity Pilot Project (EOPP) survey and the 1992 Small Business Administration (SBA) survey. Both data sets contain two types of training measures. The first measure is the actual number of hours of training that newly hired workers received in the first three months of employment. The second measure, one that indicates the total human capital of a particular job, is the length of time it would take the worker to become fully trained and qualified if that worker had no previous experience on the job. While these data are limited to newly hired workers, evidence presented in chapter 3 suggests that many current data sets that rely on employee reports of training appear to exclude significant amounts of on-the-job training. In contrast, the SBA and EOPP surveys offer us a reasonably detailed account of the training that newly hired workers received at two points in time that are ten years apart.

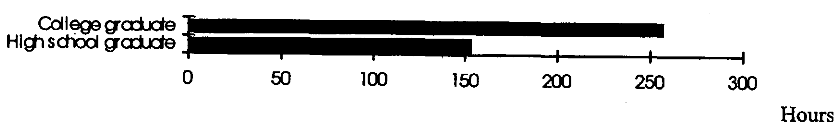
Variations in the Level of Training

In approaching the issue of which employees have access to on-the-job training, we proceed in stages. In the first part of this section, we consider the simple gross differences in various levels of training by workers who differ in their level of education, work experience, size of employer, and gender. The second part of this section provides a more comprehensive examination of who has access to training. The results presented there establish the statistically significant links between various characteristics of individuals and their level of training, with other factors being equal. The next section examines the incidence of five types of training: off- site formal training, on-site formal training, informal management training, informal co-worker training, and training by watching others. To understand the importance of the truncation of our training measure at the first three months of employment, the final section of this chapter examines the likelihood that each type of training may continue after the first three months.

Differences by Education, Experience, Gender, and Employer Size

Not surprisingly, we find that those characteristics that are highly correlated with wages are also correlated with access to on-the-job training. For instance, in both the 1982 EOPP and the 1992 SBA data sets, we find that highly educated workers receive more on-the-job training. In figure 4.1, we depict the mean level of training for college and high school graduates from the SBA data. College graduates receive over one hundred more hours of on-the-job training in the first three months of employment than do high school graduates, or nearly 68 percent more training. Thus, employers of more educated workers provide them with more on-the-job training.

Figure 4.1 Hours of Training in the First Three Months of Employment, 1992 SBA Data



The difference between college and high school graduates is even more dramatic when we look at the time required to become fully trained and qualified. In figure 4.2, we depict the means for the time to become fully trained for college and high school graduates. On the average, college graduates occupy jobs that required about 95 percent more time to become fully trained, with college graduates taking nearly 37 weeks to become fully trained and high school graduates only 19 weeks.

Figure 4.2 Number of Weeks to Become Fully Trained and Qualified, 1992 SBA Data

Thus, college graduates obtain jobs that are more complex and provide more on-the-job training than do those with only a high school education.

Another important determinant of wages is a worker's labor market experience. In figure 4.3, we depict the average training received by workers with three levels of experience: those with no previous experience, those with less than a year of experience, and those with more than a year of experience. Workers with no previous experience received the lowest level of training, only about 164 hours. In contrast, workers with less than a year of experience received nearly 198 hours of training. Workers with more than a year of experience received only about 178 hours of training. These figures suggest that workers with significant experience may require less training because some of the skills that these workers learn at one employer can be transferred to their new jobs.

The relationship between labor market experience and the time to become fully trained and qualified seems to confirm the suggestion that skills are transferable across jobs. In figure 4.4, we depict the means of time to become fully trained by level of experience. Unlike hours of training, the total human capital required for a job increases as the worker develops more experience. In other words, workers with more experience are hired into more complex jobs that require more total human capital. For workers without any experience, it takes just over 16 weeks to become fully trained, but for a worker with less than a year of experience, the training takes just over 23 weeks. Workers with more than a year of experience occupy jobs that require nearly 29 weeks to become fully trained. Thus, firms tend to hire workers with more experience to fill complex jobs. But as a result of their experience in previous jobs, these workers may require less training than workers filling jobs that require less total human capital.

Figure 4.3 Hours of Training in the First Three Months of Employment by Experience Level, 1992 SBA Data

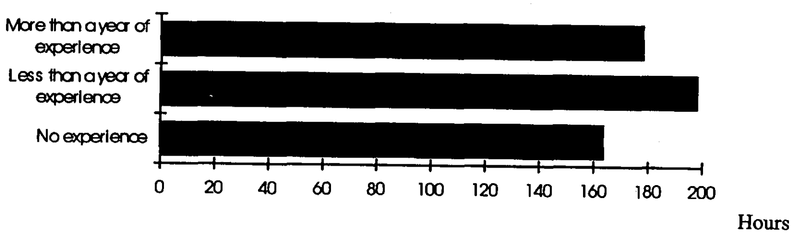
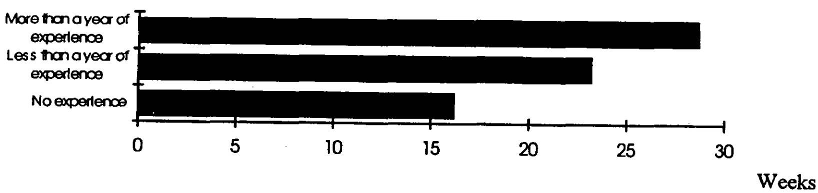
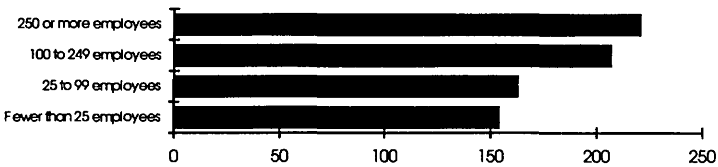


Figure 4.4 Time to Become Fully Trained and Qualified by Experience Level, 1992 SBA Data



Labor economists have also found that workers employed in large establishments tend to receive higher wages than similar workers in small establishments (e.g., Barron, Black, and Loewenstein 1987; Holtmann and Idson 1991). Similarly, both the EOPP and SBA data indicate that workers in large establishments receive more training than workers in small establishments. In figure 4.5, for the SBA data we depict the mean hours of training by four categories for the size of establishment: those establishments with less than 25 employees, those with 25 to 99 employees, those with 100 to 249 employees, and those with 250 or more employees. Workers in establishments with fewer than 25 employees received about 154 hours of training in the first three months of employment, while workers in establishments with 250 or more employees received over 220 hours of training. Thus, workers in the largest category of establishments received 42 percent more training than workers in the smallest category. Interestingly, neither the EOPP nor the SBA data reveal any significant difference in the time to become fully trained by size of establishment. Of course, larger establishments are training their workforces more intensively in the first three months than smaller establishments, so these large employers presumably are providing their workforces a greater stock of human capital.

Figure 4.5 Hours of Training in the First Three Months of Employment by Establishment Size, 1992 SBA Data

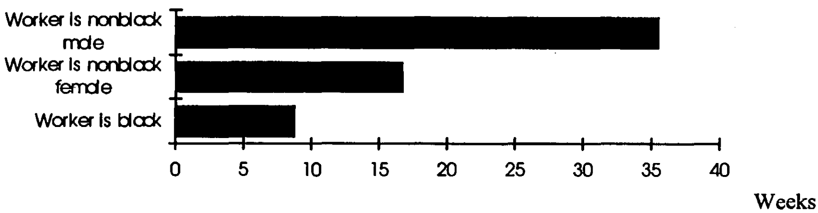


Hours

There remain very significant racial and gender differences in the wage structure across the United States economy, and one might anticipate that there would exist very substantial differences in the access to on-the-job training as well. Surprisingly, both the EOPP and SBA data provide little evidence that women receive significantly fewer hours of training in the first three months of employment than men. Unfortunately, the EOPP data contain no controls for the employee's race, but the SBA data do not indicate significant racial differences in hours of training in the first three months. In contrast, figure 4.6 shows that there are significant racial and gender differences in the time to become fully trained and qualified. Blacks occupy jobs that take about

9 weeks to become fully trained and qualified, nonblack women occupy jobs that take about 17 weeks to become fully trained, and nonblack men occupy jobs that take over 35 weeks to become fully trained and qualified. Given these differences, it is surprising that we find no significant differences in the hours of training in the first three months of employment, although we do find some evidence that limiting training to the first three months of employment might be partially responsible for not finding any gender differences. Because our measures of training are limited to the first three months, it is possible that men will obtain more total training if they are more likely than women to have training last beyond our three-month horizon.

Figure 4.6 Time to Become Fully Trained and Qualified by Race and Gender, 1992 SBA Data



Statistical Patterns Regarding Access to On-the-Job Training

The differences in training identified above still appear when we control for other factors with the use of regression analysis. In what follows, we analyze variations in on-the-job training in a more systematic fashion. For both the EOPP and the SBA data, we begin by estimating the total hours of training in the first three months of employment. For the EOPP data, the training measure is the sum of formal training, informal management training, co-worker training, and watching others. For the SBA data, we use the same four categories, and we add the number of hours of off-site training, although the results change little if we exclude this measure.

For independent variables in the SBA data, we use the worker's age, a vector of dummy variables indicating whether or not the worker is a high school dropout, whether or not the worker attended but did not graduate from college, and whether or not the worker has at least a four-year college degree. In both the EOPP and SBA surveys, employers were asked the number of years of labor market experience that an employee had in a job that the employer felt "had some application to the worker's current position." We interpret this measure as "relevant experience." Because the data rejected the use of a simple gender dummy used in conjunction with the white-nonwhite dummy, we also initially separated our sample into the following categories: black male; black female; white female; white male; nonwhite, nonblack male; and nonwhite, nonblack female. After some initial specification checks, we found we could combine the workers into four groups: black males, black females, nonblack females, and nonblack males.

We also included dummy variables indicating whether or not the worker is a union member, the number of workers at the establishment where the new employee works, and the number of employees at other sites. For the EOPP data, we attempted to use the same independent variables that we used for the SBA data, but data limitations forced us to use a specification that differed in three ways. First, the EOPP survey did not ask about the worker's race, so we used a simple gender dummy. Second, the EOPP did not have a measure of the number of employees at other sites, so we excluded this measure. Finally, the EOPP measure of unionization is the proportion of the workforce that is organized rather than a simple dummy variable. Also, for both data sets, we used hours worked occasionally as a control variable.

To obtain our samples, we excluded any worker who had a missing training measure or a missing value for any independent variable. These exclusions resulted in a sample of 888 workers for the SBA data. In addition, for the EOPP we excluded temporary or seasonal workers, which resulted in a sample of 1,473 workers. Table 4.1 reports unweighted means for both samples. We can immediately see the differences in the two data sets' sampling strategies. Workers in the EOPP data, which oversampled low-income workers, were about two years younger than workers in the SBA data and had about 0.9 less years relevant experience. More dramatic differences appear in educational attainment. For instance, only about 10 percent of the EOPP sample have a college degree or above, whereas over 25 percent of the SBA sample have a college degree. The SBA data, which oversampled large firms, has a mean establishment size of about 182 workers, compared to the EOPP's average of about 72 workers.

Table 4.1 Means for the 1992 SBA and 1982 EOPP Data

Variable	SBA	EOPP
Total hours of training	177.8	143.7
Time to become fully trained	24.0	22.3
Worker's age	29.3	27.1
Worker's relevant experience	3.4	2.5
Worker is a high school dropout	0.075	0.107
Worker is a high school graduate	0.384	0.588
Worker has some college	0.283	0.205
Worker is a college graduate	0.258	0.101
Worker is a black	0.088	---
Worker is a female	---	0.464
Worker is a nonblack female	0.483	---
Union	0.084	0.094
Hours worked	36.7	38.0
Number of employees at establishment	181.9	71.7
Number of employees at other sites	1197.8	---
Incidence of off-site formal training	0.107	---
Hours of off-site formal training	6.2	---
Incidence of on-site formal training	0.301	0.127
Hours of on-site formal training	25.2	9.8
Incidence of informal management training	0.920	0.873
Hours of informal management training	62.1	49.8
Incidence of informal co-worker training	0.695	0.625
Hours of co-worker training	40.6	27.3
Incidence of training by watching others	0.688	0.810
Hours of training by watching others	43.6	56.8
N	888	1,471

After some initial specification checks, we decided to use a double logarithm specification. To avoid taking the logarithm of zero, we added one to each employee's experience, training measure, and our measure of time to become fully trained and qualified. Columns (1) and (3) of table 4.2 report the estimates for the SBA and EOPP data, respectively. We initially used a tobit procedure to estimate the equations because 11 observations in the SBA data and 64 observations in

Table 4.2 Total Hours of Training for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA (1)	SBA (2)	EOPP (3)	EOPP (4)
Constant	4.446*	1.932*	5.093*	2.169*
	(7.62)	(2.65)	(10.10)	(2.94)
Logarithm of worker's age	-0.132	-0.165	-0.356*	-0.355*
	(0.72)	(0.92)	(2.24)	(2.27)
Logarithm of worker's relevant experience	-0.104	-0.136*	-0.108	-0.147*
	(1.65)	(2.18)	(1.90)	(2.59)
Worker is a high school dropout	0.031	0.150	-0.434*	-0.280*
	(0.20)	(0.93)	(3.40)	(2.12)
Worker has some college	-0.089	-0.036	0.264*	0.286*
	(0.82)	(0.33)	(2.89)	(3.16)
Worker is a college graduate	0.460*	0.440*	0.513*	0.504*
	(3.97)	(3.89)	(3.62)	(3.58)
Worker is black	-0.214	-0.154	--	--
	(1.16)	(0.88)		
Worker is nonblack female	-0.143	-0.039	--	--
	(1.59)	(0.43)		
Worker is female	--	--	0.019	0.122
			(0.25)	(1.61)
Union	0.085	0.072	-0.151	-0.146
	(0.60)	(0.51)	(0.96)	(0.92)
Logarithm of number of employees at the establishment	0.125*	0.099*	0.092*	0.081*
	(4.70)	(3.77)	(3.40)	(3.04)
Logarithm of number of employees at other establishments	0.031*	0.033*	--	--
	(2.39)	(2.69)		
Logarithm of hours worked	--	0.751*	--	0.808*
		(5.47)		(5.19)
R^2	0.095	0.132	0.045	0.063
N	888	888	1,471	1,471

NOTE: Absolute values ot *t*-statistics, calculated using robust standard errors, are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Ordinary least squares estimates are reported.

the EOPP data are truncated at zero. The Ordinary Least Squares (OLS) estimates, however, are very similar, which is not surprising given the low incidence of truncation. Because OLS parameters are easier to interpret than tobit parameters, we present the OLS estimates in table 4.2. To guard against any possible heteroskedasticity, we report *t*-statistics calculated using Huber's (1967) and White's (1980) robust standard errors. Despite the dissimilarities in the sample and the specification, the basic estimates appear similar except for the education profiles. In both data sets, large establishments provide more training. Both coefficients are statistically significant and similar in magnitude. A 10 percent increase in the size of the establishment increases the quantity of training by about 0.9 percent to 1.3 percent.

The differences in the education profiles are intriguing. The EOPP estimates show a strong relationship between education and training: Workers with more education receive more training. In the SBA data, however, there appears no significant relationship between education and training except for workers with college degrees. One might be tempted to ascribe this difference to a change in the pattern of training between 1992 and 1982, but using data from the 1991 Current Population Survey (CPS), Loewenstein and Spletzer (1993) find patterns in their educational coefficients that are similar to the patterns we find in the EOPP. In addition, as we shall see, the "time to become fully trained and qualified" variable is strongly correlated with education. Thus, the lack of a relationship between training and education at lower levels of education may be a sample anomaly rather than a change in the pattern of training. Both data sets agree, however, that college-educated workers receive much more training than high school graduates. From the EOPP data, we estimate that college graduates receive 56 percent more training; from the SBA data, we estimate they receive 60 percent more training.[1]

Neither data set shows any statistically significant differences in the acquisition of on-the-job training by gender, race, or union status. In the SBA data, the race and gender coefficients are substantial; the point estimates indicate that black workers receive roughly 26 percent less training than nonblack males, while nonblack females receive roughly 17 percent less training than nonblack males, although the coefficients are imprecisely estimated. The EOPP data indicate a statistically significant negative relationship between age and training while the SBA does not, although the coefficient in the SBA data is insignificantly negative. Also, the SBA data indicate that firms with more off-site employees provide more training to these employees, although the magnitude of the coefficient is about one-third the size of the coefficient for the size of the establishment.

In columns (2) and (4), we include hours of work as a control to explore the relationship between hours worked and training. In both data sets, employees who work longer hours receive more training, and both coefficients are statistically significant and similar in magnitudes. A 10 percent increase in the number of hours worked increases the amount of training about 7.5 percent to 8.1 percent. These estimates, however, should be viewed with a healthy dose of skepticism. Hours of work are clearly the result of a decision made between the firm and the worker. If a position requires a more highly trained worker, both firm and worker have an incentive to increase the hours of work for that position.

We use a similar specification to examine the determination of the number of months to become fully trained and qualified, or what we refer to as "the total human capital" of the job. OLS estimates for the total human capital measures from the SBA and EOPP data are presented in columns (1) and (3) of Table 4.3. We again use a double log specification, and we add one to each observation to avoid taking the logarithm of zero. In the SBA data, 12 observations are censored at zero, and in the EOPP data, 38 observations are truncated at zero. We initially used a tobit procedure to estimate the parameters, but because the OLS estimates were quite similar and easier to interpret, we report them. We again calculate the *t*-statistics using the robust standard errors suggested by Huber (1967) and White (1980).

Interestingly, the coefficients on experience are both positive and significant in both data sets, and their magnitudes are similar. Unlike the training equations, the coefficients on the education dummies exhibit the same pattern in the two data sets: the total human capital of the job appears to be increasing in the worker's education. While the coefficient for some college experience from the SBA data is about a sixth the size of the corresponding coefficient from the EOPP data, the coefficients on high school dropouts and college graduates are of similar magnitudes in both data sets. The coefficient on establishment size is negative and significant for the EOPP data, but is positive and not significant in the SBA data.

Unlike the training equations, the gender and race dummies indicate a difference in the job's total human capital by race and gender. For the EOPP data, women occupy jobs that require about 36 percent less time to become fully trained and qualified. In the SBA data, the race controls make for even more dramatic differences. Blacks occupy jobs that take about 58 percent less time, and nonblack women occupy jobs that take about 46 percent less time to become fully trained and qualified than nonblack males.[2] Thus, although there is no statistically significant evidence that minority workers receive less training than nonblack males or that women receive less training than men in the first three months of employment, there is considerable evidence that black workers and nonblack women occupy jobs with a good deal less total human capital than nonblack men.

Columns (2) and (4) of table 4.3 include hours of work as a control in the model specification. The inclusion of the hours variable reduces somewhat the coefficients for blacks, nonblack women, and women, but the coefficients remain highly significant. Thus, the fact that women work fewer hours than men explains only a small portion of the gender difference in the total human capital of jobs.

The use of these two different training measures, total hours of training in the first three months and the time to become fully trained and qualified, gives two pictures of the workers' access to on-the-job training. There may be differences in the type of training that we mask in our use of hours of training. For instance, if we think of training differing in quality, we might expect that blacks and nonblack women are systematically sorted into jobs that provide low-quality training, while nonblack males have access to jobs with high-quality training. An obvious limitation of the hours of on-the-job training measure is that it includes only the training received in the first three months of employment. As we saw in chapter 3, data from the NLSHS72 and NLSY suggest that the distributions of training spells have extremely long tails. Moreover, by focusing on the training received in the first three months, data sets such as the SBA and EOPP surveys cut off that long tail, which may cause us to miss racial and gender differences in training when using the hours of training measures.[3]

Table 4.3 Time to Become Fully Trained and Qualified for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA (1)	SBA (2)	EOPP (3)	EOPP (4)
Constant	1.176*	-2.708	3.032	1.757
	(2.41)	(4.97)	(7.26)	(3.24)
Logarithm of worker's age	0.289*	0.239	-0.207	-0.206*
	(1.99)	(1.73)	(1.59)	(1.67)
Logarithm of worker's relevant experience	0.222*	0.174*	0.197*	0.133*
	(4.10)	(3.45)	(4.17)	(2.91)
Worker is a high school dropout	-0.432*	-0.248*	-0.446*	-0.194
	(3.34)	(2.06)	(4.01)	(1.79)
Worker has some college	0.038	0.120	0.189*	0.225*
	(0.39)	(1.29)	(2.35)	(2.91)
Worker is a college graduate	0.525*	0.494*	0.476*	0.463*
	(4.80)	(4.87)	(4.16)	(4.24)
Worker is black	-0.804*	-0.712*	--	--
	(6.18)	(5.87)		
Worker is nonblack female	-0.570*	-0.409*	--	--
	(6.69)	(4.99)		
Worker is female	--	--	-0.422*	-0.253*
			(6.66)	(4.09)
Union	-0.139	-0.159	0.169	0.179
	(0.84)	(0.97)	(1.19)	(1.28)
Logarithm of number of employees at the establishment	0.029	-0.012	-0.049*	-0.066*
	(1.22)	(0.51)	(2.26)	(3.12)
Logarithm of number of employees at other establishments	0.008	0.012	--	--
	(0.68)	(1.06)		
Logarithm of hours worked	--	1.160*	--	1.323*
		(11.65)		(13.08)
R^2	0.162	0.271	0.075	0.141
N	888	888	1,471	1,471

NOTE: Absolute values ot t-statistics, calculated using robust standard errors, are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Ordinary least squares estimates are reported.

With the SBA data, however, we can control for this truncation at three months because we know not only the total hours of training but also the number of weeks of training. If we were to assume that the random error term in the training equation was drawn from a particular distribution, we could estimate the training equation accounting for the truncation. For instance, if we assume that the error term was normally distributed, we could use a censored regression model. Unfortunately, the prevailing wisdom is that such estimates are somewhat susceptible to misspecification (see Greene, 1993).

Rather than risk misspecification of the distribution of the error term, we estimate a Cox model, which is based on the Lehmann alternative of nonparametric statistics. The essential idea of nonparametric statistics is to allow researchers to make inferences without having to make assumptions about the distribution of random variables. For instance, letting T denote our training measure, the regressions reported in table 4.2 and 4.3 assume a model of the form

$$T = X\beta + \varepsilon \tag{4.1}$$

where ε is normally distributed with mean zero and variance σ^2, X is a vector of covariates, and β is a vector of parameters that we wish to estimate. If the assumption about the distribution of ε is true, the ordinary least squares is the most efficient estimator of the parameters β available. The nonparametric statistician asks the question, "How do you know that ε is normally distributed?" A truthful answer is often that such an assumption is convenient.

Of course, ordinary least squares estimation may still be justified as a method of moments estimator, with the caveat that tests of hypotheses are still going to require some additional assumptions about the second moment of the distribution. Cox (1972) recognized, however, that by focusing only on the rank of the dependent variable, parameters could be estimated and hypotheses tested without having to specify a distribution of the stochastic variable. Moreover, the Cox model easily handles the truncation of the dependent variable, a limitation that is relevant to both the EOPP and SBA data.

For the Cox model, the cumulative distribution function for the *ith* worker is expressed as

$$(4.2) \quad F(T_i)^{a_i}$$

where $F(\cdot)$ is what is commonly referred to as the "baseline" cumulative distribution function and a_i is an "index" function for the *ith* worker. If the term $a_i < 1$, then the *ith* worker is more likely to have a short training spell, and if $a_i > 1$ the worker is more likely to have a long training spell. (By definition, the "baseline" cumulative distribution function corresponds to the distribution of training spells for the person with $a_i = 1$.) Using this approach, we must only specify the functional form of a_i, or

$$(4.3) \quad a_i = \exp(-X_i\beta).$$

The Cox model, by focusing on the ranks of the dependent variable, allows the researcher to estimate β without specifying distribution of the dependent variable.[4] Thus, the Cox model affords two advantages of standard parametric estimates such as those we presented in tables 4.2 and 4.3. First, the Cox model does not require researchers to specify the distribution of the random variable. Second, the model allows researchers to account for the truncation of the dependent variable.

This approach is a fundamental departure from standard parametric estimation where we generally assume a probability distribution function of the form

$$(4.4) \quad f(T_i - X_i\beta).$$

In contrast, the Cox model has the probability density function:

$$(4.5) \quad f(T_i, X_i) = \exp(-X_i\beta)F(T_i)^{a_i - 1} f(T_i)$$

where $f(T_i)$ is the derivative of $F(T_i)$. Thus, while the standard parametric estimators assume that the independent variables affect only the mean of the distribution of training, the Cox model assumes that the independent variables affect the entire distribution of training spells. As a result, we cannot compare the two approaches and determine which approach better fits the data. The Cox model, however, does provide an important specification check on the results that we present in tables 4.2 and 4.3. It allows us to assess the robustness of our results with respect to the truncation of the training measures at three months and to evaluate our assumptions about the distribution of the error terms.

In table 4.4, we present the results from the Cox model. The results are generally consistent with those reported in table 4.2, with a couple of notable exceptions. First, the coefficient on the logarithm of employees at other sites is not significant. Second, the coefficient on nonblack females is negative and statistically significant, indicating that nonblack women received significantly less training than nonblack males. The coefficient on black workers, while not statistically significant, is about the same magnitude as the coefficient on nonblack females. Moreover, the lack of significance on the black coefficient is not a robust result. In Barron, Berger, and Black (1994), we report results using a slightly different specification and a larger sample size where the coefficient on black workers is negative and significant in a Cox model.[5] Using the EOPP data, Barron, Black and Loewenstein (1993) report that while women were placed in less complex jobs, they did not receive less training than males; in tables 4.2 and 4.3, we obtained similar results. In our view, these results should be viewed with suspicion. When we do not account for the truncation of training at three months, we obtain a similar result for the SBA data; therefore, we cannot reject the hypothesis that nonblack females receive the same quantity of training as nonblack males. Accounting for the truncation of training, however, reverses this result.[6] This suggests that researchers should be extremely careful when viewing racial and gender differences with truncated data.

One final finding of interest concerns the considerable change in the distribution of wages in the time between the EOPP and SBA samples. Blackburn, Bloom, and Freeman (1990); Katz and Murphy (1992); Becker (1992); and Berger (1992) document an increase in the returns to higher education during the 1980s. Juhn, Murphy, and Pierce (1993) show that in the 1970s and 1980s wages become much more disperse, with low-wage workers experiencing a fall in real wages while workers from the upper end of the distribution saw a growth in real wages. Given these fundamental changes in the distribution of wages, one might anticipate that changes in the distribution of on-the-job training are responsible for some of the changes in the distribution of wages. Surprisingly, we find little evidence for this hypothesis. The EOPP and SBA data sets provide reasonably consistent answers to the question of who receives training despite the ten-year difference in the age of the surveys. Thus, researchers will have to look beyond differences in training—at least training in the first three months of employment—to explain the dramatic increase in earnings inequality.

Table 4.4 Total Hours of Training for the 1992 SBA Data, Cox Model

Independent variables	SBA
Logarithm of worker's age	-0.021 (0.13)
Logarithm of worker's relevant experience	-0.050 (0.93)
Worker is a high school dropout	-0.118 (0.75)
Worker has some college	-0.106 (1.09)
Worker is a college graduate	0.478* (4.24)
Worker is black	-0.222 (1.50)
Worker is a nonblack female	-0.203* (2.33)
Union	0.108 (0.69)
Logarithm of number of employees at establishment	0.060* (2.40)
Logarithm of number of employees at other establishments	0.011 (0.87)
Chi-squared statistics	59.75
N	888

NOTE:. Absolute values of *z*-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level.

In the next section, we explore the determination of the incidence of training. This exercise will allow us to compare our results to many studies where the measures of training are binary (e.g., 1983 CPS data). In addition, it will allow us to examine differences in the incidence of each type of training.

Variations in the Incidence of Training

The incidence of training is inherently a dichotomous variable. Workers either receive a certain type of training or they do not. Because the incidence of training is a binary variable, we employ a logit procedure. In table 4.5, we present the logit estimates for the probability of receiving off-site formal training. The coefficient from a logit model may be interpreted as the rate of change in the logarithm of the ratio of probability that the variable is one, which we denote P, divided by the probability the variable is equal to zero, which we denote $(1 - P)$. The logarithm of the ratio of these two probabilities is often referred to as the "log odds' ratio." To convert this coefficient to a derivative of the probability, it is necessary to multiply the coefficient by $P(1 - P)$. About the sample mean, this can be easily done by referring to the mean incidence rates in table 4.1. For instance, evaluated about the mean, a worker with a college degree increases the probability of off-site training by about 0.122 [1.272 x 0.107(1 – 0.107)], or a worker with a college degree more than doubles the probability of receiving off-site training. The relationship among training, the size of the establishment, and the number of workers at other sites provides some insights into training decisions. Larger establishments tend to be less likely to provide off-site training to their employees, although the coefficient is not quite significant at the 10-percent level. In contrast, the larger the number of employees at other sites, the more likely the firm will offer off-site formal training, perhaps sending their newly hired workers to other company sites.

Table 4.6 presents logit estimates for the probability of receiving on-site formal training for the SBA and EOPP data. The two sets of estimates have some interesting similarities and dissimilarities. Both data sets indicate that college graduates are more likely to receive on-site

Table 4.5 Incidence of Off-Site Formal Training for the 1992 SBA Data

Independent variables	SBA
Constant	-2.055
	(1.27)
Logarithm of worker's age	-0.200
	(0.40)
Logarithm of worker's relevant experience	-0.067
	(0.44)
Worker is a high school dropout	-0.424
	(0.66)
Worker has some college	0.426
	(1.37)
Worker is a college graduate	1.272*
	(4.46)
Worker is black	-0.529
	(1.02)
Worker is a nonblack female	0.064
	(0.28)
Union	0.760*
	(2.23)
Logarithm of number of employees at establishment	-0.106
	(1.56)
Logarithm of number of employees at other establishments	0.108*
	(3.16)
Chi-squared statistics	43.21
N	888

NOTE: Absolute values of *z*-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

Table 4.6 Incidence of On-Site Formal Training for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA	EOPP
Constant	-2.059* (1.97)	-1.997 (1.88)
Logarithm of worker's age	-0.017 (0.05)	-0.163 (0.49)
Logarithm of worker's relevant experience	-0.195 (1.85)	-0.012 (0.11)
Worker is a high school dropout	0.523 (1.72)	-0.321 (1.03)
Worker has some college	0.062 (0.31)	0.541* (2.87)
Worker is a college graduate	0.639* (3.21)	0.617* (2.56)
Worker is black	0.181 (0.63)	---
Worker is a nonblack female	0.250 (1.52)	---
Worker is female	---	-0.070 (0.44)
Union	0.686* (2.64)	-0.190 (0.61)
Logarithm of number of employees at establishment	0.170* (3.64)	0.160* (3.04)
Logarithm of number of employees at other establishments	0.107* (4.56)	---
Chi-squared statistics	94.52	25.58
N	888	1,471

NOTE: Absolute values of z-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

formal training than are high school graduates—the differential is about 0.13 for the SBA and about 0.07 for the EOPP. Similarly, larger establishments are more likely to offer formal training; a doubling of the establishment size increases the probability of training by 0.036 for the SBA data and by 0.018 for the EOPP data.

The EOPP data indicate, however, that those workers who attended but did not complete college have about the same probability of undergoing formal training as college graduates; indeed, we cannot reject the hypothesis that the two groups have the *same* incidence of training. In contrast, the SBA data indicate that college graduates do have a higher incidence of training than high school graduates. Similarly, for the SBA data, we cannot reject the hypothesis that high school dropouts have the same incidence of on-site formal training as college graduates, but for the EOPP data, that hypothesis is easily rejected.

For the SBA data, we find that union members are more likely to receive on-site formal training than nonunion members; the differential is about 0.14. In contrast, in the EOPP data, firms with unions appear to be no more likely to offer on-site formal training than firms without unions. Thus, for the SBA data, union members appear to receive more off-site and on-site formal training. This result contrasts with the findings of Mincer (1983), who uses the 1978 PSID data; Barron, Feuss, and Loewenstein (1987), who use the 1982 EOPP data; and Lillard and Tan (1992), who use the 1983 CPS data. All of these studies find that union members receive less training than nonunion workers. Lynch (1992) and Veum (1993), using data from two different time periods of the NLSY, report that union members are more likely to receive formal training. Loewenstein and Spletzer (1993), using 1991 CPS data, report that, in a parsimonious specification that includes only race, gender, marital status, and education controls, union members are more likely to receive training than nonunion members, but the coefficient on union membership becomes insignificant yet still positive once controls for experience and tenure are included.

There are at least two potential explanations for the differences in these findings. First, differences in the sampling strategies might provide one rationale. Both the SBA and the NLSY have younger samples than the CPS or the PSID, although the EOPP is somewhat younger than the SBA sample. This suggests that younger union members are more likely to receive formal training than young nonunion members, but this difference dissipates over time and eventually reverses itself. Given that union workers have lower turnover probabilities than nonunion members, there is less reason to delay training among union members than among nonunion members.

Changes in the union sector in the last decade may provide another explanation. With the importance of international trade in the United States economy, there has been a rapid reduction in the domestic manufacturing sector of the economy, as other countries have assumed many semiskilled manufacturing jobs previously performed in the United States. A corresponding decrease in union membership has accompanied this trend. Given the United States' relatively advanced educational system and given that on-the-job training and education appear to be positively correlated, it may be that the United States' comparative advantage lies in jobs that require workers to obtain significant amounts of on-the-job training. If competitive pressures have eliminated the low-training union jobs, the difference between the 1982 EOPP data and the 1992 SBA data may indicate that there has been a structural shift in the economy.

With informal management training, it is important to remember that the incidence is quite high—about 92 percent for the SBA data and about 87 percent for the EOPP data. There are, therefore, few workers whom managers do not informally train. Table 4.7 presents the logit estimates for the probability of obtaining informal manager training for the SBA and EOPP data. Not surprisingly, we cannot reject the hypothesis that all the coefficients are insignificant for the SBA data. For the EOPP data, only two coefficients are significant. Workers with more experience are less likely to receive training, and larger establishments are more likely to offer this type of training.

For informal co-worker training, both the SBA and the EOPP data indicate that larger establishments are more likely to offer co-worker training. Table 4.8 presents the logit estimates for the two data sets. Evaluated at the sample means, a 100 percent increase in the size of the establishment increases the probability of co-worker training by 0.056 for the SBA data and 0.058 for the EOPP data. This finding probably reflects the fact that larger establishments are more likely to have a worker performing similar tasks, which reduces the cost of co-worker training. There are some minor differences in the two data sets. The EOPP results suggest older workers are less likely to receive co-worker

Table 4.7 Incidence of Informal Management Training for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA	EOPP
Constant	2.454 (1.50)	2.525* (2.55)
Logarithm of worker's age	0.058 (0.11)	-0.291 (0.94)
Logarithm of worker's relevant experience	-0.179 (1.09)	-0.262* (2.54)
Worker is a high school dropout	-0.577 (1.24)	-0.101 (0.39)
Worker has some college	-0.536 (1.77)	0.191 (0.91)
Worker is a college graduate	0.060 (0.17)	-0.037 (0.14)
Worker is black	-0.087 (0.18)	---
Worker is a nonblack female	-0.095 (0.36)	---
Worker is female	---	-0.043 (0.27)
Union	0.224 (0.41)	-0.099 (0.30)
Logarithm of number of employees at establishment	0.064 (0.80)	0.218* (3.72)
Logarithm of number of employees at other establishments	0.008 (0.21)	---
Chi-squared statistics	9.21	30.93
N	888	1,471

NOTE: Absolute values of z-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

Table 4.8 Incidence of Informal Co-Worker Training for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA (1)	EOPP (2)
Constant	0.857	2.313*
	(0.87)	(3.31
Logarithm of worker's age	-0.323	-0.769*
	(1.07)	(3.50)
Logarithm of worker's relevant experience	-0.165	0.055
	(1.70)	(0.74)
Worker is a high school dropout	0.317	0.006
	(1.02)	(0.03)
Worker has some college	0.211	0.080
	(1.13)	(0.56)
Worker is a college graduate	0.312	-0.244
	(1.54)	(1.31)
Worker is black	0.021	---
	(0.07)	
Worker is a nonblack female	0.151	---
	(0.94)	
Worker is female	---	0.117
		(1.04)
Union	-0.253	0.074
	(0.90)	(0.32)
Logarithm of number of employees at establishment	0.265*	0.247*
	(5.21)	(6.17)
Logarithm of number of employees at other establishments	0.044	---
	(1.79)	
Chi-squared statistics	63.55	72.77
N	888	1,471

NOTE: Absolute values of z-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

training, and the coefficient for the worker's age is highly significant. In the SBA, while the coefficient for the worker's age is negative, it is not significant. The coefficients for college-educated workers have the opposite signs in the two data sets, although neither is significantly different from zero.

Finally, both data sets again indicate a preference for "watching others" training among larger establishments. The logit estimates for the SBA and EOPP data are given in table 4.9. Evaluated at the sample means, a 100 percent increase in the size of the establishment increases the likelihood of this type of training by 0.029 in the SBA data and 0.027 in the EOPP data. Again, there are some minor differences in the two data sets. For the EOPP data, workers with more experience are less likely to undergo training by "watching others," while the same coefficient for the SBA data is negative but not significant. Similarly, for the EOPP data, workers with some college education are more likely to "watch others," while the same coefficient for the SBA data is positive but not significant.

Taken together, the two data sets provide a reasonably similar description of the incidence of the various types of training. Three major themes run through these regressions. First, large firms have higher rates of training incidence. These establishments tend to be more likely to offer formal on-site training, informal co-worker training, and training by watching others. Also, the EOPP data indicate that larger establishments are more likely to offer informal management training, and the SBA data indicate that firms with larger numbers of employees at other sites were more likely to offer off-site formal training. Second, both the SBA and EOPP data agree that college graduates are more likely to receive formal training than are high school graduates. And finally, the SBA data indicate that union members are more likely to receive formal training, but the EOPP does not provide any evidence of this differential.

Truncated Spells of Training

Because the SBA survey asked for the number of weeks of training and the number of hours of training per week, we may also assess how

Table 4.9 Incidence of Training by Watching Others for the 1992 SBA and 1982 EOPP Data

Independent variables	SBA (1)	EOPP (2)
Constant	1.026	0.978
	(1.06)	(1.12)
Logarithm of worker's age	-0.221	0.029
	(0.74)	(0.11)
Logarithm of worker's relevant experience	-0.115	-0.282*
	(1.21)	(3.10)
Worker is a high school dropout	0.158	-0.051
	(0.52)	(0.23)
Worker has some college	0.191	0.396*
	(1.03)	(2.11)
Worker is a college graduate	-0.017	-0.271
	(0.08)	(1.26)
Worker is black	0.209	---
	(0.73)	
Worker is a nonblack female	0.060	---
	(0.39)	
Worker is female	---	0.253
		(1.82)
Union	-0.047	-0.352
	(0.17)	(1.36)
Logarithm of number of employees at establishment	0.133*	0.180*
	(2.83)	(3.67)
Logarithm of number of employees at other establishments	0.022	---
	(0.92)	
Chi-squared statistics	20.24	39.96
N	888	1,471

NOTE: Absolute values of z-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

many spells of training are truncated by the three-month time frame. For the SBA data, we treat any spell lasting at least 12 weeks as truncated.[7] Conditional on receiving each type of training, the rates of truncation are 0.19 for off-site formal training, 0.16 for on-site formal training, 0.24 for informal management training, 0.26 for co-worker training, and 0.19 for "watching others." We have seen in chapter 3 that data from the NLSY and NLSHS72 indicate that many spells of formal training last beyond three months. Also, while the NLSHS72 data had an extremely low incidence rate of informal training, many spells of informal training were longer than three months. The SBA data indicate that informal training spells are also likely to last at least 12 weeks.

In table 4.10, we present logit estimates of the probability for each type of training that a training spell lasts at least 12 weeks. In column (1), we list the estimates for the off-site formal training. Only one of the coefficients is significant at the 5 percent level. Nonblack females are less likely than nonblack males to have undergone off-site training spells that last at least 12 weeks. Evaluated at the sample means, nonblack women's probability is about 0.30 smaller than nonblack men's. Thus, the truncation of the training measure at three months hides the gender differences in the off-site training variable. In column (2), we present the estimates for on-site formal training, and again, only one coefficient is statistically significant at the 5 percent level. Nonblack females are less likely to have on-site training spells that last at least 12 weeks than are nonblack males. Evaluated at the sample means, nonblack women's probability is about 0.12 lower than nonblack men's. Thus, for both formal training measures, nonblack women are less likely to experience a long spell of formal training, and the truncation of the training measures at three months hides the true gender differential. We also note that while the coefficient on blacks in the on-site formal training equation is not statistically significant, it is much larger than the nonblack female coefficient. Because there are only a limited number of blacks in the sample, it is difficult to obtain a precise estimate of the coefficients, and the reader should not interpret the insignificance of the coefficient as strong evidence that blacks are as likely as nonblack males to have long spells of on-site formal training.

Also in both formal training equations, it was necessary to eliminate the high school dropouts because no high school dropout had a spell of

Table 4.10 Incidence of Training Spells Lasting at Least 12 Weeks for the 1992 SBA Data

Independent variables	Off-site formal training (1)	On-site formal training (2)	Informal manager training (3)	Informal co-worker training (4)	Train by watching others (5)
Constant	4.560	0.645	-1.703	-0.718	1.974
	(0.85)	(0.24)	(1.54)	(0.54)	(1.37)
Logarithm of worker's age	-1.293	-0.018	0.263	-0.054	0.234
	(0.75)	(0.02)	(0.77)	(0.37)	(0.53)
Logarithm of worker's relevant experience	0.499	0.311	-0.125	0.196	-0.022
	(1.20)	(1.30)	(1.13)	(1.54)	(0.16)
Worker is high school dropout	---	---	-0.257	0.008	-0.073
			(0.72)	(0.02)	(0.16)
Worker has some college	-1.185	-0.486	-0.520	0.010	-0.029
	(1.24)	(0.94)	(2.25)	(0.04)	(0.10)
Worker is college graduate	-0.308	0.352	0.605*	0.852*	1.061*
	(0.41)	(0.87)	(2.98)	(3.60)	(4.04)
Worker is black	1.395	-1.126	-0.489	-0.201	-0.350
	(1.04)	(1.38)	(1.44)	(0.55)	(0.82)
Worker is nonblack female	-1.928*	-0.866*	-0.242	-0.224	-0.256
	(2.64)	(2.30)	(1.39)	(1.14)	(1.17)
Union	1.249	0.445	0.189	0.397	-0.234
	(1.58)	(0.95)	(0.66)	(1.24)	(0.59)

Logarithm of number of employees at establishment	-0.292 (1.47)	-0.164 (1.65)	-0.019 (0.38)	-0.028 (0.47)	-0.106 (1.63)
Logarithm of number of employees at other establishments	-0.111 (1.17)	-0.034 (0.64)	-0.006 (0.24)	-0.026 (0.90)	0.002 (0.05)
Chi-squared statistic	21.77	18.98	30.58	25.95	26.14
N	92	244	817	617	611

NOTE: Absolute values of z-statistics are in parentheses. Coefficients marked with an * are significant at the 5 percent level. Logit estimates are reported.

training lasting at least 12 weeks. As such, dropping out of high school perfectly predicts that the training spell is not truncated. The estimation procedure cannot determine a coefficient, and so it is necessary to drop the observations. In the case of off-site formal training, we exclude only three cases, and we should not overemphasize this exclusion. In the case of on-site formal training, however, we exclude 23 cases, which makes it highly likely that dropouts are less likely to have long spells of training. Given that dropouts have revealed an aversion to or a lack of aptitude for formal schooling, it is not surprising that they possess jobs that do not require long spells of formal training.

Column (3) presents the logit estimates for the probability of undergoing a spell of formal management training lasting at least 12 weeks. Two coefficients are significant at the 5 percent level. Workers who attended but did not graduate from college are less likely to have a long spell of informal management training than are high school graduates. At the sample mean, a high school graduate has a 0.096 higher probability of having a truncated spell of informal management training than a worker with some college education. This finding further demonstrates that the SBA sample of workers with some college education is somewhat anomalous because almost all other studies have found that training increases with education. Workers with college degrees, on the other hand, are more likely to have a truncated spell of informal management training. Evaluated at the sample mean, college-educated workers have about a 0.112 higher probability of undergoing a spell of informal management training that lasts at least 12 weeks than do high school graduates.

In columns (4) and (5) of table 4.10, we present the logit estimates for the co-worker training and "watching others" equations. In both equations, only one coefficient is significant at the 5 percent level: college graduates have a greater chance than high school graduates of undergoing a training spell that lasts at least 12 weeks. Evaluated at the sample means, the college graduates have a 0.164 greater probability of undergoing a training spell that lasts at least 12 weeks for both training measures than do high school graduates. Thus, for each type of informal training, college graduates have a significantly higher probability of undergoing a spell lasting at least 12 weeks than do high school graduates. This suggests that the 43 percent differential in the number of hours of total training between college graduates and graduates of only high school understates the true differentials in the training experiences of newly hired employees.

Thus, the three-month sampling frame of the EOPP data and the SBA data appears to have two major consequences. First, the sampling frame may hide a gender differential in formal training measures. Nonblack women are significantly less likely to have formal training spells that last at least 12 weeks than are nonblack men. Second, the sampling frame also understates the training differential that college graduates receive; college graduates are significantly more likely to have informal training spells that last at least 12 weeks than are high school graduates.

Conclusions

There are four conclusions that emerge from both the EOPP and SBA data. First, college graduates receive much more training than high school graduates. Even after controlling for other factors, college graduates receive between 56 to 60 percent more training than high school graduates in the first three months of employment. Moreover, analysis of the SBA data suggests that this difference is understated because college graduates are more likely to have their training spells continue past three months. When labor economists estimate the returns to college education without controls for on-the-job training, perhaps some of the presumed return to college education may be a return to the greater quantities of on-the-job training.

Second, when hiring workers for more complex jobs, firms apparently hire workers with greater prior relevant work experience for a couple of reasons. First, firms may find it more expensive to train workers themselves than to hire experienced workers. This would be especially true for positions in which the worker was expected to "learn by doing," or learn by actually performing the task at hand. Second, by hiring experienced workers, firms may reduce the risk that they have hired a worker who is incapable of doing the job. As we shall see in chapter 7, firms spend much more time evaluating workers when hiring for jobs with more training and greater total human capital.

Third, we find evidence that large establishments offer more training, which might explain some of the wage premium earned by workers at these larger sites. We find that a 10 percent increase in the size of the establishment increases the hours of training in the first three months of employment by about 0.9 percent to 1.3 percent. Larger establishments are also more likely to offer on-site formal training than are smaller establishments, and using the SBA data, we find some evidence that employees of large firms who work at small sites are more likely to undergo off-site training, as they presumably receive training at larger sites within the firm.

Finally, both data sets indicate substantial gender difference in the time to become fully trained and qualified, and the SBA data indicate a substantial racial difference in the time to become fully trained. Even after we control for other factors, blacks hold jobs that require about 60 percent less time to become trained and qualified while nonblack women occupy jobs that require between 36 and 46 percent less time than the jobs of nonblack males. Interestingly, we find that the truncation in the hours of training may hide gender differences in hours of training; women are significantly less likely than men to have spells of formal training that last longer than three months.

NOTES

1. We use the adjustment that Kennedy (1991) develops, where the differential g is approximated by $g = \exp[\hat{\beta} - 0.5\, \mathrm{VAR}\,(\hat{\beta})] - 1$.

2. Again, we sue the adjustment that Kennedy (1991) develops to estimate the differential.

3. The time to become fully trained, however, also suffers from some biases. The time variable does not measure the intensity of training, and the intensity of training may vary by the type of job. Moreover, certain jobs may require very little training, but workers may take a great deal of time to master fully the tasks that the job requires; they may learn by actually doing the job. Other jobs may require workers to be continuously in training while becoming fully trained and qualified. thus, two workers may take the same time to be fully qualified but may have much different training experiences. Because of these limitations, we interpret the "time to become fully trained" variable as a measure of a job's total human capital rather than as a measure of on-the-job training. As wel shall see in chapter 6, the evidence from wage growth equations supports this view: wage growth is more highly correlated with total hours of training than the time to become fully trained and qualified.

4. See Kalbfleisch and Prentice (1980) for details of the estimation procedure.

5. Given the similarity in magnitude, the reader may wonder if the two groups should be pooled. We cannot reject the hypothesis that the coefficients on black and nonblack female workers are the same. If we do pool these two groups, the coefficient is negative and significant with a z-statistic of -2.43.

6. As in the OLS case, controlling for hours reduces the magnitude of the coefficient on non-black female workers and eliminates its statistical significance. When controlling for hours, the coefficient is -0.121 with a z-statistic of -1.38.

7. Although there are 13 weeks in a three-month period, there was an unusually large frequency of responses at 12 weeks, indicating that many respondents used a 12-week horizon.

CHAPTER 5

How Well Do We Measure On-the-Job Training?

The Upjohn Institute Survey

In previous chapters, we examined different employer and employee measures of training and studied who receives training. This chapter addresses the issue of how well training is measured. In particular, we examine the causes and consequences of errors in the measurement of training. Several studies in the last few years have considered this issue, but most have examined the impact of measurement error on the estimation of wage equations; none has analyzed measurement errors in training, or the effects of training measurement errors on the estimated returns to training in wage equations. Consequently, economists are lacking information on how accurately training can be measured and on the consequences of the mismeasurement of training.

To examine measurement error issues, we designed and implemented a new survey that matches employer and employee responses to training questions. This survey permitted analysis not only of errors in the measurement of training, but also wages, productivity, and other commonly used variables in labor economics. This new data set allows us to address several questions: How accurate are measures of on-the-job training? Do employers' and employees' responses to identical questions concerning on-the-job-training differ? If answers to these common questions do differ, who reports more training? Do firm and worker responses to formal training questions show more agreement than responses to informal training questions?

The Upjohn Institute commissioned this new survey, which we conducted in spring 1993. Approximately 300 firms and workers participated in the survey. Each firm was asked about its last worker hired, and we conducted interviews with both the firm and the worker. Measurement error is more apparent for some variables than others. Figure 5.1 shows the scatterplot of firm and worker starting wages.

Workers and firms agree for the most part on the rate of pay at the start of employment. In fact, the correlation between worker and firm reports is 0.974. The level of agreement between the worker and the firm is somewhat lower for hours worked per week and months of relevant experience prior to employment. The scatterplots for these variables are shown in figures 5.2 and 5.3. The correlation between worker and firm reports for hours worked is 0.769 and 0.727 for months of relevant experience. Firms and workers disagree to an even greater extent about the amount of training provided. Figure 5.4 shows the scatterplot of worker and firm reports of total hours of training provided in the first four weeks of employment. The correlation coefficient is 0.475. Firms report 25 percent more hours of training on average than do workers, although firms and workers report similar incidence rates.

In the remainder of this chapter, we first examine previous measurement error studies in labor economics and then describe our new data set. The results of the survey are then discussed, beginning with the correlations between employer and employee responses for a number of variables considered in previous studies. We then turn to an analysis of the measurement of on-the-job training. Finally, we examine the determinants of training and the correlates of differences in employer- and employee-reported training.

Previous Validation Studies in Labor Economics

A number of papers have considered the issue of reporting errors. Mellow and Sider (1983) use an employer-employee matched supplement taken from the 1977 January Current Population Survey (CPS) and the first wave (1980) of the Employment Opportunity Pilot Project (EOPP). While there is a fair amount of disagreement between employers and employees on industry and occupation, it appears to have little effect on cross-section wage structure. Further, the structure of wages is independent of whether employer- or employee-reported wages are used.

Figure 5.1 Firm- and Worker-Reported Wages

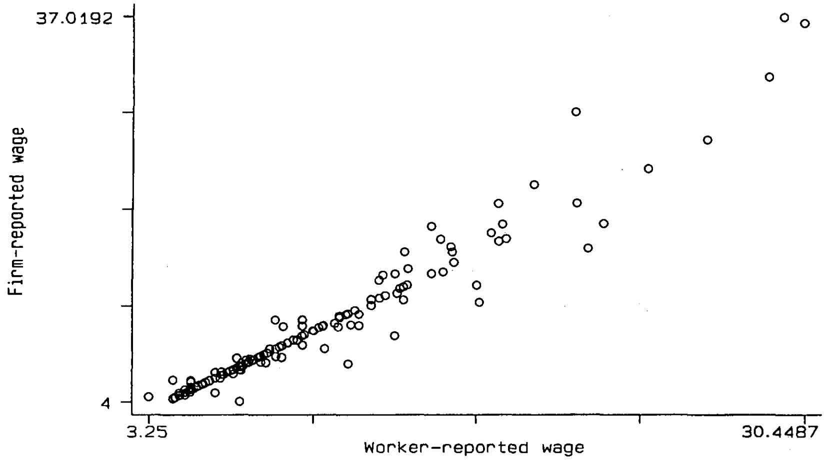


Figure 5.2 Firm- and Worker-Reported Hours Worked

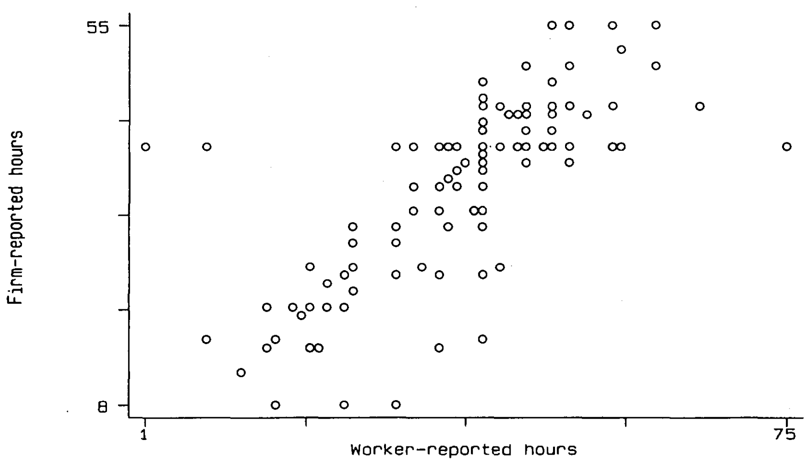


Figure 5.3 Firm- and Worker-Reported Experience

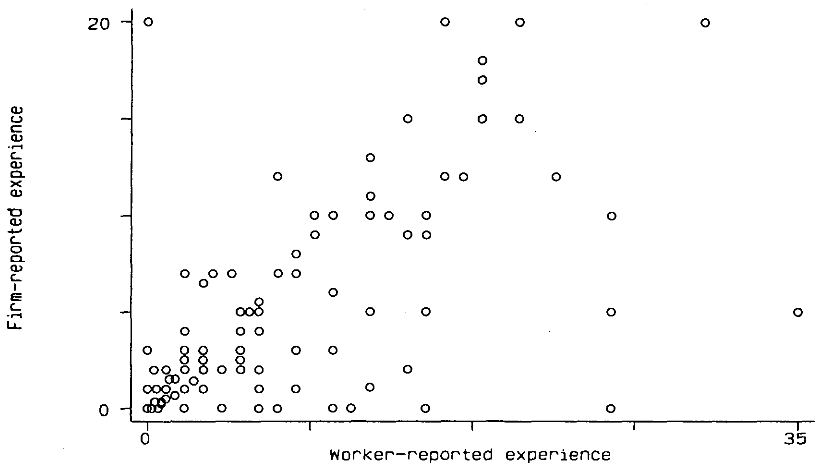
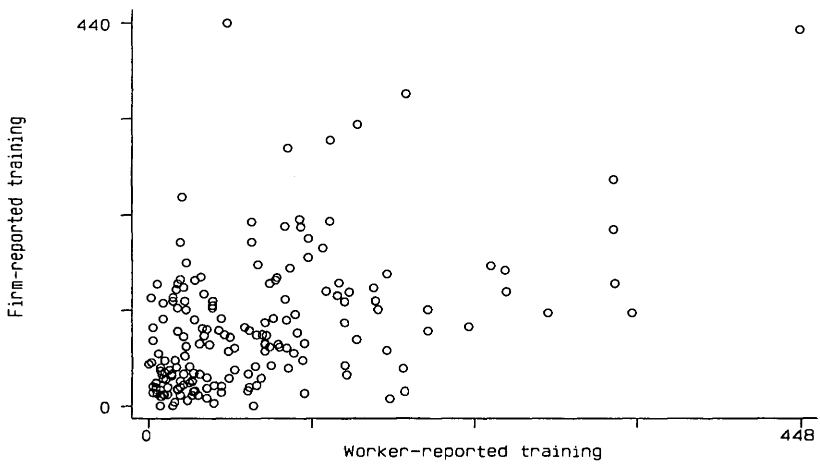


Figure 5.4 Firm- and Worker-Reported Training



Greenberg and Halsey (1983) investigate the effects of reporting error in the Seattle and Denver income maintenance experiments by comparing employer reports with respondent self-reports. Previous studies based on respondent self-reports find appreciable reductions in work efforts by those eligible for experimental income maintenance payments. Using employer-reported data, they find that the reductions in the work effort of husbands, wives, and young workers who are not heads of households are much smaller than the self-reported data would imply. The estimates for female heads appear to be unaffected by reporting error.

Freeman (1984) examines differences in reporting union status between worker and firm reports. He uses the 1977 January CPS employer-employee matched data as well as the 1979 May CPS regular survey and the pension supplement. Assuming differences between employer and worker reports are the result of measurement error, he argues that such error may negate the value of using longitudinal data to estimate union wage effects because the measurement error biases the union wage effects downward.

Duncan and Hill (1985) use a unique set of data from a single manufacturing company that provides unprecedented detail on the work history of employees. They compare administrative records with information obtained from a sample of workers as a validation exercise for the Panel Study of Income Dynamics (PSID). They report estimates of wage equations that include schooling and tenure using both worker and firm reports. They find a bias of 30 percent in the estimated return to tenure due to the correlation between measurement error and tenure.

In related work, Lillard, Smith, and Welch (1986) examine measurement error induced by the Census Bureau imputation procedure used to fill in missing observations in the CPS. They find that the Census imputation procedure severely understates income in some occupations. Because the evidence suggests that nonreporting is tied to income, the imputation procedure also probably causes an understatement of average income.

Bound and Krueger (1991) use matched CPS-Social Security data to examine the prevalence of measurement error. They find that measurement error in panel data is not as significant as previously thought because positive serial correlation and mean reversion of the measurement error increase the reliability of panel earnings data.

Bound, Brown, Duncan, and Rodgers (1990, 1994) also use the matched CPS-Social Security data and a later wave of the PSID validation survey. They find annual earnings to be fairly reliably estimated. *Hourly* earnings estimates are quite unreliable, however, because hours worked are imprecisely reported. Rodgers, Brown, and Duncan (1993) also examine errors in survey reports of earnings, hours worked, and hourly wages using the two waves of the PSID validation study. They find that measures of hourly earnings are "distressingly unreliable," arguing their findings suggest the importance of measurement error.

A New Validation Survey

None of the studies described in the previous section considers errors in reporting on-the-job training, or the effects of reporting errors on the estimated returns to training. Our survey, conducted in the spring of 1993, obtained evidence on this question from employers and employees. A major focus of the survey (hereafter referred to as the Upjohn Institute survey) was to provide worker-firm comparisons of on-the-job training measures. Because the intensity of on-the-job training is likely to be highest for newly hired workers, we decided to target those workers, similar to the EOPP and SBA surveys. We asked both firms and their last worker hired a series of training questions based on those used earlier in the EOPP and SBA surveys.

One of our major concerns centered on the accuracy of responses given by employers and employees. We did not want employers or employees to have to recall details of training that took place months or even years prior to the interview. To minimize this problem, we twice interviewed employers and employees about training, once after two weeks of employment, and a second time after four weeks of employment. This meant that we had to conduct interviews within narrow windows of time, within a couple days of the end of the second and fourth week of employment.

Because of the survey design, we needed to contact firms that were hiring at the time we conducted the survey. Given our experience with the EOPP and SBA surveys, we were concerned that a number of establishments, especially small ones, would be ineligible for the survey simply because they were not hiring. Therefore, we eliminated establishments with fewer than 100 workers from our sample universe to increase the chances of locating suitable establishments.

We obtained a nationwide random sample of 5,000 establishments with 100 or more employees from Survey Sampling, Inc. The survey was conducted in 1993 by the Survey Research Center (SRC) at the University of Kentucky. Each of the 5,000 establishments was first sent a letter describing the survey and the general nature of the training questions. Telephone interviewing began on March 22, 1993 and was completed on June 21, 1993. In the initial interview we asked background information about the firm and the characteristics of the last worker hired. If firms had not hired in the past ten days or were not planning to hire soon, they were deemed ineligible for the survey and dropped from the sample.[1]

Once we had completed the initial firm interview, we asked to speak with the worker. We then asked the worker a set of background questions similar to what we had asked the firm. The second interview of the employer and employee occurred after the employee had been with the employer for two weeks. At that time, we asked both the worker and the firm a set of questions about the training activities of the worker during the first two weeks of employment. The last interview occurred after the worker had been with the employer for four weeks. This interview consisted of the same set of questions about training as the second interview and concluded with a few questions about worker productivity and promotion probabilities.

There were a couple of drawbacks to our survey design and sampling strategy. Restricting the original sample to establishments with 100 or more workers meant that the sample would not be nationally representative of all establishments. Second, with three interviews for both the firm and the worker, there was the potential of attrition bias. While there was some attrition, it did not appear to be a significant problem. Eighty-five percent of the worker-firm pairs that completed the initial interview completed the entire set of interviews.

Given the level of commitment required from the firm and the worker, we anticipated a much lower level of response than a typical one-time interview would provide. From our initial sample of 5,000, we did not attempt to contact 1,603 establishments due to budget considerations. The hiring restriction eliminated a number of firms immediately from the analysis. Out of the 3,397 establishments that we attempted to contact, 1,359 reported that they were not hiring. Others were ineligible to complete the survey due to a number of reasons: 229 had disconnected phones; in 111 establishments we only received answering machines, busy signals, computer tones, or no answer in repeated attempts; 60 establishments had gone out of business; we had language communications problems with eight establishments; 76 establishments had moved or closed, and we could not obtain a new address or verify closure. These exclusions left 1,554 establishments eligible to complete the survey. Of these, 541 directly refused to participate, 241 said it was against company policy to answer a survey, 255 were unwilling to let an employee participate in a survey, and 212 repeatedly scheduled callbacks, which we took as an implicit refusal. There were 258 completions of all six interviews, and 47 partial completions. We only counted as a partial completion those cases in which we completed at least one employer and one employee interview. The response rate of partial and full completions was approximately 20 percent.

To compare our sample of completions with our original sample, we obtained from Survey Sampling a few characteristics of each of the establishments in our initial sample of 5,000. These included Standard Industrial Code (SIC), Metropolitan Statistical Area (MSA) code, and status, state, and establishment size code. We distinguished among four establishment size categories: 100-249 employees, 250-499 employees, 500-999 employees, and 1000 or more employees. To summarize the representativeness of our sample, we estimated a probit model indicating a partial or full completion for a particular establishment. The explanatory variables consisted of eight one-digit SIC industry dummies (The first two SIC codes were combined to avoid empty cell problems), eight Census region dummies, three establishment size dummies, and a MSA status dummy. This analysis indicated that there were no significant differences at the 5 percent level in the industry and establishment size composition of the completions and the original sample. The sample of completions, however, was significantly more likely to come from rural areas and from the Mountain and Pacific Census regions than the overall sample of 5,000.

Correlations Between Employer and Employee Reports and Comparisons with the Results of Previous Studies

The survey provides us with two potentially inaccurate measures on the same variable. Thus, for a continuous variable of interest x, such as on-the-job training, we have the firm's report

(5.1) $y_i^f = x_i + u_i^f$

where y_i^f is the firm's report for the *ith* worker, x_i is the true value of the variable for the *ith* worker, and u_i^f is a random error term. The survey also provides the worker's report

(5.2) $y_i^w = x_i + u_i^w$

where y_i^w is the *ith* worker's report and u_i^w is a random error term. We assume that the two random error terms are uncorrelated with each other and x_i. We allow the distributions of the error terms to contain mass points at zero, which would allow for the worker and firm measures to agree with positive probability. We do not require that error terms have zero mean, and so the measures may be biased. In such a model, the correlation coefficient between the worker and firm reports gives a type of signal-to-noise ratio; as the variance of the worker's or firm's report increases, the correlation coefficient declines. In the limiting case of degenerate distributions for both the worker's and firm's error terms, the correlation coefficient would be one.

We may subtract equation (5.2) from equation (5.1) to obtain

(5.3) $\Delta yi \equiv y_i^f - y_i^w = u_i^f - u_i^w \equiv \Delta u_i.$

We may use a paired t-test to test the hypothesis that $E(\Delta u) = 0$. Conditional on $\Delta y_i \neq 0$, the Wilcoxon rank test examines the hypothesis that the error terms from the firm's and worker's measures are drawn from the same distribution, and similarly, the sign test examines if the median of the distribution of differences is zero.

To summarize differences in employer and employee reports and to facilitate comparisons of the Upjohn Institute data to other matched data sets, we present in table 5.1 a series of means, correlations, and significance tests of differences between employer and employee reports on a number of demographic, human capital, wage, fringe benefit, and productivity variables. For binary variables, in addition to the employer and employee means and the correlation, we show the percentage agreements and disagreements and the results of a test of equality of means. For continuous variables, we show means, correlations, and the results of three tests of differences in the employer and employee reports.

There is little disagreement between firm and worker race reports. Only 2.73 percent of workers and firms disagree about whether or not a worker is nonwhite and the correlation between the responses is over 0.9. We next consider employer and employee responses on whether the worker is covered by a collective bargaining agreement. The simple correlation between the employer and employee responses is 0.689, and the means are similar: employers report 10.5 percent of workers are covered and employees report 9.7 percent. Because the distributions are binomial, we use Fisher's exact test to determine if the reported rate of unionization is the same; the one-tail p-value from Fisher's test is 0.594, so we cannot reject the hypothesis that the rates are the same. From the 1977 January CPS employee-employer match, Mellow and Sider (1983) report that 7.1 percent of the employees disagree with their employers on their union status. Using a somewhat smaller sample from the same data set, Freeman (1984) finds only 3.45 percent of the employees disagree with their employers on union status. In the Upjohn Institute survey, 5.65 percent of the employees disagree with their employer on collective bargaining coverage. Thus, the Upjohn Institute data appears to compare favorably with the matched 1977 January CPS data about union coverage.

Variables 3 through 10 in table 5.1 show employer and employee responses to a series of schooling completion questions. There is a fair amount of disagreement between employers and employees, and it is especially dramatic in some of the schooling categories. College attendance, in particular, shows a massive amount of disagreement. Almost 42 percent of the observations disagree on whether the worker attended college! The correlation between the employer and employee's reports is only 0.186. Firms are almost twice as likely to report that the worker has attended, when the worker says he or she did not attend, as are the

Table 5.1 Correlation between Worker and Firm Responses

	(1) Nonwhite status	(2) Union status	(3) High school dropout	(4) High school diploma	(5) G.E.D.	(6) vocational or technical school
Firm mean	0.206	0.105	0.045	0.905	0.049	0.146
Worker mean	0.210	0.097	0.030	0.864	0.106	0.192
Correlation	0.917	0.689	0.598	0.588	0.536	0.459
Percentage disagreements	2.73%	5.65%	3.03%	8.71%	7.31%	15.42%
Percentage agreements	97.27%	94.35%	96.97%	91.29%	92.69%	84.58%
Test of equality of means (p-value)	0.706	0.594	0.158	0.022	0.001	0.071
N	257	248	264	264	246	240
Wilcoxon-Rank Test (significance level)	-	-	-	-	-	-
Sign Test Significance Level (two-tailed binomial test)	-	-	-	-	-	-
Paired t-test (significance level)	-	-	-	-	-	-

(continued)

Table 5.1 (continued)

	(7) Attended college	(8) Earned associate degree	(9) Earned bachelor's degree	(10) Attended graduate school	(11) Age	(12) Relevant experience (in months)
Firm mean	0.553	0.175	0.304	0.065	31.458	5.284
Worker mean	0.414	0.179	0.279	0.056	31.611	6.954
Correlation	0.186	0.546	0.861	0.850	0.961	0.727
Percentage disagreements	41.80%	13.25%	5.84%	1.73%	-	-
Percentage agreements	58.20%	86.75%	94.16%	98.27%	-	-
Test of equality of means (p-value)	0.001	0.860	0.109	0.318	-	-
N	244	234	240	231	218	248
Wilcoxon-Rank Test (significance level)	-	-	-	-	0.40 (0.6868) (N=115)	-5.90 (0.0001) (N=181)
Sign Test Significance Level (two-tailed binomial test)	-	-	-	-	0.0150 (N=115)	0.0000 (N=181)
Paired t-test (significance level)	-	-	-	-	-0.83 (0.4060)	-5.35 (0.0000)

	(13) Starting wage	(14) Wage after two years	(15) Hours worked	(16) Health insurance (initially)	(17) Health insurance (after two years)	(18) Paid vacation (initially)
Firm mean	8.95	10.00	36.98	0.385	0.880	0.331
Worker mean	8.84	10.84	38.50	0.486	0.928	0.350
Correlation	0.974	0.828	0.769	0.590	0.469	0.247
Percentage disagreements	-	-	-	21.01%	9.57%	33.84%
Percentage agreements	-	-	-	78.98%	90.43%	66.16%
Test of equality of means (p-value)	-	-	-	0.0004	0.025	0.597
N	210	153	263	257	209	263
Wilcoxon Rank Test (significance level)	1.06 (0.2845) (N=86)	-4.41 (0.0001) (N=148)	-4.73 (0.0001) (N=113)	-	-	-
Sign Test Significance Level	0.450 (N=86)	0.0002 (N=148)	0.0000 (N=113)	-	-	-
Paired t-test (significance level)	1.31 (0.1932)	-3.36 (0.0010)	-3.95 (0.0001)	-	-	-

(continued)

Table 5.1 (continued)

	(19) Paid vacation (after two years)	(20) Eligible for sick pay (initially)	(21) Eligible for sick pay (after two years)	(22) Reitrement plan (initially)	(23) Retirement plan (after two years)	(24) Child/elderly care (initially)
Firm mean	0.192	0.395	0.756	0.289	0.730	0.086
Worker mean	0.916	0.472	0.831	0.362	0.772	0.113
Correlation	0.490	0.294	0.428	0.312	0.327	0.298
Percentage disagreements	7.98%	35.08%	19.30%	30.60%	25.32%	12.67%
Percentage agreements	92.02%	64.92%	80.70%	69.40%	74.68%	87.33%
Test of equality of means (p-value)	0.828	0.0414	0.0064	0.0434	0.1973	0.258
N	263	248	254	232	237	221
Wilcoxon Rank Test Isignificance level)	-	-	-	-	-	-
Sign Test Significance Level	-	-	-	-	-	-
Paired t-test (significance level)	-	-	-	-	-	-

	(25) Child/elderly care (after two years)	(26) Life insurance (initially)	(27) Life insurance (after two years)	(28) Disability (initially)	(20) Disability (after two years)	(30) Profit sharing (initially)
Firm mean	0.162	0.448	0.841	0.433	0.725	0.122
Worker mean	0.281	0.556	0.841	0.585	0.812	0.187
Correlation	0.252	0.516	0.508	0.283	0.206	0.197
Percentage disagreements	26.87%	25.10%	13.18%	37.33%	28.75%	21.30%
Percentage agreements	73.13%	74.90%	86.82%	62.67%	71.25%	78.70%
Test of equality of means (p-value)	0.0035	0.0007	1.0000	0.0002	0.0386	0.0318
N	160	239	182	217	160	230
Wilcoxon Rank Test Isignificance level)	-	-	-	-	-	-
Sign Test Significance Level	-	-	-	-	-	-
Paired t-test (significance level)	-	-	-	-	-	-

(continued)

Table 5.1 (continued)

	(31) Profit sharing (after two years)	(32) Discounts (initially)	(33) Discounts (after two years)	(34) Productivity in first two weeks (relative to fully trained worker)	(35) Producitivity in second two weeks	(36) Probability of promotion within two years
Firm mean	0.413	0.517	0.583	57.42	76.64	25.77
Worker mean	0.468	0.492	0.574	58.09	80.35	45.61
Correlation	0.426	0.468	0.509	0.312	0.382	0.259
Percentage disagreements	28.51%	26.66%	23.97%	-	-	-
Percentage agreements	71.49%	73.34%	76.03%	-	-	-
Test of means (p-value)	0.1124	0.4544	0.7935	-	-	-
N	235	240	242	227	218	211
Wilcoxon-Rank Test (significance level)	-	-	-	-0.55 (0.5845) (N=205)	-2.57 (0.0101) N=190)	-6.58 (0.0001) (N=183)
Sign Test (significance level)	-	-	-	1.000 (N=205)	0.0045) (N=190)	0.0000 (N=183)
Paired t-test (significance level)	-	-	-	-0.35 (0.7293)	-2.59 (0.0101)	-7.13 (0.0000)

workers to report attendance when the firm does not. Not only does this discrepancy affect measurement of schooling completion levels, but it may also have some effect on the estimated returns to schooling.

The employer and employee mean ages are almost identical. Workers report 31.61 years and firms report 31.46 years (Variable 11). The correlation between the two is 0.961 (n=218). Interestingly, conditional on disagreement, the sign test that the two variables have the same median can be rejected at the 1.5 percent confidence level (n=115); workers report that they are younger than firms believe. Thus, despite a high correlation and virtually identical means, the sign test cautions us against accepting the hypothesis that the two error terms have the same median.

Variable 12 provides reports of relevant experience. Workers report more relevant experience than do firms, and the correlation between the reports is 0.727. While this variable also appears in the EOPP and SBA surveys, it has not been included in household surveys. Given the relatively high correlation between worker and firm reports of relevant experience, this question may warrant inclusion in future household surveys.

Variables 13 and 14 show the mean values and correlations reported by employees and employers between the starting wages and predicted wages after two years. The mean starting wages are almost identical, and the correlation between the employer and employee reports is 0.974. This figure is significantly higher than the correlations reported by Bound, Brown, Duncan, and Rodgers (1990) and Rodgers, Brown, and Duncan (1993) using the PSID validation survey. Rodgers, Brown, and Duncan report a correlation for annual earnings of 0.792, and 0.601 and 0.456 for the previous pay period and the usual pay period, respectively. Using a slightly different sample from the same data set, Bound, Brown, Duncan, and Rodgers (1990) find a correlation of 0.806 for annual earnings, and 0.456 and 0.461 for the last pay period and the usual pay period.[2] Perhaps employees and employers disagree less when the worker has just begun employment. However, the correlation in the Upjohn Institute survey for the predicted wage in two years is 0.828, which is still higher than that obtained using the PSID validation survey.[3] The mean differences in starting wages also appear small in the Upjohn Institute survey. Workers underreport wages by $0.12, compared to figures of $0.63 and $0.66 reported by Duncan and

Hill (1985) for the first wave of the PSID validation survey. The mean differences for wages also appear to be in line with those obtained from the 1977 January CPS by Mellow and Sider (1983). Workers report higher predicted wages in two years by $0.84 when compared to their employers in the Upjohn Institute data. These numbers, however, do not represent actual wage data but rather predictions on the part of employers and employees about what will occur in two years. Overall, the differences in reported wages appear to be quite low in the Upjohn Institute survey.

Variable 15 shows that workers report on average 1.5 more hours worked (workers report 38.5 and firms report 37.0), and the correlation between worker and employer measures is 0.769 (n=263). Conditional on disagreement, a Wilcoxon signed rank test rejects the hypothesis that the two error terms are drawn from the same distribution with a *z*-statistic of 4.73 (n=113). Bound, Brown, Duncan, and Rodgers (1990) and Rodgers, Brown, and Duncan (1993) find that the log of hours during the previous pay period from administrative data and the log of usual hours from employee measures are correlated between 0.60 and 0.64 using the PSID validation survey. When the Upjohn Institute hours of work data are converted to logs, the correlation between employer and employee measures is 0.61, essentially identical to the correlation obtained from the PSID validation study. Mellow and Sider (1983) report that the mean difference in the log of hours worked is 0.039. We find that workers overreport the log of hours worked by a somewhat smaller amount (0.031).

Variables 16 through 33 show employer and employee reports of various components of the fringe benefit package. The correlations range from 0.197 for the initial provision of profit sharing to 0.59 for initial eligibility for health insurance. Duncan and Hill (1985) report on the level of agreement between employees and administrative records on various components of the fringe benefit package. These differences only come from employee errors and not employer errors because of the use of administrative records in the PSID validation survey. On the other hand, the Upjohn Institute survey contains disagreements resulting from errors on the part of both employers and employees. In addition, the PSID only surveyed a single manufacturing firm, where workers are likely to have long tenure with the company, while the Upjohn Institute surveyed newly hired workers. For these reasons, there is more agreement about fringe benefit provision in the PSID validation survey than in the Upjohn Institute survey.

Only 1 percent of the employees in the PSID validation study disagree with their employers on the provision of health insurance. On the contrary, 21 percent of employees in the Upjohn Institute survey disagree about the provision of health insurance at the beginning of employment and 9.6 percent disagree about health insurance provision after two years. The level of agreement after two years is probably more meaningful because many workers may be confused about when health insurance coverage starts.

In the PSID validation survey, 9 percent of the employees disagree with their employer about the provision of sick pay, and 10 percent disagree about life insurance. This compares to 35 percent and 30 percent in the Upjohn Institute survey at the start of employment and 19 percent and 13 percent after two years. Only 1 percent and 3 percent of the employees disagree with their employer about the provision of paid vacation days and a retirement plan in the PSID validation survey, while 34 percent and 31 percent disagree with their employer in the Upjohn Institute survey at the beginning of employment and 8 percent and 25 percent disagree after two years with the employer.

While there is a large amount of agreement between employers and employees on well-measured variables such as age, race, gender, starting wages, and completion of a bachelor's degree, there is less agreement on variables more difficult to measure, such as attendance of college, and for variables about which the newly hired worker is not likely to know, such as fringe benefits. As a whole, the responses we received are more or less comparable with those found in the literature.

The Upjohn Institute survey also asked several more subjective questions. For example, we asked the employer and new employee to rate on a scale of 0 to 100 the employee's productivity relative to a fully trained worker in the first two weeks of employment and after the second two weeks of employment. Variable 34 shows the reported productivity during the first two weeks, and Variable 35 shows the reported productivity at the end of the second two weeks. The mean responses for productivity during the first two weeks are almost identical, yet the correlation is only 0.312. The correlation increases slightly to 0.382 for the second two weeks, and workers report somewhat higher productivity on average. Variable 36 shows that workers report a much higher probability of promotion in the first two years than do firms for typical workers in the same position, and the correlation between responses is 0.259.

Measures of On-the-Job Training

We now turn to table 5.2, in which we report the employer and employee measures on training. For each type of training, we report the firm mean, the worker mean, the correlation between the two measures, the Wilcoxon signed rank test, the sign test, and the paired *t*-test. In general, firms report more training than workers. Employers report more on-site training (*t*-statistic of 2.32), informal management training (*t*-statistic of 0.56), informal co-worker training (*t*-statistic of 3.06), and "watching others" (*t*-statistic of 1.55). Workers report more off-site training (*t*-statistic of 1.46). The Wilcoxon test, which does not require the normality assumption of the *t*-test, indicates the only significant differences are for on-site formal training and informal co-worker training, although the sign test cannot reject the hypothesis that the median of on-site formal training is the same for workers and firms.[4] While all the correlations are statistically significant, they are surprisingly low. The formal training measures have a correlation of about 0.4. Informal management training has a correlation of only 0.176. Co-worker training and "watching others" have correlation coefficients of 0.379 and 0.287, respectively. When we aggregate the five different variables into a single training measure, the correlation increases to 0.475. Firms report nearly 25 percent more training than workers (*t*-statistic of 2.72 and a Wilcoxon *z*-statistic of 2.73).

The Upjohn Institute data also contain PSID-like questions that measure the time to become fully trained and qualified. Unlike the PSID, however, we asked the firm and worker to evaluate how long it would take a worker with no experience to become fully trained and qualified, which is the same question asked by both the SBA and EOPP data. Of course, workers are being asked to evaluate how long it will take to become fully trained and qualified after only four weeks on the job. The means of the two distributions are quite similar (*t*-statistic of 0.44), and, indeed, the means are quite similar to those we reported

Table 5.2 Employer and Employee Measures of Hours of Training

	Hours of on-site formal training	Hours of off-site formal training	Hours of informal, managerial training	Hours of informal, co-worker training	Hours of training by watching others	Total hours of training	Time to become fully trained (weeks)
Firm mean	9.31	1.64	26.90	26.31	24.48	87.50	18.88
Worker mean	6.06	2.45	25.56	19.67	20.54	71.83	20.99
Correlation	0.398	0.457	0.176	0.379	0.287	0.475	0.172
N	248	251	219	216	209	179	222
Wilcoxon-Rank Test	1.98	-1.35	0.89	3.13	1.46	2.73	2.48
(significance level)	(0.0475	(0.1774)	(0.3709)	(0.0017)	(0.1433)	(0.0064)	(0.0131)
	(N=103)	(N=38)	(N=211)	(N=192)	(N=186)	(N=177)	(N=208)
Sign Test Significance Level	0.1145	0.4177	0.0732	0.0048	0.2125	0.0350	0.0313
	(N=103)	(N=38)	(N=211)	(N=192)	(N=186)	(N=177)	(N=208)
Paired *t*-test (significance level)	2.32	-1.46	0.56	3.06	1.55	2.72	-0.44
	(0.0210)	(0.1465)	(0.5793)	(0.0025)	(0.1266)	(0.0072)	(0.6630)

in the SBA and EOPP data in chapter 3. The employer and employee responses, however, have a correlation of only 0.172, and, when we test the hypothesis that the two are drawn from the same distribution, the Wilcoxon *z*-statistics lead us to reject that hypothesis. Similarly, the sign test rejects the hypothesis that the two distributions have the same median. The difference between the nonparametric Wilcoxon and sign tests and the parametric *t*-test suggests that there are several large outliers, and when we repeated the *t*-test with observations that differed by less than 48 months, the *t*-test indicated that firms reported a longer time to become fully trained and qualified (*t*-statistic of 2.90).

One may be tempted, given the differences in the correlations among the formal and informal training measures, to conclude that we measure formal training more accurately than informal training. This is not the case. In table 5.3, we aggregate the training measures into a formal and informal training measure. The correlation coefficients are very similar (0.419 for formal training and 0.408 for informal training). While the difference between firm and worker mean measures is statistically significant for informal training and is not significant for formal training at the 5-percent level, firms report 28.9 percent more formal training and "only" report 19.3 percent more informal training. Thus, this experiment does not offer much evidence for the belief that informal training is more difficult to measure than formal training.

Table 5.3 Employer and Employee Measures of Hours of Formal and Informal Training

	Hours of formal training	Hours of informal training
Firm mean	10.66	77.44
Worker mean	8.30	64.67
Correlation	0.419	0.408
N	245	184
Wilcoxon Rank Test (significance level)	1.10 (0.2713) (N=106)	2.65 (0.0081) (N=182)
Sign Test Significant Level	0.2065 (N=106)	0.0451 (N=182)
Paired *t*-test (significance level)	1.55 (0.1220)	2.34 (0.0206)

Table 5.4 Employer and Employee Measures of Training Incidence Rates

	On-site formal training	Off-site formal training	Management training	Co-worker training	Training by watching others
Firm mean	0.310	0.084	0.954	0.842	0.847
Worker mean	0.270	0.120	0.900	0.819	0.790
Correlation	0.318	0.377	-0.0731	0.227	0.269
Percentage disagreements	28.23%	11.56%	14.61%	21.76%	22.01%
Percentage agreements that training occurred	14.92%	84.06%	85.39%	72.22%	70.81%
Percentage agreements that training did not occur	56.85%	4.38%	0.00%	6.02%	7.18%
Test of equality of means (p-value)	0.2327	0.0947	0.0336	0.4671	0.0768
N	248	251	219	216	209

Because many survey instruments are concerned with the measurement of incidence rates of training, in table 5.4 we present statistics for these rates. For each of the five training measures, we list the firm mean, the worker mean, the correlation between the firm and worker measures, the percentage of disagreements between the firm and worker measures, the percentage of time that the firm and worker agree that the worker received the training, and the *p*-value for Fisher's exact test of the equality of means for binomial variables. It is important to note that firms and workers agree that the incidence of informal training is quite high. The lowest rate of agreement is for "watching others," and nearly 71 percent of the sample agree that the worker was trained by watching others. These rates are similar to those reported in chapter 3 for the EOPP and SBA data and offer further evidence that the incidence rate of informal training is extremely high for new hires. Indeed, there are no observations for which worker and firm agree that the worker did not receive informal management training, and there are only 13 observations that agree there is no informal co-worker training. The means are similar for each of the five training incidence rates. Thus, although not highly correlated, firm and worker measures give very similar estimates of the incidence rates.

Determinants of Training and Reported Differences in Training

Our survey data suggest significant deviations in worker- and firm-reported training measures. While we cannot examine measurement error directly, we can examine the determinants of differences in worker- and firm-reported training. In table 5.5, we regress the log of the total hours of worker- and firm-reported training in the first four weeks of employment and deviations in reported training on a series of worker- and firm-reported variables. Columns 1 and 2 contain regressions explaining the amount of training, and columns 3 and 4 contain regressions explaining differences in reported training. In column 1, using firm-reported training and independent variables, there is some evidence that high school dropouts receive less training than high school graduates. In column 2, using worker-reported training and independent variables, there are no significant determinants of train-

Table 5.5 Regression Explaining Levels of Training and Worker-Firm Differences in Reported Training

Independent variables	Levels of training (1)[a]	Levels of training (2)[b]	Worker-firm differences in training (3)[c]	Worker-firm differences in training (4)[d]
log (age)	0.339	0.117	0.340	-0.340
	(1.01)	(0.334)	(0.781)	(0.850)
log (relevant experience)	-0.130	-0.119	-0.016	0.127
	(1.22)	(1.18)	(0.122)	(1.08)
High school dropout	-0.844	0.396	0.189	0.377
	(1.98)	(0.719)	(0.369)	(0.599)
Some college	0.307	-0.023	-0.628	-0.244
	(1.45)	(0.103)	(2.39)	(0.941)
College graduate	0.188	-0.172	-0.682	0.376
	(0.786)	(0.689)	(2.35)	(1.27)
Worker is black	0.385	0.480	0.111	0.492
	(1.31)	(1.70)	(0.288)	(1.450)
Worker is nonblack female	0.0030	0.044	0.167	0.0087
	(0.016)	(0.251)	(0.740)	(0.041)
Worker is covered by collective bargaining	-0.161	0.052	-0.338	0.121
	(0.628)	(0.194)	(1.10)	(0.409)
log (establishment size)	0.115	0.034	-0.084	0.048
	(1.66)	(0.483)	(0.935)	(0.583)

(continued)

Table 5.5 (continued)

Independent variables	Levels of training (1)[a]	Levels of training (2)[b]	Worker-firm differences in training (3)[c]	Worker-firm differences in training (4)[d]
log (number of firm employees at other establishments)	0.031	0.031	0.0338	0.074
	(0.501)	(0.488)	(0.446)	(1.02)
log (hours worker per week)	0.095	-0.019	-0.272	-0.500
	(0.348)	(0.100)	(0.756)	(1.67)
Constant	1.94	3.463	0.314	2.28
	(1.37)	(2.44)	(0.168)	(1.25)
R^2	0.099	0.036	0.110	0.081
N	152	206	128	157

a. The dependent variable is the log of total hours of firm-reporting training plus one. The independent variables are firm reports.

b. The dependent variable is the log of total hours of worker-reported training plus one. The independent variables are worker reports except for the log of establishment size that was asked of firms.

c The dependent variable is the log of worker-reported training plus one minus the log of firm reported training plus one. The independent variables are firm reports.

d. The dependent variable is the log of worker-reported training plus one minus the log of firm reported training plus one. The independent variables are worker reports except the log of establishment size that was asked of firms.

ing.[5] These differ greatly from the SBA and EOPP training regressions in chapter 4 in which prior experience, establishment size, and schooling were all significantly related to the amount of training. One important difference is that the Upjohn Institute training measures include only training in the first month and not in the first three months. In addition, the Upjohn Institute sample sizes are much smaller, making it more difficult to find significant relationships between training and other variables.[6]

Columns 3 and 4 show the regressions explaining log differences in worker- and firm-reported training. There is some evidence using firm-reported characteristics that the difference between worker- and firm-reported training is negatively related to the level of schooling. No variables are significantly related to the difference in reported training when worker-reported characteristics are used.

Given the lack of any significant correlates of differences in reported training, it may be that the differences in employer- and employee-reported training result largely from measurement error. Hours of training may simply be difficult to estimate for the employee and the employer. If measurement error is present, then estimated effects of training on wages and productivity are biased toward zero (Greene 1993, p. 283). We investigate this issue further in chapter 6.

Conclusions

In this chapter we reported the results of a matched survey of employers and employees in which both employers and employees were interviewed three times over the course of a month. The survey focused on training activities of the last worker hired but also sought to obtain demographic information about the worker from both the firm and the worker.

There is very little difference in the quality of demographic information about the worker obtained from the firm and the worker. For variables such as race and age, the correlations between worker and firm reports are over 0.9. Surprisingly, given that previous studies have obtained much lower correlations for wages, the correlation between worker- and firm-reported starting wages is 0.974. Lower levels of agreement between firm and worker reports are obtained for variables such as hours worked and union status; however, the correlations are in line with previous studies. The agreement of fringe benefits in the Upjohn Institute data are somewhat below previous estimates, perhaps because the Upjohn Institute survey focused on newly hired workers.

Correlations between worker and firm reports of training activities are in general lower than most other variables. There are no significant determinants of worker- or firm-reported total hours of training in the first month, except perhaps that those with very low schooling levels receive less training. Similarly, there are in general no significant determinants of differences in worker- and firm-reported hours of training in the first month. A significant amount of measurement error in the worker- and/or firm-reported training variables may in part explain these findings.

There are some conclusions that can be drawn from the analysis in this chapter about whether investigators should obtain information from the firm or from the worker. For many variables, such as relevant experience, the differences in the effects of firm-reported and worker-reported variables are slight. Since the correlations for many variables are fairly high, if firm information is unavailable, it should be obtained from the worker. For example, questions about previous experience are not often asked of the worker in household surveys, but the correlation with firm-reported previous experience is quite high. Workers have much more difficulty answering questions about specific elements of the fringe benefits package. While the correlation between worker- and firm-reported training is quite low, both are significantly related to productivity. It would be useful to have similar training and productivity questions appear in future household surveys.

We believe our analysis offers several insights into the measurement of on-the-job training. First, there is a great deal of measurement error in attempts to gauge the quantity of on-the-job training. Even using the aggregate measure of training, the correlation between worker and firm measures is less than 0.5, which is much lower than other variables that have been used in wage equations. Differences between firm and worker reports, however, appear uncorrelated with any of the normal variables used in wage equations. Second, firms report more training (about 25 percent more) than do workers. Heckman and Smith (1993) find evidence that workers underreport the incidence of training when comparing administrative and self-reported data for a group of Job Training Partnership Act recipients. Given our data, however, we cannot determine whether this difference arises because workers underreport training or firms overreport training (or both).

Third, both firm and worker measures indicate a large amount of informal training for newly hired workers. The incidence rate of each type of informal training exceeded 75 percent, and the mean number of hours for each type of informal training was 20 hours or more in the first four weeks of employment for both worker and firm measures. In contrast, formal training measures had relatively low incidence rates, and the mean hours of formal training in the first four weeks of training was about ten hours. This suggests that surveys such as the NLSY that focus only on formal training spells are missing a majority of the spells of training. Similarly, surveys such as the NLSHS72 and the CPS that ask retrospective questions about the informal training also appear to miss most of the spells of informal training.

Fourth, in surveys of newly hired workers, there is not much evidence that formal training is more accurately measured than informal training. When one aggregates the five measures of training into a formal training measure and an informal training measure, the correlations between worker and firm measures are almost identical for formal and informal training. Fifth, firm and worker measures of the time to become fully trained and qualified, at least for our sample of newly hired workers, have lower correlations than measures of training. And finally, the agreement between worker and firm measures is higher for the aggregate training measure than for any individual training measures. This suggests that, where possible, researchers may be better served using an aggregate measure than using each measure separately.

Among the findings cited above, one of particular significance concerns the size and reliability of informal training measures relative to formal training measures. The substantial magnitude of informal training suggests that the focus of the federal government on formal training may be misplaced. In fact, formal training, both as an activity to measure (e.g., the 1993 Survey of Employer-Provided Training funded by the Employment and Training Administration) and as an activity to fund (e.g., tax incentives for training personnel, instructional materials, and schools) appears less important than informal training.[7] If policy makers choose to subsidize formal training, they will ignore an overwhelming fraction of the training that workers receive. Moreover, as we demonstrated in chapter 4, small firms are less likely to utilize formal training than their larger counterparts. A subsidy of formal training programs, therefore, is an implicit decision to subsidize larger firms.

NOTES

1. If the establishment reported that they would be hiring in the near future, a callback was scheduled for the expected hiring date.

2. Rodgers, Brown, and Duncan (1993) and Bound, Brown, duncan, and Rodgers (1990) report correlations between log earnings. In the Upjohn data, the correlation between the logs of the employer- and employee-reported starting earnings is 0.980.

3. The correlation between the logs of the predicted wage after two years as reported by the employer and employee in the Upjohn data is 0.842.

4. We perform the Wilcoxon sign rank test and the sign test on those pairs in which the worker's and firm's reported disagree.

5. The worker has not asked about the establishment size so the firm-reported establishment size is used in all of the regressions reported in the paper.

6. Another important difference is that the Upjohn Institute training measure includes off-site formal training while the SBA training measure excludes off-site formal training. The SBA data report significantly different effects of off-site training from other types of training in wage and productivity equations. We find no evidence of different effects of off-site formal training using the upjohn Institute data and therefore aggregate all types of training into a single measure.

7. The 1993 Survey of Employer-Provided Training is a survey of about 12,000 employers that attempts to measure only the incidence of formal training programs.

CHAPTER 6

The Impact of Training on Wages and Productivity

Economists have long believed that on-the-job training is an important determinant of the structure of wages. Since the seminal work of Mincer (1962), economists have attributed the growth in wages associated with increases in the labor market to rising productivity generated by on-the-job training. Moreover, researchers have nearly always found that wages increase with a worker's tenure at the firm.[1] Economists have generally interpreted this return to tenure as evidence of firm-specific training; as noted in chapter 2, on-the-job training theory justifies such an interpretation. Consider two workers with the same years of experience in the labor market, but the first worker has been at his or her current firm for a year while the second worker has just started at a new firm. As both workers have the same total labor market experience, they should have accumulated similar general human capital. If the first worker has accumulated some firm-specific training, the theory of on-the-job training suggests that he or she should receive some of the returns to that training.

In the first section of this chapter, we review two theories that challenge the traditional training view of the source of wage growth: the incentive-based compensation models and the learning/job matching models first discussed in chapter 2. We note that, while these theories may contribute to the links between wage growth and work experience, available evidence indicates that on-the-job training remains an important determinant of wage growth. However, establishing the predicted effects of on-the-job training can be difficult at times. The second section of this chapter illustrates some of the difficulties in testing the prediction that on-the-job training reduces a worker's starting wage. In contrast, the next two sections reaffirm the predicted impact of training on wage and productivity growth, although even here questions arise because investments in training appear overwhelmingly to be financed by employers. We then examine the effect of measurement error on the estimated effects of training wages and productivity. The final section reviews various modifications to the simple on-the-job theory that may explain the apparent small impact of training on starting wages.

Alternative Theories of Wage Growth

There have been significant challenges to the traditional interpretation that employee wage growth reflects a return to on-the-job training. For instance, Lazear (1979, 1981) offers an incentive-based compensation model where wage profiles slope upward to guard against worker shirking. In essence, the worker posts a bond with the firm by agreeing to work for a low wage early in his or her career in return for higher wages later in the career. If workers shirk and the firm fires them, they lose the high wages or pension payments they would have otherwise received. Thus, it is possible for wages to increase with tenure without any on-the-job training. Moreover, Lazear's theory can explain the existence of several other features of labor contracts that are difficult to explain using human capital theory. For instance, firms restrict the number of hours many employees can work. But, if firms are paying these workers the value of their marginal products or perhaps less because the workers and firms share the returns to specific training, firms should be content to let these employees work more hours. In contrast, in Lazear's theory, firms would not want senior employees to work more hours because they are receiving a wage more than the value of their marginal products to compensate them for accepting the low initial wages.

Taking a different approach, Johnson (1978) and Jovanovic (1979a) have constructed job-matching models that may account for the correlation of wages and job tenure. We can capture the essence of the job-matching model in a simple two-period example. Consider a model where a worker's productivity is given by

(6.1) $p_i = p^0 + \varepsilon$

for $t = 1, 2$, where p^0 is the worker's expected productivity across firms and ε is a random variable with $E(\varepsilon) = 0$. The term ε is a match-specific error term that neither the firm nor worker knows before employment. During the first period, the firm and worker learn the value of ε. Because ε is not initially known, a firm, competing with other firms for workers, will offer a first period wage equal to the worker's expected productivity, or $w_1 = p^o$. In the second period, however, both the worker and the firm know the true value of ε. If changing jobs costs nothing, when $\varepsilon < 0$ it is inefficient for the match to continue because the worker's expected productivity elsewhere is p^o. The worker and firm will agree to terminate the relationship, and the worker will earn the value of his or her expected productivity, or p^o. If $\varepsilon > 0$, it is efficient for the match to continue and presumably the firm and the worker will do so. For ease of exposition, let us assume that the firm pays the worker the value of his or her marginal product, or $w_2 = p^o + \varepsilon$.

In the above example, workers who change employers receive the wage p^o, while workers who remain at their previous period employer receive the expected wage $p^o + E(\varepsilon \mid \varepsilon > 0) > p^o$. Thus, seniority (tenure) and wages are positively correlated. Yet, this in no way reflects a return to firm-specific training but rather demonstrates that good matches survive. While we may add many complications to make this example more realistic, the example does illustrate the essential insight of the job-matching literature: when good matches survive and bad matches do not, there may be a return to tenure that is a "statistical artifact."[2] While both the worker-shirking model and the job-matching model offer alternative explanations of the correlation between wages and tenure, neither theory offers a particularly compelling explanation for the correlation of labor market experience and wages. These models do suggest, however, that special care must be used when trying to measure the impact of training on wages.

Fortunately, there have been numerous studies that have used direct measures of on-the-job training to examine the impact of that training on wages; see Mincer (1989a) for a review. Mincer reports that even when allowing for a 15 percent depreciation rate, the returns from training range from 10.5 to 25.6 percent in the five studies that he reviews. If some of this training is firm-specific and if firms share in some of the returns to specific training, these rates of return understate the return to the economy as a whole.[3]

On-the-Job Training Effects on the Starting Wage

While research has typically confirmed the predicted impact of training on wage growth, the second major prediction of on-the-job training theory has proven more difficult to confirm. In fact, most previous research has failed to find the negative relationship between training and the starting wage predicted by this theory. Barron, Black, and Loewenstein (1989), using the EOPP data, found no statistically significant relationship between the starting wage and the quantity of training that a worker receives. As we discuss in more detail below, they argue that lack of a negative relationship between training and the starting wage may be the result of productivity differences that are not observed in the data. If high-ability workers are matched to jobs that require much training and those high-ability workers command a wage premium, there may be a spurious correlation between training and the starting wage.

Difficulties in Testing the Starting Wage Prediction Given Heterogeneous Labor

To see how heterogeneity across workers introduces difficulties in testing on-the-job training's predictions concerning the starting wage, consider the following wage equation:

$$(6.2) \quad \ln(w) = X\beta + \gamma \ln(T) + \alpha + \varepsilon$$

where w is wages, X is a vector of firm and worker characteristics, T is the quantity of training that the worker receives, α is a measure of the worker's ability not captured in the data, ε are the standard error terms, and β and γ are parameters to be estimated. If the labor market matches workers with large α's to jobs with high levels of required human capital, the coefficient γ will be biased upward. This problem is probably unavoidable in cross-sectional data. For instance, consider a class of graduating seniors majoring in economics from the same university. From the standpoint of most standard cross-section data sets, this is a remarkably homogeneous group. They all have the same level of education, their college major is the same, and they have graduated from the same university. Yet, we would hardly expect earning differences among this group to be completely random. Employers would have a wide range of information about these prospective employees that are generally unavailable to the econometrician. For instance, employers will have access to certain quantifiable measures such as grade point average, test scores, and past job earnings. In addition, employers will have access to subjective information such as workers' appearances, skills in handling interviewers' questions, personality traits, recommendation letters, and other informal sources of information.

Despite the homogeneity of this group, we would not be surprised to learn that there are substantial earnings differentials among the graduates. If training and worker ability are complements in the production process so that high-ability workers have larger increases in productivity for a given level of training than do low-ability workers, then we would expect the "good" economics majors from the university to hold jobs that have more training than their less able classmates. Although the more able students receive more training, we still might find that they are paid a higher starting wage than the less able if the wage premium for their superior abilities exceeds their portion of the training costs.

To buttress the case that unobserved ability differentials may explain the lack of a negative correlation between initial wages and training, chapter 7 documents that firms look at more applicants and spend more time evaluating applicants when filling jobs that require more training. While this evidence is consistent with the lack of correlation, others disagree. Parsons (1989), using the NLS youth cohort, finds that there is a positive relationship between training and the starting wage, but the relationship is generally not statistically significant. As the NLS youth cohort is a panel data set with a much richer collection of worker characteristics (including their Armed Services Vocational Aptitude Battery scores) than the SBA and EOPP data, Parsons (1990) argues that the improved controls suggest that unobserved heterogeneity may not explain the failure of starting wages to be negatively correlated with training. Similarly, Lynch (1992), using a sample from the NLSY data of people who had attended college but did not graduate, reports that uncompleted spells of training are positively associated with the higher wages, suggesting that workers are not bearing the costs of training. While Lynch does find some differences by the level of education, there is little evidence in her study that training lowers the starting wage.[4] If the panel data do offer substantially more controls for worker abilities than do cross-sectional data, the failure of researchers to find a negative correlation between the starting wage and training represents a serious challenge to traditional on-the-job training theory.

A slightly more complicated model than equation (6.2) will demonstrate, however, the inherent difficulty in testing this proposition, even with panel data. Suppose that the wage equation is of the form

$$(6.3) \quad \ln(w) = X\beta + \gamma \ln(T) + \alpha\,\eta + \varepsilon$$

where α is again the worker's unmeasured ability, η is a measure of the total human capital of the job, and the other variables are as before. The term η reflects the intrinsic human capital necessary to do the job, and, presumably, η is highly correlated with the quantity of training that workers receive. Of course, if we hold η fixed, variation in the workers' experience, schooling, and other forms of human capital will affect the quantity of training that workers receive, so η and training are not perfectly correlated.

Suppose that workers and firms learn about the true values of α over time. Thus, workers moving from low-training to high-training jobs (or more precisely, from low-η to high-η jobs) are more likely to be high-ability workers. If the labor market matches high ability workers to more complex jobs, then, as we discussed in chapter 2, even first "differencing" the equation does not necessarily remove the bias from the estimated coefficients for

$$(6.4) \quad \Delta\ln(w) = \Delta X\beta + \gamma \ln(T) + \alpha\Delta\eta + \Delta\varepsilon$$

where Δ preceding the variables reflects the changes in those variables. We have assumed for convenience that the worker's previous job required no on-the-job training. High-ability workers will tend to have larger changes in η than low-ability workers. As unobserved ability is positively correlated with increases in η, and training is positively correlated with increases in η, then the spurious correlation between training and the wage will not be eliminated unless we have accurate measures of η over the worker's job history, which to our knowledge no panel data set contains. Workers will still have to finance their part of the training costs through accepting lower wages than firms would have paid to workers requiring less training, but the matching of high-ability workers to more complex jobs will hide the negative relationship between training and the starting wage.

An Intrafirm Test of the Starting Wage Prediction of On-the-Job Training

Fortunately, the SBA data allow a direct test of the proposition that workers finance a part of their training cost through accepting a lower starting wage. As part of the survey, we asked whether the worker had more training, less training, or the same training as the typical worker hired into the position. In addition, we asked if the worker was paid more than, less than, or the same as the typical worker hired into the position. Human capital theory predicts that workers receiving more training than average should receive lower wages than the typical worker hired into the position, and those workers receiving less training than average should receive higher wages than the typical worker hired into the position. In table 6.1, we report cross tabulations for the responses to these two questions. There are several interesting observations. First, 21.8 percent of the sample were paid a wage higher than the typical wage whereas only 2.6 percent of the sample were paid a wage lower than the typical wage. Similarly, many more workers received less training than the typical worker (25.7 percent) than received more training than the typical worker (6.9 percent). These figures indicate that respondents' determination of the typical worker is biased; the worker identified as a "typical" worker is actually paid less and requires more training than the median worker. Of the small segment of the sample who received more training than the typical worker, more workers (16.5 percent) received wages higher than normal than received lower wages than normal (7.1 percent). Thus, there appears to be little evidence in column (1) to support the notion that workers are paying for their training when they receive more training than the typical worker.

When workers receive less training than the typical worker, which occurs 25.8 percent of the time, they are much more likely to receive higher than typical wages; 48.3 percent of these workers receive higher wages compared to only 12.2 percent of the workers who receive the typical amount of training. This suggests that while firms are unwilling to penalize workers who require more training than the typical new employee, they are willing to (or must) increase the wages of well-trained workers who need less training than the typical new employee. Moreover, firms' hiring decisions seem to mirror this form of rent sharing. They are much more likely to hire workers who require less training than they are to hire workers who require more training than the typical new worker. As firms appear reluctant to pass the increased training costs on to workers by lowering wages, it is not surprising that they would not hire workers needing more training than the typical worker.[5]

Table 6.1 Training and Starting Wage, 1992 SBA Data (percent)

	More training than typical worker	Same training as typical worker	Less training than typical worker	Total
Higher wage than typical worker (n=268)	16.5	12.2	48.3	21.8
Same wage as typical worker (n=931)	76.5	85.6	49.2	75.6
Lower wage than typical worker (n=32)	7.1	2.2	2.5	2.6
Total[a]	100.0	100.0	100.0	100.0
Sample size	85	829	317	1231

a. Totals may not add to 100 percent due to rounding.

A flaw in the above analysis is that it does not consider interfirm variation in wages and training. For instance, certain employers hiring clerks may specialize in hiring workers without much experience in the field, but they may offer these inexperienced workers a great deal of training. Other firms may only hire clerks with a great deal of experience who require very little training. While all workers hired at either firm may be offered the same wage, wages may differ between the firms, with the firm offering a great deal of training providing a relatively low starting wage. This high-training firm will not be able to attract workers with a great deal of experience because of the low starting wage. The low-training firm will offer a relatively high starting wage but may not be willing to hire workers without substantial experience. Thus, in the labor market as a whole, the predicted trade-off between the starting wage and training would exist, but we may not see this relationship if we look only at relative wages within the firm.

An Interfirm Test of the Starting Wage Prediction of On-the-Job Training

The prior discussion relied on firms' characterizations of a worker's wage and training relative to the "typical" worker to test for the expected trade-off between training and the starting wage. In this section, we use the 1992 SBA data set to test whether differences in training across firms explain differences in starting wages. The SBA survey differs from most other data sets because of its focus on newly hired workers. To help the reader compare the SBA data with other data sets, table 6.2 presents estimates of a wage equation without the measures for training but includes many of the standard controls that economists use in wage equations: age, age squared, experience, experience squared, years of education, the logarithm of the size of the establishment, hours worked, and hours squared. In addition, we use dummy variables indicating the worker's union status and whether the worker is black or a nonblack female.[6] The reader will recall that our measure of experience is the employer's estimate of the number of years of relevant experience that a worker possesses. The results offer few surprises. Experience has a concave relationship, as do age and hours worked. More highly educated workers earn more, with one additional year of schooling increasing wages by about 10 percent. Black workers earn about 23 percent less than nonblack males, and females earn about 17 percent less than nonblack males.

In the appendix to this chapter, we provide a detailed comparison of estimates from similarly specified wage equations using the 1992 Current Population Survey (CPS), the 1990 Census, and the SBA data. The estimates are similar. The largest differences are that the returns to education are much higher in the SBA data and the gender gap is much smaller in the SBA data.[7] Because workers have no tenure with the firm in the SBA data, the higher return to education may reflect the fact that employers use formal education as a signal for the productivity of newly hired workers more than for a sample of workers as a whole.

Table 6.2 Impact of Training Proxies on the Starting Wage, 1992 SBA Data

Independent Variables	
Constant	-0.255
	(1.60)
Worker's age / 10	0.342
	(3.82)
Age squared / 1000	-0.426
	(3.31)
Worker's relevant experience / 10	0.476
	(7.56)
Experience squared / 1000	-0.933
	(3.72)
Years of education	0.100
	(15.75)
Worker is black	-0.235
	(4.96)
Worker is nonblack female	-0.171
	(6.22)
Worker is union member	0.072
	(1.56)
Logarithm of establishment size	0.040
	(5.36)
Hours worked / 10	0.082
	(1.88)
Hours squared / 1000	-0.982
	(1.65)
R^2	0.513
N	796

NOTE: Absolute value of *t*-statistics given in parentheses. The dependent variable is the logarithm of the starting wage. Ordinary Least Squares estimates are reported.

The smaller gender wage gap is consistent with the recent findings of Wood, Corcoran, and Courant (1993). In their study of Michigan Law School graduates, they found that the gender gap was an increasing function of the time since graduation. Perhaps the largest difference, however, is that the SBA sample's equation has an R^2 over 0.51, while the R^2 for the CPS is 0.36. This is not surprising, however, since wages become more dispersed as workers gain experience, and the SBA sample is limited to newly hired workers.

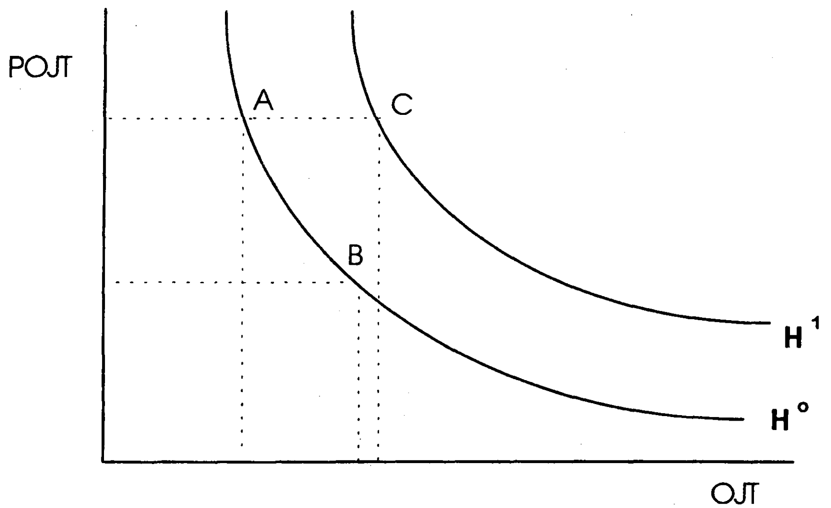
Our initial approach to estimating the impact of training on the starting wage is to estimate equation (6.3). This requires that we not only use measures of on-the-job training but also of the total human capital of the job. After some initial experimentation with the specification using the SBA data, we decided to use two measures of training. The first measure is the sum of the intensity measures for on-site formal training, informal management, informal co-worker, and "watching others," while the second is the intensity measure of off-site formal training. We justify this separation purely on statistical grounds for we know of no theoretical reason why we should treat off-site formal training differently from other forms of training, although this has the advantage of insuring that EOPP and SBA training measures are similarly defined.

For the correct estimation of equation (6.3), however, it is necessary to exclude measures of previously acquired on-the-job training. To see why, consider a production function for human capital. The inputs to the production function are on-the-job training acquired before employment at the firm (denoted POJT) and on-the-job training that the firm offers (denoted OJT), or

$$(6.5) \quad H = f(\text{POJT}, \text{OJT})$$

where $f(\cdot)$ is a production function with the standard properties. In figure 6.1, we depict two isoquants for production of total human capital, with the lower level of total human capital depicted by the curve denoted H^0 and the higher level of total human capital denoted H^1. These two isoquants correspond to two jobs with differing total human capital requirements. Standard human capital models would predict that a movement from point A to point B should reduce the worker's starting wage. If we hold fixed the quantity of previously acquired on-the-job training and increase the quantity of current on-the-job training, we then have a movement from point A to point C. If better workers are matched to positions that require more training, however, we would expect the worker at point C to be more able than the worker at point A. If we could perfectly control for worker ability, we would expect to find a negative relationship between training and the starting wage, but it is doubtful that we can achieve such a control.[8] Indeed, we include the measure of H as a control for the unmeasured ability component.

Figure 6.1 Human Capital Production Function



If we include measure of POJT, OJT, and H into an equation simultaneously, however, we are identifying only random fluctuations in training. To see why, consider the following parameterization of the human capital production function, equation (6.5),

$$(6.6)\quad \ln H + \gamma_0 + \gamma_1 \text{ POJT} + \gamma_2 \text{ OJT} + u$$

where the γs are parameters and u is an error term. The inclusion of any two of the variables POJT, OJT, and H on the right-hand side of equation (6.3) would allow, in principle, the estimation of the trade-off between training and wages. Including POJT and OJT, one can identify how, holding fixed the amount of previously acquired human capital, increases in training affect the starting wage; this is the approach of Barron, Black, and Loewenstein (1989). If markets match more able workers to jobs that require the accumulation of more human capital, however, workers at point C will be more able in observed ways than workers at point B, which will cause the coefficient on training to be biased upward.

To control for this ability bias, we propose using our measure of total human capital, the length of time it takes an untrained worker to become fully trained and qualified, as a proxy for unmeasured ability. As equation (6.6) indicates, however, the inclusion of POJT, OJT, and H into a wage equation creates some difficulty in interpretation. Holding constant POJT and H, movements in OJT require movements in u, unobserved factors that affect human capital accumulation. Thus, a regression so specified would not be informative. Our approach is to consider movements along the human capital isoquant such as movements from point A to point B in figure 6.1. One can think of there being many different workers located on the locus H^0. In a labor market equilibrium, firms should be indifferent, *ceteris paribus*, between hiring workers with a lot of experience who command high starting wages and workers with limited experience who have lower starting wages.

To consider movements from point A to point B, however, it is necessary that we allow for changes in prior on-the-job training to accompany changes in the current employer's on-the-job training. Labor economists use age (or potential experience, which is age minus the number of years of schooling minus six) as a substitute for general human capital, and our relevant experience is also a substitute for labor market experience. Thus, both are used as measures of prior on-the-job training. We exclude age and relevant experience from our estimation, but include our measure of H, which allows us to interpret changes in current on-the-job training as movements along the human capital isoquant.

In columns (1) through (3) of table 6.3, we report the wage equation estimates for the SBA data set using three specifications. (To avoid taking the logarithm of zero, we take the logarithm of one plus the training

Table 6.3 The Impact of Training on the Starting Wage, 1992 SBA and 1982 EOPP Data

Independent variables	SBA data (1)[a]	SBA data (2)[a]	SBA data (3)[a]	EOPP data (4)[b]	EOPP data (5)[b]	EOPP data (6)[b]
Logarithm of job's human capital requirement	0.108	0.091	0.077	0.053	0.050	0.041
	(8.59)	(7.53)	(6.91)	(7.18)	(7.03)	(5.96)
Logarithm of hours of training in first three months	-0.046	-0.035	-0.022	-0.017	-0.011	-0.004
	(4.29)	(3.39)	(2.22)	(2.91)	(1.90)	(0.81)
Logarithm of off-site training in first three months	0.024	0.032	0.033			
	(1.91)	(2.64)	(2.82)	---	---	---
Age and square of age added	no	yes	yes	no	yes	yes
Relevant experience and its square added	no	no	yes	no	no	yes
R^2	0.442	0.500	0.545	0.343	0.3924	0.425
n	796	796	796	1636	1636	1636

NOTE: The absolute values of *t*-statistics are in parentheses. Dependent variables are logarithms of starting wage. Ordinary Least Squares estimates are reported.

a. Other control variables are the number of years of schooling, the logarithm of the number of employees in the establishment, hours worked, the square of hours worked, and dummy variables indicating whether or not the worker is black, a nonblack female, and a union member.

b. Other control variables include years of education, a dummy variable equal to one if the worker is a female, fraction of the establishment that is unionized, the natural logarithm of the number of employees at the establishment, hours worked, hours squared, and a dummy variable indicating whether or not the worker is a temporary or seasonal employee.

measures and the required human capital measure.) As mentioned earlier, we use two measures of training. The first measure is the sum of the measures for on-site formal training, informal management training, informal co-worker training, and "watching others"; the second is the measure of off-site formal training.[9] In column (1) we present our preferred estimates, in column 2 we add age and age squared to the equation, and in column 3 we add relevant experience and its square to the equation. In column (1), the coefficient for the on-site training is negative (-0.046) and highly significant. A 10 percent increase in training decreases the starting wage by about 0.5 percent. When we add age and age squared to the equation, the coefficient on training increases from -0.046 to -0.035, and when we add relevant experience and its square, it increases to -0.022, although it remains statistically significant at the 5-percent level. While the sign of the coefficient for on-site training is negative and consistent with the theory, the magnitude of the coefficient is small.

The coefficient for off-site formal training, however, is positive (0.024) and significant at the 10-percent confidence level. When we add age and its square, the coefficient increases to 0.032 and becomes significant at the five percent level, and remains significant when we add experience and its square to the equation. We tried numerous specification checks to see if we could eliminate the positive sign for off-site training, but none were successful.[10]

In columns (4) through (6) of table 6.3, we report the estimates for the EOPP data. In column (4)—our preferred specification—the coefficient on the logarithm of training is -0.017 with a *t*-statistic of -2.91.[11] Again, while highly significant, the magnitude of the coefficient is quite small. A 10 percent increase in training decreases the starting wage only 0.2 percent. If we add age and its square to the equation, the coefficient increases to -0.011, and if we add relevant experience and its square to the equation, the coefficient increases to -0.004 and ceases to be statistically significant even at the 10-percent confidence level.

For our preferred specification—columns (1) and (4)—the empirical tests provide mixed support for the human capital model. For both data sets, the coefficients for on-site training are negative and significant, confirming the predictions of the theory. Yet, the magnitudes of the coefficients are small, with estimated elasticities of less than -0.05. Indeed, for the SBA data, a 10 percent increase in training—an increase of about 17 hours when evaluated at the mean—results in a reduction in the wage of $0.04. In addition, for the SBA data, the coefficient on off-site training is positive and significant, directly contradicting the theory.

Some alternative measures of training provide somewhat larger estimated elasticities. Mincer (1974), in his derivation of the earnings equations, emphasizes that the appropriate measures of training should be "the fraction of time (or 'time equivalent' if the investment costs include direct outlays as well as time costs) the worker devotes to improving his earning power" (p.19). This suggests that the appropriate measure of training is the intensity of training rather than the total number of hours of training received. For the SBA data, we know both the number of weeks of training and the hours per week of training that the worker receives, so we may construct a measure of the intensity of training. This allows us to construct an on-site training intensity measure as the total number of hours per week of on-site training and an off-site training measure as the number of hours of off-site training. Unfortunately, we cannot replicate the measure for the EOPP data set because it does not contain a measure of weekly hours of training. For the SBA data, the coefficient for on-site training intensity is -0.056 with a t-statistic of -4.05. For off-site training intensity, however, the coefficient is 0.039 with a t-statistic of 2.33.[12]

Contrasting On-the-Job Training Impact on Wages Versus Productivity

While the results above support the contention that on-the-job training and the starting wage are negatively correlated, the magnitude of the starting wage adjustment is small. This may be due in part, however, to our inability to control fully for worker heterogeneity. In this section, we present a second test of human capital theory that involves a comparison of the effects of training on productivity and wage growth, a test that under certain assumptions controls for worker heterogeneity. To see why, consider the following simple representation of the on-the-job training model developed in chapter 2.

Assume that workers hired into the same position have similar unobserved abilities. Recall from chapter 2 that on-the-job training theory predicts that, if training is general and competition exists across employers, then the starting wage and the wage paid to a fully trained worker will adjust such that for each position, the worker bears all the costs and reaps the entire return to such training. That is, the beginning wage, w_b, and the wage after training, w_a, are such that:

$$(6.7)\quad w_a = f_b \equiv p(E, \alpha, 0, 0) - c(E, \alpha, T_g, 0)$$

$$(6.8)\quad w_b = f_a \equiv p(E, \alpha, T_g, 0)$$

where f_b is the productivity net of training costs of a worker with initial human capital E and ability α who receives general training T_g and zero specific training, while f_a is the productivity of this worker after training.

Equation (6.7) highlights the key claim of human capital theory that an increase in training T will lower the starting wage given $dc/dT > 0$. If positions with increased training are filled with more able individuals (i.e., $d\alpha/dT > 0$), and we cannot fully control for this matching of more able workers to positions with greater training, then our estimate of the negative effect of training on the starting wage will be biased upward.

Equations (6.7) and (6.8) suggest, however, an alternative test for the prediction that workers will bear the entire costs of general training. Specifically, dividing (6.8) by (6.7) and differentiating this expression with respect to a change in training, we obtain the following expression:

$$(6.9)\quad d\,(f_a / f_b) / dT = d\,(w_a / w_b) / dT.$$

Equation (6.9) implies that the effect of general training on an index measuring productivity growth, f_a/f_b, and an index measuring wage growth, w_a/w_b, should be identical. That is, it is predicted that $d\,(f_a/f_b) / dT = d(w_a/w_b) / dT > 0$. Thus, we can restate the prediction as follows: an increase in training should result in identical increases in the growth rates of productivity and wages.

Unfortunately, the SBA and EOPP data sets do not have measures of the worker's posttraining wage. Both data sets do, however, contain measures of the wage paid to the typical worker in the same job two years after beginning employment. While there may be some jobs in which the worker is not fully trained at the end of two years, we think the wage after that time period is a good proxy for the posttraining wage. By taking the log difference of the wage paid to a typical worker in the same job after two years and the worker's starting wage, we construct an index of wage growth.

Table 6.4 reports the regression for the wage index. Ideally for this regression, we would like to have all the training that the worker receives in the first two years of employment, but both the EOPP and the SBA data contain only the training that the worker receives in the first three months. In column (1) we report the estimates for the SBA data. A 10 percent increase in the quantity of on-site training increases the wage growth only 0.2 percent. A 10 percent increase in off-site training decreases the wage growth 0.05 percent, although the coefficient is not significant. In column (3) of table 6.4, we report estimates using the EOPP data. A 10 percent increase in training increases wage growth by 0.3 percent as well. Thus, both of these coefficients suggest that increases in the quantity of training increase wage growth, and both give quantitatively similar estimates of the magnitude.

The magnitude of the trade-off, however, is not large. One would expect that a 10 percent increase in training would reduce the worker's initial productivity by much more than 0.2 percent. Fortunately, both the EOPP and SBA data contain measures of the worker's productivity so that we may compare the change in wages to the change in productivity. In the EOPP data, respondents were asked the following question:

> Please rate your employee on a productivity scale of zero to 100, where 100 equals the maximum productivity rating any of your employees [in this] position can attain and zero is absolutely no productivity by your employee. What is the productivity of [the last worker hired] during (his/her) first two weeks of employment?

We asked a slightly different version of this question to the SBA respondents. They were asked:

Table 6.4 Wage and Productivity Growth Index, 1992 SBA and 1982 EOPP Data

Independent Variables	Wage index SBA[a] (1)	Productivity index SBA[b] (2)	Wage index EOPP[a] (3)	Productivity index EOPP[b] (4)
Logarithm of training	0.020	0.283	0.028	0.230
	(4.20)	(10.01)	(8.98)	(14.49)
Logarithm of off-site training	-0.005	0.045		
	(0.76)	(1.23)	---	---
Chi-squared statistic	---	102.82	---	200.02
R^2	0.020	---	0.046	---
N	860	860	1683	1683

NOTE: Absolute value of t-statistics is given in the parentheses.

a. The dependent variable is the difference between the logarithm of the wage paid to the typical worker after two years and the logarithm of the starting wage of the last worker hired. Ordinary Least Squares estimates are reported.

b. The dependent variable is the logarithm of one plus the index of the productivity of the fully trained worker relative to the productivity of the last worker hired. Tobit estimates are reported.

> Please rate (name of last worker hired) on a productivity scale of zero to 100, where 100 equals (name's) productivity when (he/she) is fully trained and zero is absolutely no productivity by (name). . . . What was (name's) productivity on this scale during his/her first two weeks of employment?

Because the reciprocals of the productivity indices are estimates of productivity growth, we may see if the impact of training on worker productivity growth is similar to the impact of training on wage growth.

In column (2) and (4) of table 6.4, we report estimates for the productivity growth in the SBA and EOPP data, respectively. Because the productivity index is bounded by zero and 100, we use a tobit to estimate the coefficients. A 10 percent increase in training in the first three months of employment decreases initial productivity by 2.3 percent in the SBA data and 1.9 percent in the EOPP data.[13] A comparison of the impact of training on starting wage indices and on the productivity indices reveals that training lowers the initial productivity of workers 11 times more for the SBA and eight times more for the EOPP data than do the starting wage indices. This suggests that workers pay for only a small portion of their training.

Indeed, in our view, it is too small a portion to be consistent with the prevailing theories of on-the-job training. While the elasticities of wage growth with respect to training are 0.02 for both data sets, the elasticities of productivity growth with respect to training are about 0.23 and 0.19 for the SBA and EOPP data. In addition, the use of a productivity measure to calculate the cost of training understates the true cost of that training. Such a variable fails to account for the reduction in productivity of managers and co-workers who spend time away from their duties to train newly hired workers.

An obvious objection to this comparison questions the validity of the productivity measures. One could argue that employers are not very good about rating employees on a productivity scale. It could be that employers exaggerate the swings in productivity that new employees experience when using the productivity index. While this may be true, the underlying index values do not appear unreasonable. In table 6.5, we compare the distributions of the productivity index and a wage index for the SBA and EOPP data. The mean value of the productivity index is 51.39 percent for the SBA data and 51.01 percent for the

EOPP data. The 25th percentile is 30 percent for both data sets, the median is 50 percent for both data sets, and the 75th percentile is 75 percent for both data sets.[14] In our view, these values do not seem an implausible description of the productivity of newly hired workers in the first two weeks of their employment compared to the productivity of workers who are fully trained and qualified. In contrast, the fluctuation of wages is much smaller. For the sake of comparison, we define the wage index to be the ratio of the starting wage to the wage paid to a worker after two years, and multiply by 100 to scale the index.[15] The mean of this index is 86.88 percent for the SBA data and 87.85 percent for the EOPP data. The 25 percentiles are 80.91 percent for the SBA and 78.27 percent for the EOPP data, the medians are 88.50 percent for the SBA and 88.78 percent for the EOPP data, and the 75 percentiles are 94.13 percent for the SBA and 98.09 percent for the EOPP data. Thus, the underlying distribution of the wage index is much more compressed than that of the productivity index.

Table 6.5 A Comparison of Productivity and Wage Index, 1992 SBA Data and 1982 EOPP Data

	SBA data productivity index	SBA data wage index	EOPP data productivity index	EOPP data wage index
25th percentile	30	106	30	102
50th percentile	50	113	50	113
75th percentile	75	124	75	128
Mean	51.39	86.88	51.01	87.85
Standard deviation	---	---	---	---
N	860	860	1683	1683

Further Evidence on Wage and Productivity Growth

The SBA data allow another test to determine if productivity growth greatly exceeds wage growth. This test is based completely on the experiences of the newly hired worker instead of relying on the experiences of the typical worker. For the SBA data, we asked about the worker's productivity after three months of employment as well as the worker's productivity relative to a fully trained worker. In table 6.6, we report the regression estimates for wage and productivity growth. For independent variables, we include the two measures of training: total off-site training in the first three months and total hours of other forms of training.

In columns (1) and (2) of table 6.6, we report the estimates for the wage and productivity growth equation. Interestingly, training is positively correlated with wage growth with an elasticity of 0.011, but off-site training is negatively associated with wage growth. Training is also positively associated with productivity growth with an elasticity of 0.190. Off-site training has no significant relationship with productivity growth in the first three months of employment. Thus, again, the growth in productivity greatly exceeds the growth in wages.

Table 6.6 Wage and Productivity Growth in the First 3 Months of Employment, 1992 SBA Data

Independent variables	Wage growth SBA[a] (1)	Productivity growth SBA[b] (2)	Wage growth SBA[a] (3)	Productivity growth SBA[b] (4)
Logarithm of training	0.011	0.190	0.026	0.241
	(3.78)	(9.00)	(3.30)	(6.11)
Logarithm of off-site training	-0.010	-0.005	-0.018	0.002
	(2.58)	(0.16)	(1.59)	(0.38)
R^2	0.019	0.082	0.039	0.105
N	929	929	326	326

NOTE: Absolute value of *t*-statistics is given in the parentheses. Ordinary Least Squares estimates are reported.

a. The dependent variable is the difference between the logarithm of the wage of the last worker hired after three months of employment and the logarithm of the starting wage of the last worker hired.

b. The dependent variable is the difference of logarithm of the productivity index of the last worker hired at the end of three months of employment and the logarithm of the productivity index of the last worker hired in the first two weeks of employment.

An objection to this test is that firms may find it costly to adjust wages continuously because of what the macroeconomics literature refers to as "menu costs." The essential idea of menu costs is that price changes, or in this case wage changes, are costly to the firm. For instance, we might imagine that the decision to give a worker a raise may require the worker's manager to meet with him or her. In addition, the worker's manager may not have the authority to give the worker a raise and may need approval from other individuals within the firm. In addition, in many jobs workers and firms sign formal contracts that may cover a period longer than three months. For instance, each of the authors of this monograph has an annual contract that specifies his wages for a year in advance, and while none of us would object to our employers renegotiating our contract to give us a raise, we do note, sadly, that such renegotiations have not been common.[16]

To guard against a bias created by any costs to changing the wage, in columns (3) and (4) we reestimate the equation on the sample of workers who have received wage increases in that period. While the wage-training elasticity increases to 0.026, the productivity-training elasticity increases to 0.241 and remains over nine times as great at the wage-training elasticity. Thus, again, productivity growth is much larger than wage growth.[17] Moreover, while menu costs may account for some of the lack of responsiveness of wages to productivity changes, they do not explain the whole story. Indeed, given that firms have incurred any menu costs associated with an increase in wages, we would expect this group to be free of this form of bias. If those workers who receive general training are more likely to receive a raise in the first three months of employment, this group may well yield a coefficient on training that is biased upward.

The Effect of Measurement Error on the Estimated Effect of Training on Wages and Productivity?

If there is measurement error in training, the effects of training on wages and productivity are biased toward zero. One way to correct for the effects of measurement error is through the use of instrumental variables (Greene 1993). A variable must be found that is correlated with the variable that is measured with error but not correlated with the measurement error. If we assume that firm estimates of training fulfill this condition for worker-reported training and worker estimates of training do the same for firm-reported training, then we can apply instrumental variables to correct for the effects of measurement error.[18] This is similar to the approach of Ashenfelter and Krueger (1994), who use one twin's estimate of the other twin's schooling as an instrument for estimating the return to schooling for the other twin.

The first step in the estimation of the instrumental variables model involves estimating the variable measured with error as a function of the instrument or instruments. In this case, we use only a single instrument—the other party's reported total hours of training. We use the training instrument for starting wage equations, wage index, and productivity regressions similar to those estimated earlier in this chapter. The wage growth regressions explain the growth in starting wage to the wage of a typical worker in the same position after two years. The productivity growth is the inverse of a productivity index of the new hire measured relative to the fully trained worker on a scale from 0 to 100. The productivity regressions indicate how much training increases the productivity of the new worker relative to a fully trained worker. The first stage equations for training in the first four weeks of employment are:

$$\log(\text{worker-reported training}) = 2.09 + .414 \log(\text{firm-reported training})$$
$$R^2 = .156 \quad n = 179$$

$$\log(\text{firm-reported training}) = 2.64 + .376 \log(\text{worker-reported training})$$
$$R^2 = .156 \quad n = 179$$

Table 6.7 shows ordinary least squares and instrumental variable estimates for starting wage, wage growth, and productivity growth regressions using the Upjohn Institute, SBA, and EOPP data sets. Using the Upjohn Institute data, the instrumental variable training estimates are greater in absolute value than the ordinary least squares estimates in four out of six cases. Correcting for measurement error does not appear to have a dramatic effect on the estimated training effects in the Upjohn Institute data. The estimated training effect in the firm-reported wage growth regression, however, is significant after correcting for measurement error.

Table 6.7 Estimates of the Impact of Training on Starting Wages, and Wage and Productivity Growth

	Ordinary least squares			Instrumental variables[f]		
Upjohn Institute data	**Parameter**	**t**	**n**	**Parameter**	**t**	**n**
log (worker-reported starting wage)[a]	0.0031	0.143	180	0.0476	0.760	146
log (worker-reported wage growth)[b]	0.0113	1.11	171	0.0074	0.223	138
log (worker-reported productivity growth)[c]	0.1490	4.38	222	0.2259	2.48	181
log (firm-reported starting wage)[a]	0.0173	0.634	158	-0.0134	0.206	172
log (firm-reported wage growth)[b]	0.0100	0.87	150	0.1286	3.02	159
log (firm-reported productivity growth)[c]	0.1557	4.98	197	0.1854	2.18	207
SBA data						
log (starting wage)[a]	-0.046	4.26	796	-0.122	4.26	796
log(wage growth)[b]	0.020	4.20	860	0.053	4.20	860
log(productivity growth)[c]	0.283	10.01	860	0.753	10.01	860
EOPP data						
log (starting wage)[d]	-0.019	2.96	1,386	-0.051	2.96	1,386
log(wage growth)[b]	0.027	8.98	1,683	0.072	8.98	1,683
log(productivity growth)[e]	0.230	14.47	1,683	0.612	14.47	1,683

a. The other independent variables used with the Upjohn Institute and SBA starting wage regressions are years of schooling; dummies for black male, black female, other male, other female, white female, and collective bargaining coverage; log of establishment size, hours of work, hours of work squared; and log of job complexity. Training is measured by the log of total hours of training plus one in the first four weeks in the Upjohn Institute regressions, and the log of total hours in the first three months plus one in the SBA regressions. The SBA regressions include off-site training as a separate variable.

b. Wage growth is growth in the log of the starting wage calculated using the wage of the typical worker in the same position after two years

(continued)

c. Productivity growth is the inverse of the log of the productivity in the first two weeks of employment relative to the productivity of a fully trained worker in the same position on a scale of 0 to 100.
d. The other independent variables used in the EOPP starting wage regressions are years of schooling, a female dummy variable, percent of the firm's labor force covered by collective bargaining, log of establishment size, hours worked, hours worked squared, and log of job complexity., Training is measured by log of total hours of training plus one in the first three months (excluding off-site formal training).
e. Productivity growth in the EOPP data in the inverse of the log of productivity in the first two weeks of employment relative to maximum possible productivity on a scale of 0 to 100.
f. Firm reported training is the instrument in the worker-reported regressions, and worker-reported training is used as the instrument in the firm-reported regressions. The Upjohn Institute worker-reported training regression estimates are combined with firm-reported training data from the SBA and EOPP data.

The bottom half of table 6.7 uses the estimated regression of worker-reported training on firm-reported training from the Upjohn Institute data and the actual firm-reported training from the SBA and EOPP data to illustrate the effects of measurement error in those data sets.[19] The estimated training effects increase by almost a factor of three in absolute value in each case over the estimates reported in Barron, Berger, and Black (1993a), illustrating the effect of the measurement error on the estimated effects of training on wages and productivity.

Why is the Impact of Training on the Starting Wage so Small?

Gary Becker, in his 1992 lecture accepting the Nobel Prize for Economics (1993), said, "A close relation between theory and empirical testing helps prevent both the theoretical analysis and the empirical research from becoming sterile. Empirically oriented theories encourage the development of new sources and types of data, the way human capital theory stimulated the use of survey data, especially panels. At the same time, puzzling empirical results force changes in theory" (p. 403). For both the SBA and EOPP data, we find a small impact of training on the starting wage, and an impact of training on productivity growth that is several times bigger than the impact of training on wage growth.

Are these findings consistent with theory? To be consistent, virtually all of the training must be specific and firms must bear an overwhelming share of the cost of training, or the returns to training must be deferred past our two year horizon by long term contracts. Fortunately, the EOPP survey asked directly about how specific their training was. The survey asked:

> "How many of the skills learned by employees in this job are useful outside of this company?
>
> 1. Almost all
> 2. Most
> 3. Some
> 4. Or almost none

We report the responses in table 6.8. Nearly 60 percent of the sample report that the training is almost all general human capital. Only about 8 percent report that almost none of the skills are of value outside the company. Thus it appears that employees pay only a fraction of the training costs and employers feel much of that training is general training. We think that our findings represent one of the puzzling empirical results of which Becker speaks that need further explanation.

Currently, there are four interesting and somewhat interrelated explanations for such a finding. Parsons (1989) proposes that workers may be unable to fund their training costs because of financing constraints. We can find some evidence for this hypothesis in the SBA data. We broke up our sample into four groups: those without a high school degree (n = 65), those whose highest grade completed was 12 (n = 313), those who attended college but did not complete four years (n = 212), and those who completed at least four years of college (n = 206). If financial constraints are important, we would expect that the coefficient on the training measure should be monotonically increasing in education because more highly educated workers earn more money and, hence, should be better able to finance their training. The SBA data support this hypothesis. For high school dropouts, the coefficient on training is 0.038 with a *t*-statistic of 1.2; for high school graduates, the coefficient on training is -0.023 with a *t*-statistic of -1.6; for those with some college, the coefficient on training is -0.058 with a *t*-statistic of -2.5; and for college graduates, the coefficient on training is -0.107 with a *t*-statistic of -4.2.

Table 6.8 The Degree to Which Skills are General, 1982 EOPP Data

How many skills are general	Fraction
almost all	59.59%
mostly	13.11
some	19.25
almost none	8.05
N = 2707	100.0

Unfortunately, other data are not so kind to this hypothesis. For instance, using the same four groups, we see no clear pattern in the EOPP data. For high school dropouts, the coefficient on the stock of training is -0.03 with a t-statistic of -2.0 (n = 192); for high school graduates, the coefficient on the stock of training is -0.021 with a t-statistic of -2.9 (n = 960); for those with some college, the coefficient on the stock of training is -0.013 with a t-statistic of -2.1 (n = 331); for college graduates, the coefficient on the stock of training is -0.002 with a t-statistic of -0.1 (n = 153). Lynch (1992), using data from the NLSY, divides her sample into three groups: high school dropouts, high school graduates, and those who attended but did not graduate college. She finds that the coefficient on current training spells is monotonically increasing in education, which directly contradicts the hypothesis. Indeed, of her three groups, only the coefficient for high school dropouts is negative.

A second potential reason for the small impact of training on the starting wage focuses on the informational problem that workers and firms face. Firms have specialized in the production of their product, and presumably, they have learned a great deal about the market in which they operate. As such, these firms may know that the training they offer is general training, but convincing workers of that fact may be very difficult. Clearly, learning to use a word-processing program on a computer is general training, although many forms of training are less obvious. For instance, learning to operate a medium precision-measuring machine *may* facilitate learning about other pieces of electronic equipment. In addition, workers may find it difficult to signal their training to alternative employers. In other words, workers may believe that they are being provided with general training, but they are not convinced that they will be able to signal alternative employers of their skills.

Moreover, the extent of the market and the willingness of workers to relocate also affect the generality of training. For instance, in Scott County, Kentucky, the Toyota Motor Company has a large manufacturing plant. Suppose that all the training that Toyota offers to their workers is general training in the sense that it makes the workforce more productive at other automobile plants. Unfortunately for Toyota's workers, the present worldwide excess capacity in automobile production currently lessens the value of such training in the automobile marketplace. Should there be an increase in demand for experienced automobile workers, however, the training would be of use only to those workers who were willing to leave Scott County because there is only one automobile plant in Scott County. Thus, by forcing workers to bear the cost of training, Toyota may dissuade from applying those workers who wish to stay in Scott County for the rest of their lives. On the other hand, these may be the very workers Toyota wishes to attract.

A third possible explanation for the weak relationship between training and the starting wages focuses on the nature of labor contracts. This small literature includes Barron, Black, and Loewenstein (1993), Black and Loewenstein (1990), and Kuhn (1993).[20] Each of these papers focuses on a prevalent feature of labor contracts: they seldom specify wages for long periods into the future. Apparently, workers and firms find it efficient not to specify the wage too far into the future because of the underlying uncertainty inherent in the economy. Rather, firms and workers rely on implicit agreements, or what Okun referred to as the "invisible handshake." Such contracts allow firms and workers to respond to the changing economic climate. The flexibility associated with such contracts, however, is not without costs. Because the agreement is implicit, it is not enforceable by third parties, and contracting parties may renege on these implicit agreements.[21]

One solution to this "hold-up" problem is for contracts to be self-enforcing; that is, neither party would have any incentive to break the underlying contract once it is signed. For instance, in Barron, Black, and Loewenstein (1993) and Black and Loewenstein (1990), firms are given the right to make "take-it-or-leave-it" offers. In return for giving the firm this monopsony power, workers receive a large starting wage. In Kuhn's paper, workers and firms play a Nash-Rubinstein bargaining game to divide the surplus. In Barron, Black, and Loewenstein (1993) and Kuhn (1993), workers who receive more specific training may receive a higher starting wage. In these models, the reason for this premium is that these workers have lower turnover probabilities and, hence, are more valuable to the firm. Rather than offering the workers higher initial wages, firms would ideally prefer to "backload" by offering to pay the workers high wages later in their career, but workers would not find such a promise credible.

Our fourth proposed explanation focuses on a broad class of models that economists call "efficiency wage" models. While this class of models is quite broad, the thread that relates these models is the notion that employers, for one reason or another, find it optimal to pay wages in excess of the market clearing wage (see Weiss 1990 for an excellent introduction to the various forms of efficiency wage models). To simplify, we divide these models into five broad classes: effort-inducing, adverse selection, turnover-reducing, nutritional-based, and gift exchange.

In the effort-inducing efficiency wage models, with the classic reference being Shapiro and Stiglitz (1984), firms pay higher than market clearing wages to induce employees to work hard. If workers should choose to shirk work and risk being fired, they also risk losing the quasi-rents associated with the employment at high-wage firms. In the adverse selection version of efficiency wage models (e.g., Weiss 1980), firms offer higher wages to attract the most talented workers. If workers know their abilities while firms imperfectly observe the abilities of workers and lower the wages paid, high-ability workers will no longer apply, but relatively low-ability workers will. Hence, reducing wages may lower worker quality. The turnover-reducing efficiency wage models (e.g., Stiglitz 1975) emphasize that firms with differing turnover costs may offer similar workers differing wages because high-turnover-cost firms will want to reduce turnover by offering relatively high wages. The nutritional-based efficiency model deals with the nutritional needs of workers and is better suited for the developing world than the U.S. economy. The last form of efficiency wages—the gift exchange—takes a more sociological or psychological approach. Akerlof (1982) provides the classic paper in which he argues that employees will work harder when firms provide them a "gift" of higher wages.

Efficiency wage models remain controversial; the exchange between Carmichael (1990) and Lang and Kahn (1990) offers an interesting discussion of the controversies. The appeal of efficiency wage models is that they provide a cogent explanation of why labor markets may not clear in the sense that the quantity of labor demanded at a given wage is not equal to the quantity of labor supplied. In our context, if labor markets do not clear, there may be no requirement that training lower the starting wage. Unfortunately, as Carmichael (1990) emphasizes, efficiency wage models have not generated a rich set of predictions that economists have subjected to rigorous empirical tests. Indeed, many of the current forms of the efficiency wage models appear inconsistent with the observed patterns of wage profiles. For instance, the shirking explanation of efficiency wages fails to explain the covariance of wage premiums across firms. While workers in positions that are difficult to monitor should receive wage premiums, workers in jobs that are easily monitored should not receive the premiums. The existing empirical work (e.g., Krueger and Summers 1988), however, suggests that wage premiums are highly correlated across occupations within industries, which suggests that monitoring costs are not driving these wage premiums.

NOTES

1. Ransom (1993) is the exception. In his study of academic salaries, he finds that there is a negative relationship between a professor's time at a university and the professor's salary.

2. See Garen (1988) for a detailed review of the matching literature. The term "statistical artifact" is from Mortensen's (1984) working paper.

3. Altonji and Spletzer (1991); Barron, Black, and Loewenstein (1989, 1993); Booth (1993); Brown (1989); Duncan and Hoffman (1978); Lillard and Tan (1992); Loewenstein and Spletzer (1993); Lynch (1992); Mincer (1988); Parsons (1989); and Pergamit and Shack-Marquez (1987) all find that training is associated with wage growth. To our knowledge, only Veum (1993) has failed to find a positive correlation between training and wage growth.

4. In particular, Lynch finds that for high school dropouts, uncompleted spells of training are negatively correlated with wages, with a significant level of 11.6 percent two-tailed. For those with a high school degree, there is a positive correlation between uncompleted spells of training and wages, with a significance level of about 5.5 percent. For those with some college but no college degree, there is also a positive correlation between uncompleted spells of training and wages, which is significant at a 3.2-percent level.

5. Unfortunately, the EOPP data do not contain the same questions, so we cannot replicate these findings for that set.

6. We initially used separate gender-race controls for black, white, and other racial groups, but we could not reject the hypothesis that we could use the more parsimonious specification. See Barron, Berger, and Black (1993a) for results that use the more general specification. We also initially used the logarithm of the number of employees at other sites, a variable we used in our specification of the training equations, and we could not reject the hypothesis that its coefficient was zero.

7. Our analysis of the matched employer-employee Upjohn Institute data suggested that there may be substantial measurement error in the amount of education or workers. The SBA uses employer reports of worker education. If there is more measurement error in employer reports than in worker reports, the true gap between the SBA and the Census and CPS estimates is even wider.

8. We use ability here in a very general sense. For instance, workers with a lower turnover propensity would be considered "more able."

9. We tested to see if we could combine the four measures of training; the F-statistic was 1.11 with a p-value of 0.35. In contrast, if we try to aggregate all five measures of training, the F-statistics is 7.01 with a p-value of 0.001.

10. We used specifications that included one-digit industry and occupation controls, a dummy variable indicating multiple sites, the logarithm of the number of sites, and the logarithm of the number of employees at other sites. In addition, we estimated the various permutations that arise

from these three controls. Finally, we estimated the equation using the three other site controls and the one-digit industry and occupation controls. In all specifications, the coefficient was positive.

11. For the same of comparison with the EOPP data, we also estimated an equation using the SBA data without the measure of off-site training. The coefficient for on-site training was -0.044 with a *t*-statistic of -4.06.

12. A potential weakness of this measure of on-site training intensity is that it assumes all types of training are acquired simultaneously. For instance, it is possible that the workers may have types of training that begin at different times. Perhaps for the first two weeks, workers are sent to a formal training program, but for weeks three and four, they receive informal training from their managers and co-workers. To guard against this potential bias, we define an alternative measure of training intensity as the maximum hours of any single component of training, which precludes any double counting but may understate the intensity of training. Using this alternative measure of training, the coefficient on the logarithm of training is -0.063 with a *t*-statistic of -4.03, while the other coefficients remain virtually unchanged. Thus, a 10 percent increase in training decreases starting wages by 0.63 percent. As another test of the robustness of the result, we define a third measure of training intensity to be the sum of the hours of all four types of training if that sum is less than or equal to the number of hours of work and equal to the number of hours of work otherwise, which may overstate the intensity of training. When we use this measure of training, the coefficient on training is -0.079 with a *t*-statistic of -4.49, while again the remaining coefficients hardly change.Using this estimate, a 10 percent increase in training lowers the starting wage by about 0.79 percent. Using these last two measures of on-site training intensity left the coefficient for off-site training virtually unchanged—positive and significant at the 5-percent confidence level.

13. One cannot interpret the coefficients from a tobit equation as derivatives. Evaluated at the means of the two samples, the derivatives are 0.8074 x β for the SBA data and 0.8238 x β for the EOPP data.

14. Both productivity distributions are centered at the 50th percentile and are the 75th percentile of both distributions are is at an index value of 75. These scores occurred without norming the productivity indices. That the medians occur exactly at 50 is probably due to the fact that employers are likely to pick round numbers when reporting the indices.

15. Because the literature has traditionally used the log difference in wages as an approximation for wage growth, we used the log difference in the starting wage and the wage after two years as an approximation of the wage index in our regression analysis. None of the results change much, however, if we use this real wage index.

16. Of course, many firms may have a probationary period and wage built into their wage payment structure over a fixed period of time, say three months, so wages may remain constant for other reasons than menu costs. Thus for either menu cost of account period reasons, firms may not adjust wages at the same time and rate they observe productivity growth.

17. We also estimated the wage growth equation using a tobit procedure, which resulted in a smaller coefficient on training than did restricting the sample to those with wage changes. We also ran a probit equation with the dependent variable equal to one if there was a wage change and zero otherwise. For independent variables we used the worker's experience, logarithm of training, logarithm of off-site training, and years of education. In addition, we used the number of hours worker per week, the logarithm of the number of employees at the establishment, and a dummy variable indicating if the job was covered by a union contract. The worker's education is negatively related to receiving a wage change in the first three months, the worker's experience is negatively related to receiving a wage change, and each of these coefficients is significant at the 5-percent level. In addition, the coefficient on establishment size is negative with a *t*-statistic of -1.66. The

coefficient on the logarithm of training in the first three months is positive (0.088) and statistically significant (t-statistic of 2.38). In contrast, the coefficient on the logarithm of off-site training is negative (-0.080) but has a t-statistic of only -1.70.

18. We are not assuming that the firm's or worker's reported training are measured without error. Rather, we are assuming that one party's report can be used as a valid instrument for the other party's report.

19. The SBA and EOPP data sets only asked firms about the training of the last worker hired and did not interview workers. Therefore, it is not possible to construct instruments directly from the SBA and EOPP data. Rather, one must rely on the estimates obtained from the Upjohn data. The estimated t's and standard errors remain the same because we are simply substituting a linear combination of the old variable back into the regression. Thus, these estimates are intended only to illustrate the effects of measurement error on estimated training effects. In addition, the training measures used in the SBA and EOPP data sets do not include off-site formal training. Therefore, another set of Upjohn instrumental regressions are estimated for use with the SBA and EOPP data that do not include off-site formal training.

20. Parsons (1990) also mentions problems associated with the lack of formal contracts, but he does not offer a formal model.

21. Indeed, it is often difficult to determine what that agreement actually is. For instance, IBM had a company "practice," as opposed to a "policy," of not laying off its employees. Recently, of course, IBM has fallen on hard times and has laid off some employees. Did IBM violate its implicit agreement with its employees? We conjecture that recently laid-off workers would be much more likely to say yes than the current stockholders. (That still doesn't mean IBM violated a contract, since it was only "implicit" and not enforceable by law.)

Appendix to Chapter 6

In this appendix, we compare wage equation estimates from the SBA data with those from two commonly used micro data sets: the 1990 Census and the March 1992 Current Population Survey (CPS). For both the Census and the CPS, we limit our sample to wage and salary workers who are between 16 and 64 years of age and not full-time students. In columns (1) and (2) of table A.6.1, we report wage equation estimates where the dependent variable is the hourly wage calculated from yearly earnings for the previous year (1991 for the CPS and 1989 for the Census), the number of weeks worked, and number of hours worked per week. Both data sets contain a large number of observations, nearly 65,000 for the CPS and over 950,000 for the Census. For the independent variables, we include potential experience (age minus years of education minus six) and its square, years of education, a dummy variable indicating that the worker is black, a dummy variable indicating that the worker is a nonblack female, and hours worked per week and its square. The coefficients in columns (1) and (2) are similar except for the hours profiles, which are relatively imprecisely estimated. For comparison, we include a wage equation using similar specification with the SBA data. Several differences are apparent. First, the coefficient on education is much larger in the SBA data than in the Census or the CPS. Given that the SBA is a sample of newly hired workers, however, it is perhaps not surprising that employers reward newly hired workers more for their education than workers with some experience at the firm. Education is a readily available signal to employers that is observable at the time of hire. After employers have the opportunity to observe workers, wages may depend less on the workers' education and more on their on-the-job performance. In fact, the true magnitude of the difference may be larger than it appears if there are more problems with measurement error in employer-reported education than with worker-reported education.

There is also a dramatic difference in the coefficient on the dummy variable indicating that the worker is a nonblack female. For the SBA data, nonblack females earn about 19 percent less than nonblack males, while the corresponding figure for the CPS is 32 percent and is nearly 38 percent for the Census. We think this difference arises for two reasons. First, to the extent that women have higher turnover rates than men, we would expect women to have shorter tenure than men and hence the gender wage gap should be larger in a cross section of all workers than in a sample of newly hired workers.[1] Second, women are more likely to be temporary or seasonal employees than men, and temporary and part-year employees earn substantially less than permanent employees. Thus, by using a sample of all workers rather than permanent workers, the CPS and Census will yield lower wages for women than the SBA data, which is a sample of permanent workers.

In column (4), we report a wage equation for the outgoing rotation of the March 1992 CPS. The outgoing rotation, which comprises about a quarter of the March 1992 CPS, was asked questions about their February earnings and union status. Thus, only workers who were employed in February are included in this sample. As a result, the sample size is only about 22 percent of the sample in column (1). By focusing on only those employed in February, we eliminate not only the 75 percent of the sample not in the outgoing rotation but also an additional 3 percent of the sample who are not currently employed. In addition to using a current wage variable, this specification also includes as controls current measures of union status and firm size. Restricting our sample to these workers has a dramatic effect on the coefficient on the nonblack female variable. The coefficient now indicates that nonblack women are paid about 22 percent less than nonblack men. If we remove the firm size and union variables, the difference rises to 23 percent. Thus, the change in the sample is responsible for most of the change in the coefficient. Also, the coefficient on education falls, which is again consistent with the notion that education matters less for more experienced workers.

In column (5), we report the same specification of the wage equation for the SBA data. While the inclusion of the firm size and union status variables lowers the coefficient on education somewhat, it is about 50 percent higher in the SBA data than the CPS data. Similarly, for the SBA the coefficient on nonblack females is about 80 percent the magnitude of the coefficient for the CPS data. The other coefficients are reasonably similar, although the hours profile is very imprecise in the SBA data. Taken as a whole, we think this represents a reasonably similar set of estimates, especially given the differences in the samples.

NOTE

1. See Ureta (1992) for a description of the gender difference in tenure in the CPS.

Table A.6.1 **Wage Equation Estimates (Ordinary Least Squares) Using the 1992 March Current Population Survey and the 1990 Census**

Independent variables	CPS (1)	Census (2)	SBA (3)	CPS (4)	SBA (5)
Constant	0.667	0.607	-0.104	0.217	-0.069
	(49.70)	(112.55)	(0.86)	(6.33)	(0.58)
Potential experience / 10	0.399	0.362	0.311	0.282	0.316
	(57.57)	(194.47)	(7.61)	(24.50)	(7.86)
Potential experience squared / 1000	-0.605	-0.499	-0.563	-0.419	-0.571
	(38.82)	(122.01)	(4.58)	(16.05)	(4.71)
Years of education	0.098	0.093	0.133	0.085	0.128
	(113.37)	(406.98)	(20.80)	(59.04)	(19.84)
Worker is black	-0.292	-0.301	-0.257	-0.279	-0.278
	(34.97)	(134.25)	(5.16)	(19.60)	(6.16)
Worker is nonblack female	-0.319	-0.375	-0.186	-0.224	-0.178
	(65.10)	(270.31)	(6.36)	(27.22)	(6.16)
Worker is union member				0.137	0.070
	---	---	---	(13.64)	(1.45)
Logarithm of firm size				0.039	0.035
	---	---	---	(19.88)	(4.34)
Hours worked / 10	0.010	0.125	0.103	0.238	0.057
	(1.93)	(70.67)	(2.29)	(19.07)	(1.26)

(continued)

Table A.6.1 (continued)

Independent variables	CPS (1)	Census (2)	SBA (3)	CPS (4)	SBA (5)
Hours squared / 1000	0.004	-0.189	-0.099	-0.246	-0.046
	(0.31)	(99.97)	(1.61)	(16.41)	(0.75)
R^2	0.261	0.254	0.444	0.360	0.461
N	64,942	950,360	796	14,244	796

NOTE: Absolute values of *t*-statistics are given in the parentheses. The dependent variables are the logarithms of wages. Ordinary Least Squares estimates are reported.

CHAPTER 7

Training and Firm Recruiting Strategies

The job-matching literature stresses that workers are not equally well suited for all positions. Rather, as noted by Topel (1986), a "heterogeneity of talents and technologies generates job specific differences in productivity" (p. 200). Given complementarity between worker ability and training in production, we assumed in previous chapters that higher-ability workers are matched to positions requiring greater on-the-job training. As we have seen, because higher-ability workers command higher wages, the training variable in the starting wage regression is thus biased upward to the extent that we cannot control perfectly for ability differences among workers.

We have yet to explore the way in which high-ability workers are matched to positions with substantial training. A likely mechanism for achieving such matching is the recruiting and screening activity of employers. Employers do engage in considerable search for new workers. For instance, using data from the second wave of the EOPP Survey, Barron and Bishop (1985) report that employers, on average, screen more than nine applicants before extending an offer. Moreover, Barron and Bishop (1985) document that, on average, employers spend a considerable amount of time evaluating applicants, but there is much variation in the time spent. They report that the mean time spent evaluating applicants is about 10 hours, with a standard deviation of nearly 17.2 hours.

The issue we address in this chapter is whether the recruiting activity of employers, or what we refer to as employer search, is the mechanism through which high-ability workers are matched to positions with substantial training. To examine this issue, the first section develops a model of employer search behavior in which employers obtain a noisy signal about the quality of an applicant. Considering this signal, an employer determines whether to extend an applicant an offer or reject the applicant and continue searching. In the model, employers determine both their hiring standards and the accuracy of the signal that they obtain. For a fixed accuracy of a signal, the higher that employers set their hiring standards, or the greater their "reservation signal," the greater the expected ability of the worker. Higher reservation signals, however, increase the expected length of search, or require a more extensive search. For a fixed reservation signal, the greater the resources that employers devote to evaluating each applicant the greater the accuracy of the information they obtain from their search. Thus, firms face a choice of both extensive and intensive search.

The model of employer search allows us to examine how training and other characteristics of the position or applicants will affect employer search. Given that ability and on-the-job training are complements in production, we show that employers will adopt higher ability standards for positions requiring more training, which means that on the average they will screen more job applicants. Employers intent on matching high-ability workers to positions with more training can also raise the expected ability of a new hire by screening each applicant more intensively. By screening more intensively, an employer can raise the precision of the signals he or she obtains concerning applicants' abilities, and thus reduce the likelihood of hiring an unsuitable applicant. Employers filling positions involving more training can therefore be expected to try to increase the ability of the new hire not only by examining more job applicants but also by engaging in more intensive, and more costly, screening of each applicant.

Predictions of the effect of training on employer search are tested using data from four national data sets: the 1980 first-wave EOPP survey, the 1982 second-wave EOPP survey, the 1992 SBA survey, and the 1993 Upjohn Institute survey. We find strong evidence that employers search more, both extensively and intensively, for positions that require substantial training. There is strong circumstantial evidence that higher-ability workers are indeed matched to positions with more training. We then consider the effect of training, as well as other variables, on the duration of a vacancy.

Employer Optimal Search Strategy

Search by an employer to fill a vacancy can be viewed in the following fashion. In each period there is the probability δ that an applicant contacts the employer.[1] Then $1/\delta$ denotes the expected number of periods between applicants.[2] The per period cost of the vacancy will be the sum of direct on-going recruiting cost as well as indirect costs reflecting the loss of a vacancy remaining unfilled. Recall from chapter 2 that when an applicant contacts the employer, $p_b(E, \alpha, 0)$ denotes the productivity of the applicant in the absence of any training, where E denotes general human capital, α is a measure of the individuals ability level, and 0 denotes the zero vector of total acquired training, both general and specific. Employers offering training during the first or beginning period of employment incur costs $c(T)$ reflecting the loss in productivity of the worker during training, the loss in productivity of co-workers who provide the training, and other training expenses. Thus, the net productivity of a "beginning" worker during the training period is given by:

$$(7.1) \quad f_b = p_b(E, \alpha, 0) - c(T).$$

Recall from chapter 2 that if a trained worker remains with the firm, his or her productivity during the second period of employment is enhanced by the training received. That is, the productivity of a worker with ability α and education level E "after" receiving training T is given by:

$$(7.2) \quad f_a = p_a(E, \alpha, T)$$

where $\partial p_a/\partial T > 0$ and $\partial p_a/\partial \alpha > 0$. We assume that increased ability α not only increases worker productivity, but also affects the return to training. In particular, it is assumed that $\partial^2 p_a/\partial T\partial \alpha > 0$. In words, the return to increased training is greater for more able workers (workers with a higher α).[3]

To introduce a rationale for employer search, we follow Jovanovic (1979b) and assume that the ability of an applicant, denoted by α, can be viewed as a random variable. We assume that the employer does not know α at the time of hire. The employer, however, incurs screening and interview costs to obtain a signal concerning the ability α of the applicant. While the signal acquired for each applicant is directly correlated with the applicant's true ability, the signal is not perfect. Thus, some mistakes will be made in hiring. Such errors in hiring will be discovered after a period of employment. Unfortunately, this type of learning is expensive because the employer will have already made the investment in firm-specific training.

In the above setting, the employer optimal search strategy can be viewed as making two decisions. One is to determine the minimal or "reservation signal" for acceptable applicants. An employer searches until finding an applicant with an ability signal equal to or greater than this reservation signal. Such an applicant will be offered employment. To keep our focus on employer search, we assume that any applicant who is offered employment accepts. An employer can reduce hiring mistakes by raising the reservation signal, but the cost of doing so is an increase in the expected number of applicants seen before an acceptable one is found. The extent of an employer's *extensive search* is reflected by this expected number of applicants seen prior to an employment offer.

The second decision of an employer's optimal search strategy is to determine the precision of the productivity signal obtained for each applicant. By spending more time screening and interviewing each applicant, the employer can obtain a better measure of the true ability of the applicant, and thus make fewer hiring mistakes. An increase in such *intensive search*, however, will raise the employer's direct search costs per applicant. Casual empiricism suggests that some employers spend a great deal of resources in trying to evaluate potential applicants. For instance, each year corporate recruiters come to campuses across the country to interview potential employees. Generally, the recruiters ask some of the students to make a visit to the company's plant, where more interviews are conducted. The expense of such recruiting can be quite high. Yet, employers are attempting to avoid the costs associated with hiring an ill-matched worker.

Of course, not all jobs have such a heavy investment in information before the match has begun. Fast-food restaurants do not require their employees to undergo such a rigorous selection process. Why the difference? Apparently, fast-food employers are more willing to wait for the information to be revealed after the employment relationship has begun. The next section considers more formally four specific factors that can help explain differences in the extent of extensive and intensive search.

Measures of Intensive and Extensive Search

To test our theory of employer search, and in particular the effect of training on employer search, we begin by obtaining explicit measures of an employer's intensive and extensive search choices from actual events surrounding the hiring of a new worker. Two related measures of the amount of *intensive search* can be constructed from the time the employer spent screening and recruiting applicants, one being the time spent per applicant and the second being the time spent per applicant interviewed. Two related measures of the amount of *extensive search* are also constructed. One is the number of applicants screened per employment offer, and the other is the number of applicants interviewed per offer. Below we suggest four propositions concerning factors that are likely to influence these measures of intensive and extensive search.

Four Propositions Concerning Employer Search

The following four factors can affect an employer's choice of intensive and extensive search: the level of training (T), the dispersion in the ability distribution across applicants, the rate at which applicants arrive at the employer (δ), and the implicit cost of vacancy. Our data allow us to identify variables to proxy each of these four factors.

One key factor that influences employer search choices is the extent of training. Positions with higher training will impose larger losses on employers during the first period of employment but provide offsetting greater gains to the employment of trained workers in subsequent periods. The model predicts that for positions that require higher training, employers will typically increase expenditures on screening and interviewing each applicant (greater intensive search) as well as increase the reservation signal level, leading to an increase in the expected number of applicants (greater extensive search). The reason is straightforward. Positions that require greater training offer a greater gain to making fewer hiring mistakes, and both intensive and extensive search may reduce the number of mistakes. Thus, we have:

Proposition 1: Employers filling positions that require greater training will engage in greater extensive or intensive search.

Note that the reason why an increase in training may not increase both extensive and intensive search is the potential for substitution between the two types of search. For instance, an increase in training can lead to an increase in intensive search, which in turn may lower the gain to extensive search sufficiently such that the optimal level of extensive search falls. This substitution possibility is the reason for ambiguity not only here but in the next three propositions.

Naturally, factors other than training can influence employer search. A second factor that affects employer search is the inherent dispersion in the ability of workers. Leaving the mean unchanged, an increase in the dispersion of the ability distribution induces a gain to a more stringent reservation signal. To see why, note that increased dispersion can be interpreted as an increase in the likelihood of visits by applicants with very high ability signals. Focusing first on extensive search, it follows that the employer faces a greater gain to continued search for such high-productivity applicants. To promote continued search, the employer raises his or her reservation ability signal. Thus, as predicted by standard search theory, the result is an increase in extensive search. As employer search occurs at both the extensive and intensive margins; however, employers have two ways of detecting the high-ability workers. One way, just discussed, is for employers to increase the reservation signal level, which results in an increase in the expected number of applicants seen prior to hiring. But to better discover the high-ability applicants, the employer can also search more intensively, increasing the information content of the signal. Formally, we have:

Proposition 2: Employers filling positions for which the dispersion in ability is larger will engage in greater extensive or intensive search.

A third factor that affects employer search is the rate at which applicants arrive at the employer (δ). As an increase in the rate of flow of applicants reduces the costs of additional search, employers filling positions that have a greater applicant flow will raise the reservation productivity level, increasing the expected number of applicants. At the same time, such employers may reduce expenditures on screening and interviewing applicants, as increased extensive search substitutes for such expenditures. We thus have the following proposition:

Proposition 3: Employers filling positions where the applicant flow is greater will engage in either more extensive search or less intensive search.

A fourth factor that affects employer search is the implicit cost of having a vacant position. Let us assume that sometimes an employer receives advanced notice of a vacancy. We interpret the existence of advance notice as a reduction in the cost of search to the employer because the employer can undertake the search while the current employee is still working. Such a reduction in per-period search costs increases the optimal reservation signal because the employer does not forgo the production associated with a vacant position. On the other hand, there is no clear prediction concerning the effect of such a change on intensive search. Formally, we have:

Proposition 4: When employers receive advance notice of an opening, they search more extensively, but the effect on intensive search is ambiguous.

Empirical Specification of Employer Search Equations

The above four propositions suggest five factors that affect extensive and intensive search. One is a measure of the total amount of initial training. From Proposition 1, we expect this measure of training to be directly correlated with both extensive search and intensive search, as employers increase efforts devoted to search in response to the greater loss from hiring mistakes in positions that involved greater amounts of initial training.

Proposition 2 indicates that increased productivity variation will lead to increased employer search, intensive and/or extensive. The productivity of a randomly chosen applicant is a function of two factors: the applicant's general level of human capital and his or her innate ability to perform the tasks required by the position of a particular employer. For workers with the same level of general human capital, the dispersion in the productivity distribution then depends on the underlying dispersion in innate abilities as well as the interaction of ability and general human capital in determining productivity. If general human capital and ability are complementary inputs in determining productivity, which is what we assume, then a given distribution of abilities will generate a more dispersed distribution of productivity among individuals with a higher level of general human capital. One measure of general human capital is formal education. This leads us to expect greater productivity variation among workers with higher levels of formal education. Thus from Proposition 2 we expect that employer search will be greater for positions that are filled by more highly educated individuals. A second measure of general human capital is labor force experience. Our data contain measures of whether the worker who was hired had any relevant experience for the position. We use this as a proxy for the level of general human capital gained through experience, and expect that the dispersion of productivity among workers with no prior experience will be less because they have less general human capital. From Proposition 2, we thus expect employer search will be less for positions hiring workers with no experience.

Another factor affecting employer search is establishment size. We take the establishment size of the employer as one measure of the rate of flow of applicants δ. In particular, we expect larger employers to experience economies of scale in generating applicants for vacant positions.[4] Thus from Proposition 3 we expect that larger employers will engage in either more extensive or less intensive search.

Finally, from Proposition 4 we expect that when an employer has advanced notice of a vacancy, the employer will see more applicants prior to an employment offer (greater extensive search).

To summarize, the model provides the following two equations to be estimated with regard to the determinants of employers' intensive search and extensive search to fill a position:

$$(7.3)\quad \mathrm{IS} = a_o + a_1\,\mathrm{TRAIN} + a_2\,\mathrm{EDUC} + a_3\,\mathrm{ZEROEXP} + a_4\,\mathrm{SIZE} + a_5\,\mathrm{ADVNOTICE} + a_6\,X + \varepsilon$$

$$(7.4)\quad \mathrm{ES} = \mathbf{b}_o + b_1\,\mathrm{TRAIN} + b_2\,\mathrm{EDUC} + b_3\,\mathrm{ZEROEXP} + b_4\,\mathrm{SIZE} + b_5\,\mathrm{ADVNOTICE} + b_6\,X + \varepsilon$$

where IS is the log of the intensive search measure (time spent per applicant or time spent per interview), *ES* is the extensive search measure (number who applied or the number interviewed), TRAIN is the log of the measure of total training, EDUC is the log of the number of years of education required, ZEROEXP indicates that the worker had no prior experience in a similar position, *SIZE* is the log of establishment size, ADVNOTICE indicates that the employer had advance notice of the vacancy, and X is a vector of two control variables that indicate whether the position was associated with a union and the logarithm of the number of hours the position required per week. As discussed above, Propositions 1 through 4 imply the following signs for the coefficients: $a_1 > 0$ and $b_1 > 0$; $a_2 \geq 0$ and $b_2 \geq 0$, with strict inequality for at least one of the two coefficients; $a_3 \leq 0$ and $b_3 \leq 0$ with strict inequality for at least one of the two coefficients; $a_4 \geq 0$ and $b_4 \leq 0$ with strict inequality for at least one of the two coefficients; and $a_5 > 0$ and no predicted sign for b_5.

The Evidence on Employer Search Behavior

To test the predictions of the model as summarized by equations (7.3) and (7.4), we employ four data sets: the 1980 EOPP employer survey, the 1982 EOPP follow-up employer survey, a 1992 survey of employers financed by the Small Business Administration (SBA), and a 1993 employer survey financed by the W. E. Upjohn Institute. Except for search variables, the contents of the first surveys have been described in detail in earlier chapters. The Upjohn Institute Survey is described in detail in chapter 7. The four surveys asked employers a number of common questions about their search activities during the period prior to their most recent hire. In particular, all four surveys contained questions regarding the number of applicants interviewed for the position filled, the number of individuals who turned the employer down, if any, and the total number of hours spent by company personnel recruiting, screening, and interviewing all applicants. We obtain a measure of intensive search by dividing the total number of hours spent recruiting, screening, and interviewing applicants by the total number of applicants interviewed.[5] We obtain a measure of extensive search by dividing the total number of applicants interviewed by the total number of offers made.[6] Dividing the total number of offers made by the total number hired provides us with a measure of the number of offers made prior to an offer being accepted. In our discussions in the previous section, we assumed this variable equaled one. Multiplying intensive search, extensive search, and the number of offers made per hire provides an overall measure of the total number of hours spent recruiting, screening, and interviewing applicants per hire.

Columns 1, 3, and 5 in table 7.1 report the means of these various "interview-based" measures of employer search for the four data sets. To illustrate the differences in sampling strategies across surveys, table 7.1 breaks down the magnitude of these search measures by employer size. As the table indicates, there is a substantially larger proportion of smaller employers in the two EOPP surveys relative to the SBA and Upjohn Institute surveys. After controlling for size, however, the magnitudes of the various measures of employer search are quite similar across the four surveys, with one key exception. The total hours spent recruiting, screening, and interviewing applicants is much lower for the 1980 EOPP survey than for the other three surveys. The reason for this may be the way in which this question was framed. Unlike the other three surveys, the 1980 EOPP survey asked employers only about the number of job applicants interviewed for the position, not the total number of applicants. Thus, the answers may reflect hours involved in the interviewing process alone, not the total hours devoted to all phases of hiring for the position.

Except for the 1980 EOPP survey, the surveys also obtained information on the total number of applicants for these positions. This permits alternative measures of both intensive search and extensive search. The alternative extensive search measure is the total number of applicants seen prior to an offer, rather than the number of applicants interviewed prior to an offer. The alternative intensive search measure is the number of hours spent recruiting, screening, and interviewing applicants per applicant, rather than the number of hours spent searching per applicant interviewed. Columns 2 and 4 of table 7.1 report means of these two variables where available.

The surveys asked employers to provide information concerning on-the-job training, although the surveys differ in the measures of training provided. The 1980 EOPP survey asked two training questions:

> In the first month of employment, approximately how many hours did employees other than personnel and supervisory staff spend

Table 7.1 Employer Search, Vacancy Duration, and Training Variables by Size, 1980 EOPP; 1982 EOPP; 1992 SBA; 1993 Upjohn Institute Surveys

Employer Size	Number of hours spent per interview (1)	Number of hours spent per applicant (2)	Number of interviews per offer (3)	Number of applicants per offer (4)	Total number of hours spent per offer (5)	Number of offers made per hire (6)	Duration of vacancy in days (7)	Total training (hours) (8)	Number of observations (9)
EOPP, 1980	.83	NA	5.69	NA	5.69	1.02	13.39	33.71	2994
1-99	.80	NA	5.38	NA	5.38	1.02	13.46	32.91	2552
100-299	.96	NA	7.02	NA	7.02	1.00	12.02	37.87	300
300+	.97	NA	8.79	NA	10.75	1.02	15.13	39.33	142
EOPP, 1982	2.12	2.17	5.91	9.87	10.41	1.08	17.21	136.15	1270
1-99	1.97	2.14	5.79	8.85	9.36	1.07	16.66	131.99	1083
100-299	2.78	2.18	6.94	11.24	16.53	1.08	22.52	123.30	118
300+	3.33	2.67	5.94	23.42	16.50	1.16	16.67	223.33	69
SBA, 1992	2.73	2.11	5.58	14.08	14.03	1.14	NA	168.43	859
1-99	2.04	1.88	5.32	9.72	10.14	1.13	NA	152.72	428
100-299	2.68	2.06	5.96	13.48	16.26	1.16	NA	161.35	102
300+	3.63	2.43	5.81	19.93	18.40	1.14	NA	191.07	329
UPJOHN, 1993	3.21	1.61	6.02	22.94	18.79	1.16	30.35	83.42	210
1-99	1.52	1.08	8.39	15.68	11.75	1.46	19.23	81.1	30
100-299	3.18	1.76	5.66	18.82	18.82	1.21	29.67	78.40	58
300+	3.65	1.68	5.64	27.17	20.51	1.06	33.41	86.38	122

NOTE: For comparability across the four surveys, the sample from each survey is restricted to only those who had been hired within the last two years of the survey. The 1982 EOPP, 1992 SBA, and 1993 Upjohn Institute surveys are for the last permanent new employee hired. Information on whether the new hire was temporary, seasonal, or permanent was not available for the 1980 EOPP survey.

> away from their normal work routines orienting and training the new hire?
>
> In the first month of employment, approximately how many hours did personnel and advisory staff spend orienting and training the new hire?

The sum of these two measures provides a direct measure of the total amount of on-the-job training, TRAIN, received by a new worker during the first month.[7] Column 8 of table 7.1 reports averages of this measure of training for the 1980 EOPP sample.

The other three surveys contained a common set of four questions concerning various types of on-the-job training, although the period of time over which training was measured differed among the three surveys. The 1982 EOPP survey and the 1992 SBA survey asked for the total number of hours typically spent during the first three months of employment (a) by specially trained personnel providing formal training to the most recently hired worker, (b) by line supervisors and management personnel providing the new worker with informal individualized training and extra supervision, (c) by co-workers away from other tasks in providing the new worker with informal individualized training and extra supervision, and (d) by the worker watching others perform tasks. The average total time spent on these four training activities, reported in column 8 of table 7.1, is 136.14 hours for the 1982 EOPP data and 168.43 hours for the SBA data. These sums provide direct measures of the total amount of on-the-job training, TRAIN, received by a new worker for the 1982 EOPP and 1992 SBA surveys, respectively.[8] For the 1993 Upjohn Institute survey, the training measure is obtained from questions almost identical to those of the 1982 EOPP and 1992 SBA surveys, with the important difference being that the training questions concern only the first month, not the first three months, of employment. That explains why the average level of training for this survey, reported in column 8 of table 7.1 as 83.42 hours, is substantially below the mean for the other two surveys.

With few exceptions, measures of the other variables included in equations (7.3) and (7.4) as determinants of employer search are available in all four data sets. In all four surveys, the employer provided information on the education level of the individual hired and whether the individual hired had any experience in jobs that had some application to the position. We use responses to these two questions to infer the required education level of the position (EDUC) and if the employer did not require any prior experience (ZEROEXP). All four surveys also report the size of the establishment (SIZE). Three of the four surveys, the exception being the 1980 EOPP survey, contain a measure of the number of hours that the newly hired employee typically works. Each of the four surveys has a measure of the collective bargaining or union status of the position filled. For the two EOPP data sets, this union variable is the proportion of all positions at the establishment that are represented by a collective bargaining agreement. For the SBA and Upjohn data sets, this union variable is more specific; the employer identified whether the particular position filled was a union position. We control for union status because Holzer, Katz, and Krueger (1991), using the EOPP data, argue that queues may exist for union jobs. Only the 1982 EOPP and the 1993 Upjohn Institute surveys asked employers whether they knew in advance of the existence of the opening.

Tables 7.2 through 7.5 report estimates of equations (7.3) and (7.4) for the 1980 EOPP data, 1982 EOPP data, the 1992 SBA data, and the 1993 Upjohn Institute data, respectively. For the intensive search equations (both the number of hours per applicant interviewed measure and, for the most recent three data sets, the number of hours per applicant measure), we report the standard ordinary least squares estimates with results corrected for heteroskedasticity. These results appear in columns 1 and 2 of tables 7.2 through 7.5. Because our measure of extensive search involves sampling from a stationary distribution with a time invariant stopping rule, the theory suggests that the average number of applicants seen per acceptable offer is exponentially distributed.[9] Therefore, the results reported in columns 3 and 4 of tables 7.2 through 7.5 reflect the estimation of a maximum-likelihood exponential distribution (number of applicants) model using individual-level data.[10]

The results reported in tables 7.2 through 7.5 provide robust evidence supporting the employer search model we developed. Using four data sets collected over a fifteen-year period, we find strong support that employer search varies systematically by the type of position filled. Our results, summarized in table 7.6, indicate that all four data sets support the prediction that employers will search more for positions that require greater training, with three of the four indicating significantly greater search at both the intensive and extensive margins.

Table 7.2 Determinants of Employer Search, 1980 EOPP Survey

		Intensive search (IS)		Extensive serach (ES)		
		Log of number of hours spent per interview	Log of number of hours spent per applicant	Number of interviews per offer	Number of applicants per offer	Number of offers made per hire
		OLS White (Huber) correction	OLS White (Huber) correction	Exponential distribution	Exponential distribution	Exponential distribution
Variable	Mean	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)
Constant		-.757 (3.95)	NA	-1.40 (4.05)	NA	-.051 (0.15)
Log of total training, 1st month	2.81	.044 (5.63)	NA	.161 (11.76)	NA	.002 (0.13)
Log number of years education	2.48	.272 (3.55)	NA	.975 (7.04)	NA	.039 (0.29)
No prior experience in position	.267	-.077 (3.17)	NA	-.216 (5.11)	NA	-.012 (0.28)
Log of employer size	2.97	-.004 (0.48)	NA	.096 (7.46)	NA	-.009 (0.67)
Advanced notice of opening	NA	NA	NA	NA	NA	NA

Proportion of firm unionized	.105	.035 (0.85)	NA	-.131 (1.89)	NA	-.007 (0.10)
Log number of hours per week	NA	NA	NA	NA	NA	NA
Mean, dependent variable		.013	NA	5.69	NA	1.02
Number of observations		2,994	NA	2,994	NA	2,994
Adjusted R-squared or Chi-square		.02	NA	317.41	NA	.74

Table 7.3 Determinants of Employer Search, 1982 EOPP Survey

		Intensive search (IS)		Extensive search (ES)		
		Log of number of hours spent per interview	Log of number of hours spent per applicant	Number of interviews per offer	Number of applicants per offer	Number of offers made per hire
		OLS White (Huber) correction	OLS White (Huber) correction	Exponential Distribution	Exponential Distribution	Exponential Distribution
Variable	Mean	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)
Constant		-2.98 (4.00)	-3.11 (3.72)	-.422 (0.69)	.779 (1.19)	-.042 (0.07)
Log of total training, 1st three months	4.13	.117 (7.48)	.087 (4.34)	.057 (2.92)	.097 (5.07)	.005 (0.26)
Log number of years education	2.52	.716 (2.52)	.731 (2.37)	.472 (2.28)	.302 (1.40)	.023 (0.11)
No prior experience in position	.339	-.166 (3.41)	-.117 (1.91)	-.062 (1.03)	-.031 (0.52)	.044 (0.73)
Log of employer size	3.03	.072 (4.06)	-.031 (1.35)	.053 (2.53)	.211 (10.02)	.007 (0.33)
Advanced notice of opening	.60	.133 (2.80)	-.019 (0.33)	.180 (3.09)	.335 (5.72)	-.014 (0.24)

Proportion of firm unionized	.098	.005	-.268	-.180	.119	-.067
		(0.05)	(2.01)	(1.64)	(1.06)	(0.61)
Log number of hours per week	3.60	.239	.393	.146	-.160	.005
		(3.12)	(3.58)	(1.34)	(1.46)	(0.05)
Mean, depend. variable		.409	.341	5.91	9.86	1.08
Number of observations		1,294	1,294	1,270	1,270	1,270
Adjusted R-squared or Chi-square		.11	.05	43.12	213.68	1.15

Table 7.4 Determinants of Employer Search, 1992 SBA Survey

		Intensive search (IS)		Extensive search (ES)		
		Log of number of hours spent per interview	Log of number of hours spent per applicant	Number of interviews per offer	Number of applicants per offer	Number of offers made per hire
		OLS White (Huber) correction	OLS White (Huber) correction	Exponential distribution	Exponential distribution	Exponential distribution
Variable	Mean	Coefficient (*t*-statistic)	Coefficient (*t*-statistic)	Coefficient (*z*-statistic)	Coefficient (*z*-statistic)	Coefficient (*z*-statistic)
Constant		-4.30 (6.77)	-1.63 (2.12)	-1.13 (1.75)	-4.33 (7.18)	.248 (0.39)
Log of total training, 1st three months	4.41	.122 (5.18)	.077 (2.31)	.018 (0.69)	.061 (2.15)	.017 (0.63)
Log number of years education	2.59	1.22 (5.72)	.600 (2.31)	.777 (3.37)	1.59 (7.39)	-.040 (0.18)
No prior experience in position	.280	-.140 (1.92)	-.077 (0.86)	-.250 (3.17)	-.360 (4.60)	.021 (0.27)
Log of employer size	4.90	.043 (3.41)	-.024 (1.62)	.015 (1.09)	.123 (8.62)	.003 (0.22)
Advanced notice of opening	NA	NA	NA	NA	NA	NA
Union position	.090	.022 (0.20)	.127 (0.88)	-.122 (1.00)	-.040 (0.33)	.061 (0.51)

Log number of hours per week	3.54	.272 (2.79)	.034 (0.33)	.209 (2.18)	.545 (5.79)	-.033 (0.36)
Mean, dependent variable		.545	.248	5.58	14.08	1.14
Number of observations		859	859	859	859	859
R-square or Chi-square		.14	.02	42.53	267.58	1.01

Table 7.5 Determinants of Employer Search, 1993 Upjohn Institute Survey

		Intensive search (IS)		Extensive search (ES)		
Dependent variable:		**Log of number of hours spent per interview**	**Log of number of hours spent per applicant**	**Number of interviews per offer**	**Number of applicants per offer**	**Number of offers made per hire**
Model estimated		**OLS White (Huber) correction**	**OLS White (Huber) correction**	**Exponential distribution**	**Exponential distribution**	**Exponential distribution**
Variable	**Mean**	**Coefficient (*t*-statistic)**	**Coefficient (*t*-statistic)**	**Coefficient (*z*-statistic)**	**Coefficient (*z*-statistic)**	**Coefficient (*z*-statistic)**
Constant		-5.91 (4.68)	-2.98 (2.02)	-0.99 (0.72)	-6.47 (4.35)	.651 (0.47)
Log of total training, 1st month	4.05	.108 (1.79)	.044 (0.58)	.135 (1.93)	.132 (1.87)	-.022 (0.30)
Log number of years education	2.61	1.86 (4.14)	1.30 (2.54)	.607 (1.29)	1.96 (3.86)	-.254 (0.52)
No prior experience in position	.148	-.207 (1.37)	.129 (0.61)	-.391 (1.94)	-.834 (3.86)	-.067 (0.34)
Log of employer size	6.14	.045 (1.19)	-.009 (0.18)	-.077 (1.83)	.037 (0.85)	-.043 (0.95)
Advanced notice of opening	.743	.029 (0.23)	-.119 (0.61)	.197 (1.21)	.618 (3.75)	.006 (0.03)
Union position	.100	-.114 (0.59)	-.459 (1.85)	-.052 (0.22)	.292 (1.99)	.009 (0.04)

Log number of hours per week	3.57	.312 (2.11)	-.119 (0.61)	.283 (1.29)	.890 (4.02)	.145 (0.68)
Mean, depend. variable		.733	.000	6.02	22.94	1.16
Number of observations		210	210	210	210	210
R-squared or Chi-square		.17	.04	17.81	79.59	1.96

Table 7.6 Summary of Findings Concerning Determinants of Employer Search and Vacancy Duration Across Four Surveys

		1980 EOPP			1982 EOPP			1992 SBA			1993 Upjohn		
Interview based employer search measures	**Predicted signs for extensive search (ES) and intensive search (S)**	**Interviews per offer (ES)**	**Hours per interview (IS)**	**Duration of a vacancy**	**Interviews per offer (ES)**	**Hours per interview (IS)**	**Duration of a vacancy**	**Interviews per offer (ES)**	**Hours per interview (IS)**	**Duration of a vacancy**	**Interviews per offer (ES)**	**Hours per interview (IS)**	**Duration of a vacancy**
Total training	ES+; IS+ Duration: sign of ES	+*	+*	+*	+*	+*	+*	+	+*	NA	+*	+*	+*
Years of education	ES or IS+ Duration: sign of ES	+*	+*	+*	+*	+*	+*	+*	+*	NA	+	+*	+*
No prior experience	ES or IS+ Duration: sign of ES	-*	-*	-*	-	-*	-*	-*	-*	NA	-*	-	-*
Employer size	ES+ or IS- Duration: ?	+*	-	-*	+*	+*	-*	+	+*	NA	-*	+	+
Advanced notice of opening	ES+; IS? Duration: sign of ES	NA	NA	NA	+*	+*	+*	NA	NA	NA	+	+	+

Applicant based employer search measures		Applicants per offer (ES)	Hours per applicant		Applicants per offer (ES)	Hours per applicant		Applicants per offer (ES)	Hours per applicant		Applicants per offer (ES)	Hours per applicant	
Total training	ES+; IS+ Duration: sign of ES	NA	NA		+*	+*		+*	+*		+*	+*	
Years of education	ES or IS+ Duration: sign of ES	NA	NA		+	+*		+*	+*		+*	+*	
No prior experience	ES or IS+ Duration: sign of ES	NA	NA		-	-*		-*	-		-*	-	
Employer size	ES+ or IS- Duration: ?	NA	NA		+*	-		+*	-*		+	-	
Advanced notice of opening	ES+; IS ? Duration: sign of ES	NA	NA		+*	-		NA	NA		+*	-	

NOTE: An asterisk indicates the coefficient is significantly different from zero at the .10 significance level. Pluses and minuses indicate the sign of the coefficient.

There is similarly strong support for the prediction that employers search more extensively for positions that require greater levels of education.[11] Further, all four data sets provide at least partial support for the contention that employers search less on the extensive margin for positions that do not require prior experience, and three of the four data sets support the contention that larger employers search more at the extensive margin. On the other hand, there is no strong evidence that larger employers search less intensively. Estimates of equations (7.3) and (7.4) that include the advance notice variable for the 1982 EOPP and 1993 Upjohn Institute data sets indicate, as expected, that while advance notice has an indeterminate sign with respect to intensive search, the advance notice of a vacancy does lead to an increase in extensive search.

There is an additional finding from the four surveys that is of interest. Column 5 in tables 7.2 through 7.5 reports the estimation of a survival model for the number of offers made per hire. All four surveys indicate that it is uncommon for applicants to reject wage offers (see column 6 of table 7.1): in all four data sets, workers accept at least 85 percent of all job offers. Further, there is no systematic pattern to the rejection of wage offers across positions.

The Evidence on Vacancy Duration

The analysis in the previous section provides not only a framework for the analysis of factors affecting employer intensive and extensive search, but also suggests determinants of the duration of a vacancy. The model specifies that the duration of a vacancy is an exponentially distributed random variable with expected value $1/(\delta(1 - H(s_r))$, where δ is the per-period probability that an applicant will contact the employer seeking to hire and $(1\text{-}H(s_r))$ is the probability that an applicant is acceptable given the distribution of ability signals $H(\cdot)$ and reservation signal s_r. Thus, the expected duration of a vacancy is an increasing function of the employer's reservation ability signal choice, s_r, and a decreasing function of the probability that an applicant contacts the employer, δ.

This implies the hazard rate function for vacancy duration is time independent.[12] In this case, letting D denote the log of the duration time for a vacancy, the model we estimate is given by:

$$(7.5)\quad D = c_o + c_1\,\text{TRAIN} + c_2\,\text{EDUC} + c_3\,\text{ZEROEXP} + c_4\,\text{SIZE} + c_5\,\text{ADVNOTICE} + c_6\,\text{X} + \varepsilon$$

where the predicted signs for the coefficients in equation (7.3) are the same as those for the extensive search equation (7.4), with one exception. The exception is the employer size variable. Recall that while larger employers are predicted to search more extensively (higher s_r), the rationale for this is that larger employers have a greater probability of an applicant contact each period (a higher δ). These two changes (higher s_r and higher δ) have offsetting effects on the duration of a vacancy. Thus, the effect of size on the duration of a vacancy is ambiguous. Given that we have found that extensive search increases with training, education, and advance notice, and decreases in positions that require no prior experience, then these variables should have similar effects on the duration of a vacancy.

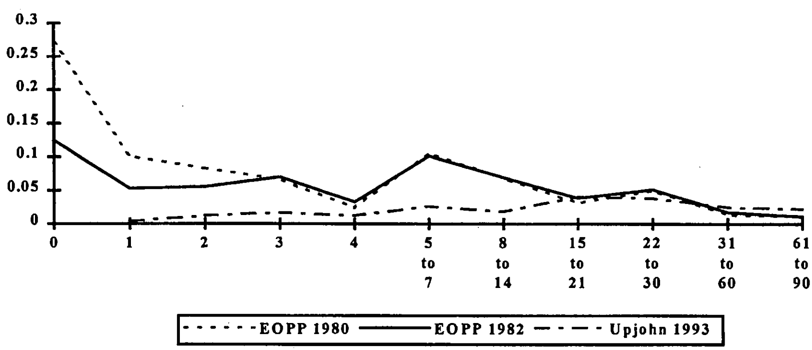
Because the data are measured in days and the number of observations in our data sets are relatively large, we treat the data as discrete.[13] While our theory implies that the hazard function should be time invariant, van Ours (1988) and Renes (1989) report evidence of duration dependence. To check this result for U.S. data, we estimate the vacancy duration model using a discrete hazard model that Kalbfleisch and Prentice (1973) suggest. Meyer (1990) recently applied this model to unemployment spells. Let the duration time have a discrete distribution with mass points at $0 \le z_1 < z_2 \cdots$. Then the Kalbfleisch and Prentice model implies the hazard at z_t for covariate x is:

$$(7.6)\quad 1 - (\,1 - \gamma t\,)^{\exp[-xC]}$$

where γ_t and the coefficient vector C are parameters estimated using maximum likelihood techniques.[14] The discrete, time-invariant hazard function is $1 - (\,1 - \gamma)^{\exp[-xC]}$, which is simply a special case of equation (7.6). We estimated both models for each of the three data sets that contain information on the duration of the vacancy and overwhelmingly reject the constant hazard function specification for each of the three data sets.

For the 1982 EOPP and 1993 Upjohn data sets, the duration of a vacancy is measured by the number of days "between the time (the employer) started looking for someone to fill the opening and the time (a new employee) started to work." For the 1980 EOPP, the term "recruit for the job" was used instead of "looking for someone to fill the opening."[15] Columns 1 through 3 of table 7.7 report the results of this estimation of the vacancy duration equation. The estimated hazard rates for the three separate data sets are plotted in figure 7.1. There is some evidence of time dependence. For instance, one pattern indicated by figure 7.1 is that the probability of filling a vacancy rises at the end of the first week. It appears that a common event is for an employer to fill a vacancy after a week of search. A second pattern, apparent in the

Figure 7.1 Vacancy Duration Hazard Rates (duration in days)



two EOPP data sets, is a relatively high likelihood of a vacancy being immediately filled. These are positions for which the employer either indicated that there were no days between the time he or she started to look for someone to fill the opening and the time that the new employee started to work or that he or she did not have to look for someone to fill the position. One reason that the immediate filling of vacancies does not occur in the Upjohn Institute data set is that firms who fill positions immediately tend to be smaller employers, and

Table 7.7 Vacancy Duration Models

Variable	1980 EOPP Coefficient (z-statistic)	1982 EOPP Coefficient (z-statistic)	1993 Upjohn Coefficient (z-statistic)	Duation (in days)	1980 EOPP Estimated hazard rates[a]	1982 EOPP Estimated hazard rates[a]	1993 Upjohn Estimated hazard rates[a]
Log of total training 1st three months (1st month for EOPP 1980)	.112	.108	.141	0	0.2706	0.1243	---
	(8.33)	(5.32)	(1.78)				
Log number of years education	.574	.886	2.432	1	0.1005	0.0528	0.0039
	(4.32)	(4.88)	(4.50)				
No prior experience in position	-.201	-.111	-0.888	2	0.0829	0.0551	0.0120
	(4.71)	(1.79)	(4.01)				
Log of employer size	.004	-0.16	.003	3	0.0662	0.0700	0.0163
	(0.34)	(0.79)	(0.68)				
Advanced notice of opening	NA	.259	.281	4			
		(4.38)	(1.59)		0.0235	0.0324	0.0125
Proportion of firm unionized (union position for Upjohn 1993)	-.015	-.206	.429	5-7	0.1054	0.1013	0.0261
	(0.22)	(1.81)	(1.65)				
Log number of hours per week	NA	.490	.491	8-14	0.0685	0.0691	0.0179
		(4.77)	(2.26)				
Median of dependent variable (in days)	14	14	22	15-21	0.0353	0.0384	0.0405
Number of observations	2,994	1,270	210	22-30	0.0494	0.0512	0.0380
Log likelihood	-6400.09	-2794.35	-394.94	31-60	0.0134	0.0172	0.0247
				61-90	0.0107	0.0110	0.0213

a. We calculate hazard rates using the mean value of the covariates for each sample.

smaller employers were specifically excluded from the Upjohn Institute sample. Our findings generally support the presumption that changes in variables that lead to more extensive search are the same changes that lead to an increase in the duration of a vacancy. The one exception, employer size, supports our conjecture that it should have different effects on extensive search and vacancy duration. While larger employers do search more extensively, the expected duration of a vacancy is equal to or less than that of smaller employers.

Recently, van Ours (1988) and Renes (1989), using Dutch data, explored the determinants of vacancy duration. They found that vacancy duration was higher at positions with high education, experience, and skill requirements. Our findings for the United States across three data sets are consistent with these findings: we find that vacancy duration is significantly greater in positions requiring more training, more highly educated workers, and individuals with some prior experience. Our findings, however, indicate a *much* shorter duration of a vacancy than that found in the Dutch data. From table 7.1, mean vacancy duration is under a month, while the average vacancy duration for the Dutch data was about six months. Some of this difference may be explained by the fact that the Dutch data was drawn by sampling vacancies, then resampling the employers later to determine the time taken to fill that particular vacancy. Such a sampling strategy overrepresents positions that have long vacancy durations. More in line with our data are Scottish data reported by Beaumont (1978), who found an average vacancy duration of between 10 and 15 days during the 1973-1975 period.

Conclusions

While much has been written concerning job search, researchers have placed less attention on employer search. This appears odd given our results, which confirm that systematic patterns to employer search do exist and that employer search is an important component of the matching of workers to positions. Table 7.8 indicates that the median duration of unemployment is substantially higher for workers in managerial and professional occupations, occupations that typically require higher levels of education and more training than service occupations. Our finding that employers search more extensively for positions that require greater formal education and training may help explain this difference in unemployment duration. More extensive search by employers means that workers must on average locate more vacancies per job offer. Our results on extensive search by employers are also consistent with Holzer's (1994) finding that positions involving greater job skill requirements generally have higher vacancy rates. That we find that employers search more extensively to fill such positions (with a resulting longer duration of a vacancy at these positions) implies higher vacancy rates, other things equal.[16]

Table 7.8 Employer Search and Unemployment Duration

Occupation	Median duration (weeks) of unemployment[a]	Mean number of applicants per offer (SBA)
Service	7.3	7.7
Operators, fabricators, and laborers	9.0	12.9
Technical, sales, and administrative support	10.1	14.8
Precision production, craft, and repair	12.2	14.5
Managerial and professional	12.6	18.7

a. As the duration of unemployment data is censored, we report the median rather than the mean of the interrupted duration spells as it is likely to provide a better measure of the underlying mean duration of completed spells of unemployment. Employer extensive search is measured by the mean number of applicants seen per offer reported by the Spring 1992 SBA survey. The median duration of unemployment is taken from the June 1992 issue of *Employment and Earnings* and for the month of May 1992.

One important extension of our analysis would be to consider further the interplay between employer search decisions and worker job search. For instance, as Montgomery (1991) emphasizes, if workers know the search behavior of employers, then employers who make relatively few offers per applicant will have to pay higher wages to compensate applicants engaged in costly job search for the reduced likelihood of receiving a job offer. Another issue that warrants further examination is the implication of employer search for the sharing of on-the-job training costs and returns. We assumed for simplicity that employers bear all the costs and reap all the returns to training. In a more general setting, however, the employer's share of training cost should be endogenously determined, with the employer bearing a larger portion of training costs and returns at positions requiring substantial training in order to induce a more careful matching of workers and positions (e.g., to induce the appropriate investment in employer search). This provides an additional rationale for the findings in chapter 5 of a weak relationship between starting wages and training.

NOTES

1. In modeling employer search, there are several options available. The simplest approach, which is the one we adopt, assumes a sequential strategy. This is standard in the literature that examines search from the worker's point of view. There are, however, situations in which the optimal search strategy combines elements of the fixed-sample-size approach with the sequential approach. The optimality of a search strategy that includes a fixed-sample-size element is demonstrated by Morgan and Manning (1985). For instance if there is a delay between the interviewing of an additional worker and the decision to hire, the fixed-sample-size element of search increases the speed at which the vacancy can be filled. Naturally, offsetting this gain is the potential of a fixed-sample-size strategy to result in an overinvestment in information. Bull, Ornati, and Tedeschi (1987) provide an illustration of an employer who chooses a combined fixed-sample-size/sequential search strategy. Employers do, in fact, often engage in search strategies that combine fixed-sample-size properties with sequential strategy. The added complexity of such a model of employer search, however, is not required to illustrate the interplay between belated information and employer search and to identify how factors such as the extent of on-the-job training influence employer search.

2. For simplicity, we assume the length of a period is sufficiently short such that the probability that two or more applicants contact the employer in the same period is approximately zero. We also choose not to complicate the model by having the employer choose the extent of (costly) advertising that would affect the speed at which applicants visit the employer. As noted by van Ours and Ridder (1992), such advertising can be the source of a pool of applicants.

3. We could also assume that higher-ability workers have lower costs of training. In other words, we could assume that $\partial^2 c/\partial T \partial \alpha < 0$, such that the reduction in output during the first period that results from an increase in training is less for higher-ability workers. The results to follow would not be affected by adding this feature to the model.

4. These economies of scale may arise in part due to the existence of personnel departments that increase the flow of applicants per vacant position. van Ours and Ridder (1992) do find that ". . . establishments with a personnel department . . . have more applicants" (p. 149).

5. Recall that the extent of intensive search excludes the indirect costs of a vacancy remaining unfilled. As this intensive search measure is computed by dividing the total time spent searching by the number interviewed, we add one to the number interviewed as reported by the employer in order that all employers, including those who reported interviewing zero individuals prior to hiring, are included in the analysis. Except for the 1980 EOPP data, the number reporting no interviews was under 10 percent. For the 1980 EOPP data set, approximately 30 percent of employers reported no one was interviewed. This large number may be due in part to the absence of a preceding framing question concerning the number of applicants for the position.

6. Given that we add one to the reported measures of the number interviewed to compute the intensive search measure, we add one to the number interviewed in computing the extensive search measure, such that the product of these intensive and extensive measures equals total search. Estimation of a survival model for extensive search also requires that the lower bound for the number interviewed must be above zero.

7. This training measure is clearly truncated as the training refers only to the first month of employment. The substantially greater estimates of training over the first three months of employment reported below for the 1982 EOPP and the 1992 SBA illustrate this point.

8. The fourth measure of training was in response to the question concerning the total hours spent by the worker "in training activities in which he or she is watching other people do the job rather than doing it himself" (EOPP data set) or "observing co-workers in order to learn skills required for the position" (SBA data set). One might omit this variable from our total training measures on the presumption that it introduces some double-counting given our other three training measures. The deletion of this fourth training variable, however, does not alter any of the findings reported below.

9. Thus, our measure of extensive search has features similar to duration data.

10. The significance of the reported results are robust to alternative estimation procedures. For instance, estimation of the model using Davidon, Fletcher, and Powell (DFP) algorithm provides identical coefficients and similar estimates of the standard errors. We also used robust confidence standard errors of Huber (1967) and White (1980) in the exponential models and our findings largely unchanged.

11. van Ours and Ridder (1992) have a similar empirical finding that employers see more applicants prior to hire at positions requiring higher educational levels.

12. In general, the vacancy duration hazard rate function λ_t is the probability that a vacancy is filled at any given instant t. Time (duration) independence means that $d\lambda_t/dt = 0$ for all t, and occurs if the distribution of vacancy duration is exponential.

13. See Kalbfleisch and Prentice (1980) for a good discussion of the estimation of discrete hazard models.

14. We follow Kalbfleisch and Prentice's (1980) suggestion and use the Kaplan-Meier estimates of the empirical hazard functions for the starting values of the γ's and $C = 0$. When $C = 0$, the method of Kalbfleisch and Prentice yields Kaplan-Meier estimates for the hazard function. Cox also proposes a discrete hazard model; if we adopt his approach, the parameter estimates for the C's and the estimates of the hazard function are very similar to the estimates produced by the method of Kalbfleisch and Prentice.

15. Holzer (1993a, 1994) and Holzer and Montgomery (1993) analyze the 1980 and 1982 EOPP data on firm vacancy rates, but do not examine the data on the duration of vacancy for the last worker hired by these firms. Holzer's (1994) finding concerning the factors that affect vacancy rates complements our findings concerning factors affecting employer extensive search and thus the duration of a vacancy.

16. By other things equal, we mean the following. Let P_i denote the number of positions of type i and let E_i denote the number employed in type i positions, such that the number of vacancies is $P_i - E_i$ and the vacancy rate for type i positions is given by $v_i = (P_i - E_i)/E_i$. Assume a constant exit rate q from the employed ranks that is the same across all types of positions. Then for type i positions, qE_i denotes new vacancies created each period. In the steady state, the number of vacancies for positions of type i that are filled equals qE_i, the likelihood that a vacancy is filled is $qE_i/(P_i - E_i)$, and the expected duration of a vacancy for a type i position is given by $D_i = 1/(qE_i/(P_i - E_i)) = v_i/q$. Thus, the steady-state vacancy rate for positions of type i equals the common rate at which employed positions are vacated times the expected duration of a vacancy for positions of type i.

CHAPTER 8

Conclusions

What have we learned about training from the EOPP, SBA, and Upjohn Institute data sets? First, all the data sets agree that nearly all newly hired workers undergo on-the-job training. Moreover, in the first three months of employment, both the EOPP and SBA data suggest that newly hired workers, their co-workers, and their supervisors spend the equivalent of nearly four 40-hour weeks in training. Several other surveys, however, demonstrate much lower incidence rates of training, even if the sample is restricted to newly hired workers. But, when newly hired employees and their employers are asked a similar set of questions about training, both groups agree that training is nearly universal. This correspondence suggests that researchers must carefully design surveys to capture fully the types and quantities of training that workers receive.

The data also indicate that college-educated workers and workers employed in large establishments receive more training than other workers. College-educated workers have shown a proficiency for accumulating human capital during schooling, and this proficiency apparently extends to the workplace. Presumably, large establishments find training less costly than smaller establishments. Large firms can spread any fixed costs associated with training over their larger number of new hires, and they are more able to have co-workers provide the training without experiencing significant productivity losses.

The EOPP, SBA, and Upjohn Institute data sets also show that training generates productivity growth. In the absence of explicit measures of productivity, we relied on a subjective measure in which the employer rates the new employee against a fully trained employee. The productivity index demonstrates that training results in substantial productivity growth: a 10 percent increase in training raises productivity 2 percent during the first three months of employment.

On-the-job training also increases wage growth. Whether looking at wage growth in the first three months or first two years of employment, the data indicate significant increases in wages associated with training. These findings confirm the predicted effects of on-the-job training on wage and productivity growth. The magnitude of the wage growth effect, however, suggests that current theories of on-the-job training are not satisfactory. A 10 percent increase in training results in only about a 0.2 percent growth in wages, or about one-tenth the magnitude of the impact of training on productivity growth. Such a differential could, with some difficulty, be justified if all training were firm-specific, but the impact of previous experience on wages makes it difficult to accept this proposition.

Further problems arise when trying to confirm the prediction that on-the-job training decreases the starting wage because workers bear at least some of the costs of training. We do find some evidence that training lowers the starting wage; a 10 percent increase in training in the first three months of employment lowers the starting wage between -0.5 and -0.2 percent. Workers appear to bear little of the training costs while firms bear most of the costs. Moreover, whether we infer the fraction of the training that is general training indirectly from the observed effect of previous experience on wages or directly from responses to survey questions, most of this training appears to be general training. The notion of firms bearing the costs of general training is inconsistent with any equilibrium model of labor markets of which we are aware.

If firms do bear a large portion of training costs, hiring the wrong worker for a job requiring a significant amount of training is a very costly mistake. Firms should be willing, therefore, to expend resources to avoid these mistakes by improving their recruiting when hiring for such positions. As we demonstrated in chapter 7, firms recruit more intensively (they spend more time with each applicant) and more extensively (they see more applicants per position) when recruiting for positions with more training. To the extent that this increased effort results in higher-quality workers, the firm matches higher-quality workers to positions that require more training. As higher-quality workers command higher wages, this finding helps explain why positions with substantial training do not have significantly lower starting wages.

Perhaps our most important findings come through the use of the Upjohn Institute survey that matched responses of employer and employees to a set of identical questions. We found significant differences in employer and employee reports across human capital measures, including both schooling and training measures. Training measures had correlation coefficients between employer and employee reports of only 0.2 to 0.4 for the individual measures and only about 0.5 for the aggregate measure. For education, the correlations are somewhat higher, especially at the higher levels of educational attainment, but there are substantial differences in employer and employee reports. Future research, therefore, must be concerned with the quality of the data used in attempting to measure the impact of human capital on wages.

For many years now, the wages of the least-educated Americans have declined while the wages of the most-educated Americans have increased. For instance, Katz and Murphy (1992) report that the earnings of high school dropouts between 1979 and 1987 declined 6.6 percent, while the earnings of college graduates increased 7.7 percent in the same period.[1] At least in part, these changes have created renewed interest in "investing in people" and mobilized policy makers to act on the perceived lack of investment in human capital. (So pervasive is this view that James Heckman (1993) refers to it as the "new consensus.") What insights do our findings provide concerning the forthcoming debates over how to increase the human capital of lower-income workers?

First, for policy makers who wish to argue that the market under-provides training opportunities for workers, our results offer some evidence for this view. Firms appear to finance a large amount of general training. While there may be other distortions that counterbalance the incentive to under-provide training, it seems unlikely, in our view, that such counterbalances would ever result in firms providing too much training. Thus, government may have an additional role to play in the human capital market.

Government already provides massive subsidies to education, and we may have too much schooling relative to job training in our human capital production. If policy makers wish to pursue policies that increase job training, our results also provide some insights into which policies may be effective. Most important, our results indicate that policy makers should not overlook the importance of informal training in the acquisition of job skills. By far the majority of employee training for all sizes of establishments, and especially for smaller establishments, is informal. Because workers and firms agree to provide this training voluntarily, we believe they do so because they find it efficient. Informal training is a less costly way of imparting human capital than formal training programs for the acquisitions of many types of skills. Because human capital should be produced as efficiently as possible, we should not encourage more expensive methods of imparting human capital through policies that emphasize formal training. Policies should not ignore the role of informal training.

For instance, suppose that you were concerned about the dismal level of training among academic economists. You mandate that universities and colleges spend 5 percent of the wage bill of their economics departments for training of economists. We predict you would soon find that there would be large training conferences in Honolulu in the middle of winter. Among the expensive flights, the expensive hotels, luaus, and mai tais, some useful training will occur, but this "training" is clearly less efficient than the economists going into their offices on a Saturday and, informally, learning some additional economics. (If this scenario sounds fanciful, examine how medical doctors meet their continuing education requirements, or where other professions have their trade meetings.)

Inducing substitution from inexpensive to expensive methods of producing human capital, either through tax credits, price subsidies, or mandates, is nobody's prescription for fixing our human capital system. Yet, this is precisely what programs that seek to encourage formal training programs would accomplish. While some of the programs may induce additional accumulation of human capital, there seems to be little doubt that much of the increase in formal training programs would simply be replacement for existing informal training. Thus, any policy initiative to encourage expenditures on training will probably succeed: it will increase *expenditures* on training. What should concern policy makers is whether or not the training programs succeed in raising the *wages* of the target groups and whether that increase represents an acceptable rate of return on investment.

One reason often given for the policy makers' preference for formal training programs is that informal training is difficult to measure.

While it is undoubtedly easier for firms to document expenditures on formal training than provision of informal training, it is not at all clear that the returns to the worker are higher for formal training than for informal training. Further, there is no guarantee that workers would even perceive such investments in formal training. When we asked identical questions to employers and employees within a month of the beginning of employment about *formal* training programs, we received significantly different responses about the quantity and incidence of training.

In our view, private sector training provides little insight into the design of a government training program to stimulate investment in human capital. For the most part, training experiences that we examined are the result of the interaction of profit-maximizing firms and utility-maximizing workers who jointly agree to an investment in training. Policy makers are interested presumably in stimulating investment in human capital beyond what the market is providing. To us, this suggests that policy makers would learn more about the efficacy of their plans by examining past training programs governments have initiated. A voluminous literature exists on government training programs, many implemented in partnership with the private sector. Available estimates indicate that the investment necessary to reverse the decline in wages of high school dropouts and those with only a high school degree will be immense. Heckman, Roselius, and Smith (1994) estimate that if training investments will provide a 10 percent rate of return, it would have required over $212 billion (in 1989 dollars) to return 1989 high school graduates to 1979 wage levels, and it would have required another $214 billion to return 1989 male high school dropouts to 1979 wage levels. While there are some notable successes in these programs, most of this literature indicates that the cost estimates of Heckman, Roselius, and Smith are quite conservative.

What advice, if any, can we provide to policy makers who wish to encourage the provision of on-the-job training in the private sector? First, given scarce resources, it seems obvious to us that any program should be targeted. While it may be true that well-paid lawyers, software engineers, accountants, and economists for that matter, may also suffer from the under-provision of on-the-job training as well as disadvantaged youth, it seems perverse to expend public resources to correct this distortion. As a society, we may wish to use resources to aid disadvantaged youth, single mothers, poor families, and perhaps dislocated workers, but we probably do not want subsidies to the highly paid.

Second, government should avoid specifying how firms choose to train these targeted workers. Firms know best how to provide training to their workforce; government should not attempt to micro manage the employment relationship. How then should we guarantee that the targeted population receives training? Our answer is simple. If employed, workers receive training; if not employed, these individuals will not accumulate human capital. Evidence from the Upjohn Institute data is unambiguous: While workers and firms may disagree on the type of training and the quantity of the component parts of training, both firms and workers agree that there is much training in the initial period of employment. Thus, if you want to raise the earnings of the economically disadvantaged, you should pursue policies that insure they are employed. Policies to encourage the labor supply of the disadvantaged—such as the earned income tax credit—or policies to increase the demand for disadvantage workers—such as explicit wage subsidies or tax credits for hiring the disadvantaged—would seem the most obvious, direct route of insuring their employment.

Finally, we note that for policy purposes the measurement of training is largely irrelevant. While it is true that the human capital accumulated through on-the-job training is difficult to measure, it is also largely irrelevant if we are concerned with increasing the earnings of the poor. While not measured with complete accuracy, wages are easier to measure than on-the-job training.

NOTE

1. Katz and Murphy (1992, table 1, p. 40). Percentages are approximated from the log of average real weekly wage.

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