



Specific Capital, Mobility, and Wages: Wages Rise with Job Seniority

Robert Topel

The Journal of Political Economy, Volume 99, Issue 1 (Feb., 1991), 145-176.

Your use of the JSTOR database indicates your acceptance of JSTOR's Terms and Conditions of Use. A copy of JSTOR's Terms and Conditions of Use is available at <http://www.jstor.org/about/terms.html>, by contacting JSTOR at jstor-info@umich.edu, or by calling JSTOR at (888)388-3574, (734)998-9101 or (FAX) (734)998-9113. No part of a JSTOR transmission may be copied, downloaded, stored, further transmitted, transferred, distributed, altered, or otherwise used, in any form or by any means, except: (1) one stored electronic and one paper copy of any article solely for your personal, non-commercial use, or (2) with prior written permission of JSTOR and the publisher of the article or other text.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

The Journal of Political Economy is published by University of Chicago. Please contact the publisher for further permissions regarding the use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/ucpress.html>.

The Journal of Political Economy
©1991 University of Chicago

JSTOR and the JSTOR logo are trademarks of JSTOR, and are Registered in the U.S. Patent and Trademark Office. For more information on JSTOR contact jstor-info@umich.edu.

©2001 JSTOR

Specific Capital, Mobility, and Wages: Wages Rise with Job Seniority

Robert Topel

University of Chicago and National Bureau of Economic Research

The idea that wages rise relative to alternatives as job seniority accumulates is the foundation of the theory of specific human capital, as well as other widely accepted theories of compensation. The fact that persons with longer job tenures typically earn higher wages tends to support these views, yet this evidence ignores the decisions that have brought individuals to the combination of wages, job tenure, and experience that are observed in survey data. Allowing for sources of bias generated by these decisions, this paper uses longitudinal data to estimate a lower bound on the average return to job seniority among adult men. I find that 10 years of current job seniority raise the wage of the typical male worker in the United States by over 25 percent. This is an estimate of what the typical worker would lose if his job were to end exogenously. Overall, the evidence implies that accumulation of specific capital is an important ingredient of the typical employment relationship and of life cycle earnings and productivity as well. Continuation of these relationships has substantial specific value for workers.

I. Introduction

The idea that wages rise relative to alternatives over the duration of a job is the foundation for several important theories of productivity

Financial support from the National Science Foundation and the William Ladany Research Fund at the University of Chicago is gratefully acknowledged. I have benefited from the comments of Kevin M. Murphy, Joe Altonji, George Borjas, Charles Brown, David Card, Henry Farber, Richard Freeman, Larry Katz, Sherwin Rosen, Nachum Sicherman, and an anonymous referee. I also thank workshop participants at Arizona State, California at Los Angeles, Chicago, Columbia, Harvard, Illinois, Ohio State, Princeton, Santa Barbara, Stony Brook, Texas A&M, and the Center for Naval Analyses. Will Carrington provided able assistance. I am responsible for any errors.

[*Journal of Political Economy*, 1991, vol 99, no. 1]

© 1991 by The University of Chicago. All rights reserved. 0022-3808/91/9901-0008\$01.50

and compensation. Most prominently, a key prediction of Becker's (1964) model of investment in specific human capital is that wages rise with job tenure (seniority), leaving workers with a stake in the specific value of the employment relationship. Related theories of agency in durable employment relations (Becker and Stigler 1974; Lazear 1981) also generate deferred compensation that encourages workers' effort and improves performance (see also Lazear and Rosen 1981; Rosen 1986). Deferred compensation in the form of rising wages can also induce profitable self-selection of heterogeneous workers that enhances productivity (Salop and Salop 1976). Other contracting models (Freeman 1977; Harris and Holmstrom 1982) produce qualitatively similar predictions for the shape of job-specific wage profiles. These ideas are sufficiently established that the assumption of rising wage profiles has become an accepted point of departure for subsequent work (e.g., Shleifer and Summers 1988).

In all these models, a major component of earning capacity is both unique to a particular employment relationship and increasing in importance as the relationship ages. Senior workers would suffer substantial wage losses if their jobs were to end. Thus a common theme is specialization and, from a worker's perspective, the *accumulation* of job-specific capital. The credibility of this view is enhanced by the common finding from survey data that workers with longer job tenures typically earn more. This has been interpreted to mean that seniority raises earnings for the typical worker and, by related evidence, that turnover rates (quits and layoffs) are strongly and negatively related to job tenure.¹ These relationships are the empirical foundation for the view that specific capital is an important ingredient of life cycle earnings and productivity in modern labor markets.

This interpretation of the evidence is open to criticism because it ignores the job-changing decisions that have brought workers to the combinations of wages, job tenure, and market experience that are observed in survey data. These decisions can affect the relationship of job tenure to wages in two ways. First, recent evidence indicates that many job-changing decisions are the outcome of a career process by which workers are sorted into more durable and productive jobs (Hall 1982; Topel and Ward, in press). High-wage jobs tend to survive, which can mean that persons with long job tenures earn higher wages. The second possibility is that more productive or able persons change jobs less often, for which there is also empirical support. Again, persons with long job tenures will earn high wages. In either case, the wage earned by the representative worker need not *rise* as

¹ Mincer and Jovanovic (1981) provide evidence on both points. Others include Borjas and Rosen (1980) and Mincer (1986, 1988).

tenure accumulates, yet in a cross section of workers those with greater tenure earn more because tenure is correlated with unobserved characteristics of workers or their jobs. Recent empirical research tends to support this view: adjusting for unobserved factors in various ways, at least four recent studies have concluded that the true returns to job-specific experience are minor (Topel 1986; Abraham and Farber 1987; Altonji and Shakotko 1987; Marshall and Zarkin 1987).² This reinterpretation of the evidence has found widespread acceptance in subsequent literature (Mortensen 1988*a*, 1988*b*; Rosen 1988).

This conclusion has important implications for the way that economists view labor markets. In Becker's (1964) terminology, it means that human capital investments are mainly general rather than firm specific, so that the main component of workers' embodied skills is portable among firms. Thus investment in human capital does not account for the prevalence of "lifetime jobs" in the United States and other labor markets (Hall 1982) or for the sharply lower turnover rates of more senior workers. Further, in the absence of specific capital, the costs of worker displacement and unemployment are likely to be small: even for relatively senior workers these events should not have important effects on workers' wealth because previously accumulated skills are portable. Finally, the independence of wages and job tenure undermines the entire compensation literature that treats the timing of wages as a strategic device for affecting worker productivity. Either the problems of moral hazard and asymmetric information that underlie this literature are unimportant or they are solved by other means.

As the title suggests, this paper provides strong evidence that wages do rise with job seniority. I analyze longitudinal data on earnings and job histories for 1,540 men drawn from the first 16 waves (1968–83) of the Panel Study of Income Dynamics (PSID). The main finding is that the average returns to seniority are substantial. The estimates imply that 10 years of job seniority raise the wage of the typical male worker in the United States by over 25 percent relative to what he could earn elsewhere. Both theory and related evidence imply that this estimate is a lower bound; the true returns are probably larger. This estimate does not vary across broad occupational categories; professionals and nonunion blue-collar workers receive roughly similar returns, though the presumed rationing of union jobs alters this conclusion for workers covered by collective bargaining. For them, a job displacement that forces a move to the nonunion sector would

² Mincer (1988) and Brown (1989) provide direct evidence that job training enhances wage growth.

reduce earnings of a worker with 10 years of seniority by nearly 40 percent. This effect is much larger than traditional estimates of union wage premiums (Lewis 1986) because it reflects the full cost of leaving the union sector, including forgone specific capital, and may account for the much greater average durations of union jobs.

All this evidence is based on a two-stage estimation procedure. The basic idea is that within-job wage growth combines the returns to general and job-specific experience. Thus the first stage estimates the determinants of wage growth but is unable to distinguish separate returns to general market experience and job-specific seniority. The second stage is a cross-sectional comparison of the wages of workers who started new jobs at different points in their careers. This stage yields an upper bound on the returns to general experience alone. In combination with estimates from the first stage, this translates to a lower bound on the returns to seniority in the typical employment relationship. In all cases that I have examined, the estimated returns to seniority are substantial. Along the way, additional sources of bias are examined and are found to have very minor effects on the results.

The paper is organized as follows. Section II provides preliminary evidence of important effects of job seniority on wages, based on the observed wage changes of workers who were displaced from their former jobs. Workers with longer prior job tenures suffer substantially greater losses from displacement, as would occur if wages rise with the duration of employment. The basic econometric framework is then developed and potential sources of bias in estimating the returns to job seniority and experience are explicitly modeled. Section III describes the PSID data and methods of selecting the sample. The main empirical results follow in Section IV.

Because these results and conclusions are substantially different from those reported in important recent studies, Section V of the paper compares my procedures and findings with those of Abraham and Farber (1987) and Altonji and Shakotko (1987), who also analyzed the PSID data. I find that earlier efforts understate within-job wage growth because of both significant problems of measurement error and methodological biases. Section VI concludes the paper.

II. Modeling the Returns to Experience and Job Tenure

A. Some Preliminary Evidence

Do wages depend on job seniority? The methods I develop below rely on panel data that follow the evolution of wages within jobs, but

TABLE 1
WAGE CHANGES OF DISPLACED WORKERS BY YEARS OF PRIOR JOB SENIORITY,
JANUARY CPS, 1984 AND 1986

	YEARS OF SENIORITY ON PRIOR JOB				TOTAL
	0-5	6-10	11-20	21+	
Average change in log weekly wage	-.095 (.010)	-.223 (.021)	-.282 (.026)	-.439 (.071)	-.135 (.009)
Percentage displaced by plant closing	.352 (.008)	.463 (.021)	.528 (.026)	.750 (.043)	.390 (.007)
Weeks unemployed since displacement	18.69 (.413)	24.54 (1.202)	26.66 (1.536)	31.79 (3.288)	20.41 (.385)

NOTE.—Estimates refer to male respondents between the ages of 20 and 60. Sample size is 4,367. Nominal data are deflated by the GNP price deflator for consumption expenditure. Figures in parentheses are standard errors.

suggestive evidence is provided by tabulating the wage changes of workers whose jobs end exogenously. If job tenure raises wages relative to alternatives, then more senior workers will suffer larger wage reductions when employment is terminated. The estimates in table 1 are based on the Displaced Workers Survey that was administered with the January Current Population Survey (CPS) in 1984 and 1986. The sample consists of 4,367 men who report that they have been displaced from a job for economic reasons (layoffs or plant closings) in the past 5 years and who are currently employed. The table reports the mean change in log weekly earnings for these workers, as well as the average number of weeks unemployed since displacement and the reason for termination. There is little doubt that displacement is costly: the average worker who has found new employment suffers a 14 percent reduction in earnings. More important for present purposes, this reduction in average earning capacity is strongly related to prior job tenure: those with longer jobs lose more, and they experience more unemployment after displacement. Thus the “costs” of displacement are strongly related to prior job tenure.

There are two possible explanations for this finding. One is that wages rise with seniority, so workers with longer job tenures are truly more specialized than their junior counterparts. The other is that long jobs paid higher wages throughout, and so tenure acts as a proxy for the relative “quality” of the terminated job. Distinguishing these hypotheses requires panel data on individuals’ job histories, as below. A third hypothesis—that workers with long former jobs are more able—is not supported by these estimates. This finding is consistent with evidence developed below, which shows that biases due to unobserved personal characteristics are a minor concern.

B. A Prototype Model

A prototype model of wage determination is

$$y_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + \epsilon_{ijt}, \quad (1)$$

where y_{ijt} denotes the (log) wage for individual i on job j at time t , X_{ijt} is total labor market experience, and T_{ijt} is current job tenure (seniority). Parameters β_1 and β_2 represent average returns to an additional year of either experience or tenure, respectively, and are the parameters of interest for the remainder of the paper. Other observables that may enter (1) are ignored for ease of exposition. No generality is lost by also ignoring higher-order terms in X and T ; they will be introduced in the empirical analysis.

The most popular interpretation of (1) is that β_1 represents the return on general human capital (training and the like) that accumulates with experience, while β_2 represents the return on accumulated job-specific capital that would be lost if a job were to end. Biases in estimating these returns are generated by covariance between the regressors and the unobservables, ϵ . In what follows my main concern will be with covariance that is the outcome of optimizing behavior, as workers seek to locate and maintain a productive (high-wage) employment relationship. Thus one can decompose the unobservables as

$$\epsilon_{ijt} = \phi_{ijt} + \mu_i + \nu_{ijt}, \quad (2)$$

where ϕ_{ijt} represents the stochastic component of wages that may be specific to a worker-firm pair, and μ_i is a person-specific effect that accounts for unobserved differences in earning capacity across individuals (e.g., "ability"). The ν_{ijt} account for marketwide random shocks as well as measurement error that is known to plague survey data. I assume that the components of (2) are mutually orthogonal and (for now) that μ_i and ν_{ijt} are orthogonal to the regressors in (1). Notice that fixed "job effects" ($\phi_{ijt} = \phi_{ij}$) are a special case of (2) in which the specific value of a job does not evolve over time. This component captures the notion of a "good match" in the sense of wages that are higher than what a worker could obtain elsewhere. It will generate bias in estimating (1) if ϕ is correlated with experience or job tenure. Thus let the auxiliary regression of ϕ on the observables be

$$\phi_{ijt} = X_{ijt}b_1 + T_{ijt}b_2 + u_{ijt}. \quad (3)$$

In light of (3), least squares applied to (1) will yield biased estimates of β_1 and β_2 since $E\hat{\beta}_1 = \beta_1 + b_1$ and $E\hat{\beta}_2 = \beta_2 + b_2$. In much of what follows, I seek evidence on the importance of the parameters b_1 and b_2 .

Theory offers some guidance on the signs of these effects. In light of job matching or search theories of job mobility (e.g., Burdett 1978; Jovanovic 1979a, 1979b, 1984), it is plausible that a productive (high-wage) match, once found, is unlikely to end. Given this, it is tempting to argue that job quality, ϕ , and tenure are positively related ($b_2 > 0$) in survey data.³ This argument ignores the fact that persons who change jobs gain, on average, from their move, and they are included in the data at low job tenures.⁴ In fact, the basic theory of search and matching implies that $b_2 < 0$ —a comparison of wages for workers with different job tenures will *understate* the returns to seniority—as the following argument demonstrates.

Consider identical individuals who sample new job offers from a stable offer distribution $G(y)$. Offers arrive randomly at an exogenous rate. If the true values of β_1 and β_2 are zero, an optimal job-changing policy is to accept any offer that exceeds the wage on the current job. Thus high-wage jobs survive because they are less likely to be dominated by an alternative offer. Under these conditions, the current wage of any individual is the maximum offer received since entering the market. The expected value of this maximum clearly rises with experience since the number of offers sampled increases. Thus $b_1 > 0$. In contrast, current job tenure indicates only the order in which the maximum offer was received: persons with high tenure received their best offer earlier. But the distribution of the maximum offer (the first order statistic) depends only on the number of offers (experience) and not on their order (tenure).⁵ This means that $E(y|X, T) = E(y|X)$ and there is no sample selection on tenure ($b_2 = 0$).⁶ In this case experience is a sufficient statistic for the distribution of wages and there is no bias in estimating β_2 . But things are different if $\beta_2 > 0$.

If $\beta_2 > 0$, acceptable new job offers must compensate workers for the forgone returns to tenure on the current job, so there is less mobility than when $\beta_2 = 0$. Since a regression compares conditional means, the issue is whether $E(y|X, T + 1) = E(y|X, T) + \beta_2$. That is, do persons with one extra year of job tenure earn a wage that is

³ Most matching models generate wage dispersion from the assumption that individuals' productivities vary among tasks. A contrasting "segmented markets" view is that wage differentials merely reflect the existence of "good" and "bad" jobs, for unspecified reasons (e.g., Doeringer and Piore 1971). Which of these is true does not affect the following analysis.

⁴ Lang (1987) makes a related point.

⁵ For n offers received, the density of the maximum offer is $f(y) = G(y)^{n-1}g(y)$, which depends only on n . With random (Poisson) arrival of offers, the expected number of offers is proportional to time in the market (experience).

⁶ Formally, the absence of a tenure effect in this case requires that experience effects be represented by a sequence of dummy variables for each level of experience.

higher, on average, by β_2 ? With $\beta_2 > 0$, some otherwise acceptable offers have been rejected. Inclusion of these marginal workers reduces the average wage of “stayers.” Further, persons who change jobs require higher average wage offers to induce their move. This raises the average wage of movers. Both of these selection effects imply $E(y|X, T + 1) - E(y|X, T) < \beta_2$, so *least squares applied to (1) must underestimate the return to seniority*.⁷ Thus a basic matching technology with rising within-job wage profiles implies $b_1 > 0$ but $b_2 < 0$. Yet the notion that “good jobs survive” still holds. Let X_0 denote experience at the start of a job, so $X = X_0 + T$. Then (3) is equivalent to $\phi = X_0 b_1 + T(b_1 + b_2)$. Thus the durability of high-wage jobs means that $b_1 + b_2 > 0$,⁸ so the *sum* of the returns to experience and tenure will be biased up in a wage regression on survey data.

In light of this analysis, there can be no presumption that standard regression techniques applied to cross-sectional data will overestimate the returns to tenure. Optimizing search behavior generally implies the opposite, though there are other sources of selection that may reinforce or offset these effects. For example, mobility costs tend to reinforce $b_2 < 0$ by reducing the set of acceptable wage offers, while costly search may cause $b_2 > 0$ because only persons with relatively poor employment matches are actively searching. Panel data on individuals' careers provide leverage for isolating these effects. I turn to this subject next.

III. A Two-Stage Estimation Procedure

Panel data from sources such as the PSID provide information on wages at different stages of a single job, as well as on different jobs for a single individual. Given this, within-job wage growth can be studied from the first differences of (1) for persons who do not change jobs, which eliminates fixed job and individual effects:

$$y_{ijt} - y_{ijt-1} = \beta_1 + \beta_2 + \epsilon_{ijt} - \epsilon_{ijt-1} \quad (4)$$

since $\Delta X = \Delta T = 1$ between periods of a single job. If $\epsilon_{ijt} - \epsilon_{ijt-1}$ has mean zero, then least squares applied to (4) will yield a consistent

⁷ The selection is easiest to see in a two-period case. If workers receive one offer per period, then exchangeability implies that $E(y_2|y_2 > y_1) = E(y_1|y_1 > y_2)$. If wages grow by β among stayers, then $E(y_1|y_1 + \beta > y_2) < E(y_2|y_1 + \beta < y_2)$. Thus, $E(y_1 + \beta|y_1 + \beta > y_2) - E(y_2|y_2 > y_1 + \beta) < \beta$. Other factors may increase or decrease the bias. For example, mobility costs strengthen the bias.

⁸ Topel and Ward (in press) find direct evidence for both $b_1 > 0$ and $b_1 + b_2 > 0$. They estimate that approximately one-third of wage growth during the first 10 years in the labor market is due to job-changing activity. Controlling directly for unobserved personal heterogeneity, they also find that wage increases and transitions to higher-paying jobs sharply reduce job mobility because acceptable offers must be better to induce a move. Endogenous search intensity weakens the bias because persons with high wages are less likely to sample.

estimate of average within-job wage growth, $\beta_1 + \beta_2$.⁹ Given (4), an estimate of β_1 can be obtained from initial wages on new jobs:

$$y_{0jt} = X_{0jt}\beta_1 + \phi_{ij} + \mu_i + v_{ijt}, \tag{5}$$

where X_0 is initial experience on the job. The error term in (5) is nonrandom because only *acceptable* new job offers are observed. For example, ϕ and X_0 are positively correlated if expected match quality rises with time in the market. One approach to this problem is to explicitly model the mobility decisions that underlie this selection bias, in which case standard sample selection corrections (e.g., Heckman 1976) might be applied. With this strategy, identification relies crucially on distributional assumptions (wage offers must be normally distributed), as well as on other strong restrictions (Topel 1986).

A more robust alternative is simply to note the selection bias implicit in (5) and to treat $(\beta_1 + \beta_2) - \hat{\beta}_1$ as an estimate of the return to seniority. If $\hat{\beta}_1$ is biased up, this two-step procedure yields a lower bound on the return to seniority. More generally, since $X \equiv X_0 + T$, model (1) may be rewritten as

$$y = X_0\beta_1 + TB + \epsilon, \tag{6}$$

where $B \equiv \beta_1 + \beta_2$. With (6), a two-step model is given by the first differences of within-job wage growth (4) and

$$y - T\hat{B} = X_0\beta_1 + e, \tag{7}$$

where $\hat{B} \equiv \widehat{\beta_1 + \beta_2}$ is the consistent first-step estimator of the sum of the returns to experience and tenure, derived from (4), and $e = \epsilon + T(B - \hat{B})$. As a second-step model, equation (7) is preferable to (5) because it makes use of data from all periods of all jobs.

The two-step model given by (4) and (7) yields unbiased estimators of β_1 and β_2 only if $EX_0'\epsilon = 0$. This condition will not hold if job matching is important. Nevertheless, we may calculate the expected values of the two-step estimators of β_1 and β_2 up to the unknown parameters b_1 and b_2 . When least squares is applied to (4) and (7), some algebra establishes that these are

$$E\hat{\beta}_1 = \beta_1 + b_1 + \gamma_{X_0T}(b_1 + b_2) \tag{8a}$$

and

$$E\hat{\beta}_2 = \beta_2 - b_1 - \gamma_{X_0T}(b_1 + b_2), \tag{8b}$$

where γ_{X_0T} is the least-squares coefficient from a regression of tenure on initial experience, X_0 .

⁹ This is assumed for now, though mobility decisions may also generate selection in (5), because only acceptable values of $\Delta\epsilon$ are observed. This point is examined in Sec. IVD.

Equations (8a) and (8b) indicate that the two-step procedure yields biased estimators of the returns to market experience and job tenure. The biases are equivalent, but of opposite signs, because the sum $\beta_1 + \beta_2$ is consistently estimated from the first-step model. If systematic job changing is important ($b_1 > 0$, $b_1 + b_2 > 0$), productive employment relationships are located later in the typical worker's career. Then (8a) implies that $E\hat{\beta}_1 > \beta_1$ —the estimated returns to experience are biased up because they include the return to changing jobs—while (8b) implies $E\hat{\beta}_2 < \beta_2$ for the same reason. Thus the two-step model establishes a lower bound on the average return to seniority. Notice in particular the difference between the bias in (8b) and the least-squares bias, b_2 , in estimating β_2 . Though earlier discussion indicates that the sign of b_2 is not well established by theory, the bias in (8b) is negative as long as “better” jobs are located as time in the labor market accumulates. Virtually any model of optimal job changing has this property.

There are two possible caveats to this conclusion. First, if job changing is the outcome of optimizing behavior for workers, then jobs offering low wage *growth* may not survive. Since equation (4) applies to an employment relationship that survived from date $t - 1$ to t , average wage growth in this sample of “stayers” may exceed the rate of growth in the population of all jobs. In this case β_2 could be overestimated. Second, persons who change jobs frequently may be less productive, on average, than persons in stable employment relations. Then X_0 is lower for able persons—they started their current jobs earlier—and so $\hat{\beta}_1$ is biased down. Again, β_2 could be overestimated. I provide evidence on both of these effects below.

IV. Estimation

A. The Data

The procedure described above is applied to panel data from the first 16 (1968–83) waves of the PSID.¹⁰ Complete sample selection criteria are reported in the Appendix. For the estimates that follow, attention is restricted to white males between the ages of 18 and 60 (inclusive) who were not self-employed, employed in agriculture, or employed by the government. All individuals are from the random, nonpoverty sample of the PSID. The data analyzed here consist of 13,128 job-years on 1,540 individuals and 3,228 jobs. Summary statistics and definitions for all variables used in estimation are reported in Appendix table A1.

¹⁰ The data were kindly supplied by Joe Altonji and Nachum Sicherman.

The wage data refer to (log) average hourly earnings in calendar years 1967–82. Since the parameters in (1) refer to relative earnings differences at a point in time, the usual regression strategy is to control for aggregate real wage growth and inflation by including year-specific intercepts. There are two problems with this in the current context. First, cross sections of the PSID data may not be representative of the underlying population at each point in time. The records available for analysis include households that participated in the survey at the last survey date (here 1983). Households that left the survey before 1983 are not in the data. Thus even if the 1983 sample is representative, past cross sections based on these households will reflect the sample selection rule that causes households to remain in the PSID. Second, in following any fixed population in panel data, time is not statistically exogenous for the same reason that experience is not: average match quality rises with time in the market. In this situation, treating time effects as exogenous may lead to an understatement of the return to seniority and an overstatement of temporal wage growth.¹¹ To avoid these problems, I deflated the wage data by a wage index for white males calculated from the annual demographic (March) files of the CPS (see Murphy and Welch 1987). This index nets out both real aggregate wage growth and changes in any aggregate price level (the gross national product price deflator for consumption was used), so that wage data from different time periods of the panel are expressed in comparable units. Values for the wage index are reported in the Appendix.

A key step in the analysis was the construction of a consistent measure of current job tenure. Because of a number of sources of measurement error, reported tenure in the PSID data is not reliable. It is often recorded in intervals of several years, and recorded values are often inconsistent between successive years of the same job. This measurement error is magnified in the first-step model, which uses changes in seniority between successive years to estimate parameters of wage growth. The problem is acute when higher-order terms (T^2 , T^3) are added to the model. To correct for these problems, the measure of job tenure used here relies on the fact that tenure must rise by 1 year in each year of a job: For jobs that start within the panel, tenure is started at zero and incremented by one each year. For jobs

¹¹ In fact, average real wages rise slightly more rapidly in the PSID than in random samples of the CPS. A vector of year dummies was included in a standard log wage regression as specified below. The year effects indicated that PSID wages grew by about 7.0 percent relative to CPS cross sections during the 1970s. When the same regression was applied to the subsample of individuals who entered the data in 1968–69 ($n = 6,929$), the time effects were eliminated. Thus there is no evidence that wages of individual PSID cohorts grew more rapidly than the population.

that were in progress at the beginning of an individual's record, current tenure is measured relative to the maximum reported during the job, again imposing the restriction that tenure change by one each year. Within jobs, the resulting sequence of measured job tenures is perfectly correlated with labor market experience, as required.¹²

B. Wage Growth within Jobs

If the evolution of wages within jobs follows a random walk, then the residuals of the wage growth model are serially independent and least squares applied to (4) is an efficient estimator of $\beta_1 + \beta_2$. As in Topel and Ward (in press), close examination of the time-series properties of within-job wage changes yields two important conclusions. First, there is no evidence of positive serial correlation in within-job wage innovations, $\epsilon_{ijt} - \epsilon_{ijt-1}$. This is a strong finding since one might expect that some types of jobs offer steeper wage profiles than others. The lack of serial correlation implies that heterogeneity in permanent rates of wage growth among jobs is empirically unimportant. Second, I find that the within-job evolution of the wage has a strong permanent component that closely approximates a random walk, so the residuals satisfy

$$\phi_{ijt} = \phi_{ijt-1} + \eta_{ijt}, \quad (9)$$

where η_{ijt} is serially independent with mean zero.¹³ Because of (9), values of η_{ijt} reflect "permanent" changes in a worker's expected lifetime wealth. For example, these may reflect uncertain returns on investments in human capital or simply new information about a worker's productivity. If these changes are firm-specific rents, they will affect future job-changing decisions. In contrast, if they mainly represent changes in general human capital, then future job mobility will be unaffected by them. These possibilities have different implications for interpreting the estimated returns to seniority; I will return to this subject below.

Given these findings, table 2 reports various specifications for first-stage models of within-job wage growth. Generalizing the earlier discussion, these and all subsequent models allow for higher-order effects of experience and tenure on wages (e.g., X^2), the effects of which are identified from within-job wage changes (e.g., $\Delta X^2 = 2X - 1$). As above, the model is underidentified by one parameter because

¹² There are many cases of ambiguity about job endings. For example, reported tenure within a job may fall to zero and then rise smoothly. These cases suggest unrecorded changes of employer. These "jobs" were deleted, but doing so had no material effect on the results. See the Appendix for details.

¹³ Detailed evidence on these points appears in Topel (1990).

TABLE 2

MODELS OF ANNUAL WITHIN-JOB WAGE GROWTH, PSID WHITE MALES, 1968-83
(Dependent Variable Is Change in Log Real Wage; Mean = .026)

	MODEL		
	(1)	(2)	(3)
Δ Tenure	.1242 (.0161)	.1265 (.0162)	.1258 (.0162)
Δ Tenure ² ($\times 10^2$)	...	-.0518 (.0178)	-.4592 (.1080)
Δ Tenure ³ ($\times 10^3$)1846 (.0526)
Δ Tenure ⁴ ($\times 10^4$)	-.0245 (.0079)
Δ Experience ² ($\times 10^2$)	-.6051 (.1430)	-.6144 (.1430)	-.4067 (.1546)
Δ Experience ³ ($\times 10^3$)	.1460 (.0482)	.1620 (.0485)	.0989 (.0517)
Δ Experience ⁴ ($\times 10^4$)	.0131 (.0054)	.0151 (.0055)	.0089 (.0058)
R^2	.022	.023	.025
Standard error	.218	.218	.218

PREDICTED WITHIN-JOB WAGE GROWTH BY YEARS OF JOB TENURE
(Workers with 10 Years of Labor Market Experience)

	TENURE									
	1	2	3	4	5	6	7	8	9	10
Predicted wage growth (%)	.068	.060	.052	.046	.041	.037	.033	.030	.028	.026

NOTE.—Estimates are based on within-job first differences of log average hourly earnings. Standard errors are in parentheses. Number of observations is 8,683

linear terms in experience and tenure are perfectly correlated within jobs. Thus the first coefficient in column 1 (.1242) is an estimate of $\beta_1 + \beta_2$ for new entrants to the labor force ($X = 0$). Wage profiles are concave in both experience and tenure, though the usual quadratic specification is insufficient to describe the data: after an initial period of rapid growth, wage profiles flatten out. This is captured in column 3 by quartics in both experience and tenure.¹⁴ To illustrate the impact of job tenure on wage changes within a job, at the bottom of the table I report predicted wage growth for a worker with 10 years of market experience and varying job tenures. Wage growth clearly declines as tenure accumulates, with experience held constant, a pattern that is difficult to reconcile with the idea that tenure has a negligible effect on wage levels.

¹⁴ As Welch (1979) points out, the usual quadratic underestimates wage growth for young workers. Murphy and Welch (1987) also advocate a quartic specification for the experience profile.

TABLE 3

SECOND-STEP ESTIMATED MAIN EFFECTS OF EXPERIENCE (β_1) AND TENURE (β_2) ON LOG REAL WAGES, AND LEAST-SQUARES BIAS IN WAGE GROWTH ($b_1 + b_2$)

	Experience Effect, β_1 (1)	Within-Job Wage Growth, $\beta_1 + \beta_2$ (2)	Tenure Effect, β_2 (3)	Wage Growth Bias, $b_1 + b_2$ (4)
Main effect	.0713 (.0181)	.1258 (.0161)	.0545 (.0079)	.0020 (.0004)
ESTIMATED CUMULATIVE RETURN TO JOB TENURE				
	5 Years	10 Years	15 Years	20 Years
Two-step model	.1793 (.0235)	.2459 (.0341)	.2832 (.0411)	.3375 (.0438)
OLS	.2313 (.0098)	.3002 (.0105)	.3203 (.0110)	.3563 (.0116)

NOTE.—Estimated within-job wage growth ($\beta_1 + \beta_2$) from table 2, col. 3. Dependent variable for other estimates is log real hourly earnings less the effects of variables that are consistently estimated from the within-job wage growth model. Other regressors in the second-step model (10) include years of completed schooling, marital status, residence in an SMSA, current disability, union membership, and eight indicators for census region of residence. Estimated cumulative returns are based on the main effect of job tenure ($\beta_2 = .0545$) plus the effects of higher-order terms in tenure shown in col. 3 of table 2. Standard errors (in parentheses) are corrected to reflect sampling error in the first-step estimates. Methods developed in Murphy and Topel (1985) are used for this. Number of observations is 10,685.

The results that follow are based on the model in column 3 of table 2.

C. *Estimated Returns to Market Experience and Job Tenure*

The main results for the separate returns to experience and tenure are reported in table 3. In implementation of the second-step model that underlies these results, consistent estimates of $\beta_1 + \beta_2$ and the parameters of higher-order terms in experience and tenure are taken from the within-job growth model in column 3 of table 2. Denote these terms by $\chi\hat{\Gamma}$. Subtracting $\chi\hat{\Gamma}$ from both sides of the wage equation and letting \mathbf{F} denote the vector of other factors (education etc.) that affect wages yield the second-step model

$$y - \chi\hat{\Gamma} = X_0\beta_1 + \mathbf{F}\gamma + e, \quad (10)$$

which is in the form of equation (7). As shown in column 1 of the table, the estimated value of β_1 from implementing (10) is about 7 percent. This estimator is substantially smaller than the value of $\beta_1 + \beta_2$ estimated from within-job growth, which is reproduced in column 2. The remainder is the main effect of job tenure on wages, β_1 . I estimate that in the first year of the typical new job, the real wage

rises by over 5 percent ($\beta_2 = .0545$) because of the accumulation of job-specific experience alone.

Cumulative returns to various lengths of job tenure are reported in the bottom panel of the table. The estimates are based on the main effect of $\beta_2 = .0545$, together with the concavity of the wage profile implied by the effects of higher-order terms in table 2. The returns to seniority are large: I estimate that 10 years of job seniority increase the wage of the typical worker by 28 percent ($e^{.2459} - 1$), relative to alternatives. For comparison, I also report estimates of the wage profile generated by ordinary least squares (OLS) applied to (1). These effects are larger, though not dramatically so. Since I have argued that the two-step procedure generates a lower bound on the true returns, the OLS estimates may actually be close to the truth.

Are these results reasonable? An appropriate interpretation of the estimated returns to seniority in table 3 is that they represent the reduction in earning capacity that would be suffered by a person whose job ends for exogenous reasons. Accordingly, these results imply that a person with 15 years of current job tenure would suffer an immediate 33 percent ($e^{.2832} - 1$) wage loss if his job ended exogenously. This is the experiment underlying the estimated losses of displaced workers in table 1, which showed an average wage reduction for workers in this tenure category of about 32 percent. Despite obvious differences in the composition of the two samples, the similarity of these estimates is gratifying. The results here also indicate that workers may bounce back from these losses fairly rapidly: relative wage growth is most rapid at the beginning of new jobs, so initial wage losses would vastly overstate changes in lifetime wealth caused by a job termination.

A final point about these estimates is noteworthy. Though the two-step procedure cannot identify the bias terms b_1 and b_2 separately, their sum is clearly identified since $\beta_1 + \beta_2$ is consistently estimated. In fact, $b_1 + b_2$ is the component of wage *growth* that is caused by systematic job changing. And since $E\phi = X_0b_1 + T(b_1 + b_2)$, the notion that “good jobs survive” is equivalent to $b_1 + b_2 > 0$. This sum can be estimated directly by reinserting the term $T(b_1 + b_2)$ on the right side of equation (10) and applying least squares. The resulting estimate, shown in column 4 of table 3, is a wage growth bias of about 0.2 percent per year. Finally, from (8) the bias in the two-step estimators of β_1 and β_2 is

$$b_1 + \gamma_{X_0T}(b_1 + b_2). \quad (11)$$

A regression of current tenure on initial experience yields $\gamma_{X_0T} = -.25$, so the second term in (11) is $-.25 \times .0020 = -.0005$, or one-twentieth of one percentage point per year. This means that the bias

in the two-step estimator of β_2 , the return to job tenure, is virtually independent of any covariance of job tenure with the unobservables, that is, of the unsigned value of b_2 . Since $b_1 \geq 0$, the downward bias in the estimated return to seniority is solely due to improvement in match quality with total labor market experience.

D. Other Sources of Bias

Under the stated assumptions, the estimates in table 3 are a lower bound on the average returns to job seniority. Other sources of bias can weaken this conclusion, and at least two are worth investigating. One possibility is that the sample used to estimate $\beta_1 + \beta_2$ is weighted toward jobs with unusually high wage growth, which may affect the interpretation of the return to seniority. The second possibility is that more able or productive persons are also less mobile, so estimated returns reflect the longer average job durations of high-wage individuals. I treat these in turn.

Selection Bias in Wage Growth

The cumulative returns shown in table 3 are estimates of job-specific wage premiums that would be earned by a typical worker as he accumulates seniority. The most popular interpretation of these returns is that workers anticipate rising compensation over the life of a job, as in contract models such as Becker (1964), Salop and Salop (1976), or Lazear (1981). A second interpretation is also possible, however, since jobs that yield high wage growth may be more likely to survive. In this case returns to seniority are realized period by period, though they may not be anticipated at the start of a job.

To illustrate this point, rewrite the wage growth model (4):

$$y_{ijt} - y_{ijt-1} = \beta_1 + \beta_2 + \eta_{ijt} + v_{ijt} - v_{ijt-1}, \quad (4')$$

where η_{ijt} is the "permanent" increment to the wage defined in (9). For a job that ends between dates $t - 1$ and t , the wage y_{ijt} that *would* have been earned up to t is not recorded, but it may have been known by workers. If a substantial component of η_{ijt} is firm specific, then knowledge of η will affect mobility decisions, and so jobs with high values of $y_{ijt} - y_{ijt-1}$ will be more likely to survive. This means that average wage growth among workers who do not change jobs may overstate growth in the population, which is to say that $E\eta > 0$ in the sample. An estimate of $\beta_1 + \beta_2$ based on wage changes within jobs includes this selection effect, which would cause a corresponding overestimate of the *anticipated* returns to seniority, β_2 , by the preceding methods.

As a practical matter, both $\beta_2 > 0$ and $E\eta > 0$ imply that wages rise with seniority. Senior workers earn more, relative to alternatives, than they did when they started their jobs. This means that previous conclusions are not materially affected if $E\eta > 0$, but also that it is difficult to distinguish these alternatives in the data. The difference in the interpretation of the returns to seniority is of some theoretical interest, however. Some headway in distinguishing these explanations is possible by examining the relationship between current wage growth and subsequent job changing.

The key condition for $E\eta > 0$ to be quantitatively important is that a substantial component of η_{ijt} must be *firm specific*.¹⁵ Then a large value of η_{ijt} has a permanent effect on the value of a job, reducing mobility in period t and all subsequent periods.¹⁶ For example, since the wage follows a random walk, job-changing decisions in period $t + 1$ are based on $\eta_{ijt} + \eta_{ijt+1}$, and the expected value of η_{ijt} must be smaller for jobs that end in $t + 1$ than for those that survive to later periods. More generally, when η is firm specific, the expected value of η_{ijt} is increasing in R_{ijt} , the remaining life of the job measured from date t . Thus

$$0 \leq E(\eta_t | R_t \geq 0) < E(\eta_t | R_t \geq 1) < E(\eta_t | R_t \geq 2) < \dots \quad (12)$$

Evidence on the inequalities in (12) is presented in table 4. Panel A shows the relationship between the remaining life of a job and current wage growth, after observables are controlled for. The estimate in row 1 shows that there is no linear relationship between these variables. Row 2 is less restrictive, allowing separate effects for jobs that end in years $t + 1$, $t + 2$, and so on. Since the omitted category is jobs that survive 6 or more years, all these effects should be negative but decreasing in magnitude if (12) is satisfied. This pattern does not hold, though there is minor evidence that jobs that end in periods $t + 1$ and $t + 2$ have slightly lower growth.

Panel B of table 4 makes the test more stringent. If high-growth jobs tend to survive, then the inequalities (12) imply that estimates of the first-step model of wage growth based on more durable jobs will overstate $\beta_1 + \beta_2$, which will increase the estimated returns to job

¹⁵ The decision to change jobs depends on the job-specific component of an alternative offer, ϕ , relative to innovations to job-specific capital on the current job, η . Mobility occurs when $\phi - \eta$ is larger than some critical value, say k . If ϕ and η are independent normal random variables, the expected value of η in the sample of nonmovers is $E(\eta | \phi - \eta < k) = [\sigma_\eta^2 / (\sigma_\eta^2 + \sigma_\phi^2)^{1/2}] \times [f(k) / F(k)]$, where $F(k)$ is the standard normal distribution function. Thus the amount of selection depends on the relative magnitudes of σ_η^2 and σ_ϕ^2 .

¹⁶ Timing is also important. Even when η is firm specific, it may take time to locate an acceptable new job. Then η_{ijt} affects mobility only *after* period t , and estimates of $\beta_1 + \beta_2$ based on (4') are unbiased ($E\eta = 0$).

TABLE 4
EFFECTS OF SELECTION BIAS IN WAGE GROWTH ON THE ESTIMATED RETURNS
TO JOB SENIORITY

A. RELATIONSHIP BETWEEN REMAINING JOB DURATION AND CURRENT WAGE GROWTH						
REMAINING JOB DURATION (Years)	JOB ENDS IN PERIOD:					
	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$
1. .0006 (.0010)						
2.	-.012 (.012)	-.015 (.013)	.013 (.013)	.012 (.014)	.020 (.015)	.004 (.017)

B. RETURNS TO JOB SENIORITY BASED ON VARIOUS REMAINING JOB DURATIONS IN FIRST-STEP MODEL				
	REMAINING JOB DURATION IN ESTIMATING WAGE GROWTH (Years)			
	≥ 0	≥ 1	≥ 3	≥ 5
Main effects:				
Experience (β_1)	.0713 (.0181)	.0792 (.0204)	.0716 (.0245)	.0607 (.0292)
Tenure (β_2)	.0545 (.0079)	.0546 (.0089)	.0559 (.0109)	.0584 (.0132)
Estimated tenure profile:				
5 years	.1793 (.0235)	.1725 (.0265)	.1703 (.0319)	.1815 (.0379)
10 years	.2459 (.0341)	.2235 (.0376)	.2181 (.0437)	.2330 (.0514)
15 years	.2832 (.0411)	.2439 (.0445)	.2503 (.0504)	.2565 (.0594)
20 years	.3375 (.0438)	.2865 (.0469)	.3232 (.0531)	.3066 (.0647)

NOTE.—In panel A, other regressors are as reported in table 3. Remaining job duration is the number of years from t to the last observed year of the job. An interaction of remaining duration with a dummy for jobs that censor at the end of the panel is also included, but it had no effect on the results. In row 2 the omitted category is jobs that lasted 6 or more years beyond the current date. In panel B, the first-step models use jobs with different remaining job durations. For example, the last column estimates wage growth at t from jobs that survive 5 or more years beyond t . Standard errors are in parentheses.

seniority. I find no evidence for this effect. In fact, tenure profiles estimated from jobs with longer remaining durations—5 or more years beyond the current date in the last column—are virtually the same as the full-sample estimates reproduced in the first column of the table. Overall, I have not been able to find any evidence that selection on η plays an important role in affecting estimates of wage growth or the returns to seniority. This evidence favors substantial, *anticipated* returns to seniority in the typical employment relationship.

Ability Bias in the Returns to Job Tenure

To this point I have maintained the assumption that unobserved characteristics of individuals, μ_i , are unrelated to observed job tenure. Yet an alternative rationale for the positive relationship between job tenure and wages is that workers' unobserved productivities are negatively related to mobility. For example, more able (high-wage) persons may change jobs less often, so tenure and wages will be positively correlated in survey data even if $\beta_2 = 0$. Evidence suggestive of this is that education, an *observed* element of human capital, is negatively related to job changing. Alternatively, if turnover is costly to employers, then the net productivity of stable workers will be greater, and employers will pay more to obtain them. In either case, unobserved characteristics that raise wages (μ_i) are positively correlated with observed tenure, which raises the estimated returns to job seniority.

Because μ_i is a fixed effect, covariance of μ with the regressors in (1) will not bias estimators of $\beta_1 + \beta_2$, which is based on wage changes. Yet estimates of β_1 and β_2 from the second-step model (7) will be biased if μ_i and initial experience, X_0 , are correlated. In this case, the bias in the estimated return to seniority from the second-step model is

$$E\hat{\beta}_2 - \beta_2 = -b_1 - \gamma_{X_0T}(b_1 + b_2) - \gamma_{X_0\mu}, \quad (13)$$

where $\gamma_{X_0\mu} = (X_0'X_0)^{-1}X_0'\mu$. If high- μ persons change jobs less often, then on average they started their current jobs earlier. This implies $\gamma_{X_0\mu} < 0$, so the second-step estimator of β_2 may overstate the returns to seniority.

The importance of this bias can be evaluated if there is an instrumental variable that is uncorrelated with the fixed effects, μ_i , but correlated with X_0 . A plausible candidate is *total* experience, X . Specifically, I assume that the distribution of μ_i is unrelated to experience (successive cohorts of workers are equally able and equally mobile) so that $E(X'\mu) = 0$. With this condition, X may be used as an instrumental variable for X_0 in estimating the second-step model (7). The resulting bias in the instrumental variables estimator of β_2 is

$$E\hat{\beta}_2^{IV} - \beta_2 = -b_1 - \frac{\gamma_{XT}}{1 - \gamma_{XT}}(b_1 + b_2), \quad (14)$$

where γ_{XT} is the least-squares coefficient from a regression of tenure (T) on experience (X). This bias in the instrumental variables estimator is independent of the distribution of μ_i . Further, $\gamma_{XT} = .50$ in the data, and previous results imply $b_1 + b_2 > 0$ (see table 3). Thus the right-hand side of (14) is negative, so that β_2^{IV} provides a lower bound on β_2 even when μ_i and tenure are correlated. If μ_i and tenure are

correlated, then the estimated return to seniority will be lower when X is used as an instrument for X_0 in the second-step model.¹⁷

Estimates of the returns to seniority when X is used as an instrument in (7) differ trivially from those reported above. The estimated main effect of seniority (β_2) falls from .055 per year, reported in table 3, to .052 under instrumental variables. Over a 10-year horizon, this implies a difference of only 3 percent in the cumulative return to seniority, relative to the 25 percent cumulative return shown above. This is fairly strong evidence that unobserved personal characteristics do not account for the substantial returns to seniority shown in table 3.

E. Occupational Differences in Wage Profiles

All the preceding results refer to workers in an array of occupations, ranging from laborers to highly paid professionals. It is not hard to imagine technological or other differences across occupations that would generate corresponding differences in wage profiles. For example, investments in specific skills may be more important among professionals, while collective bargaining agreements may limit the ability of employers to back-load wages in unionized environments. Thus the preceding results may be sensitive to aggregation across diverse groups. Because of sample size limitations in the PSID, it is not possible to examine these issues in fine detail. Instead, evidence is presented for three broad categories of workers. Among craftsmen, operatives, and laborers, I treat union and nonunion workers separately.¹⁸ The third category consists of professional and service occupations. I finesse issues of promotion and the like by categorizing all periods of a job on the basis of the reported occupation in its first observed period.

Estimates of the time-series properties of wage changes showed only minor differences across groups. Briefly, the earlier finding that wages follow a random walk within jobs also holds for each of the occupational groups. The only difference worth noting is a substantially smaller variance in wage changes among unionized workers. Since collective bargaining arrangements normally set wages according to scale, this finding is plausible.

Table 5 shows estimated main effects of experience and job tenure,

¹⁷ Comparison of eqq. (14) and (13) implies that $E\hat{\beta}_2^{IV} < E\hat{\beta}_2 < \beta_2$ when $\gamma_{X_0\mu} = 0$. Thus least squares is the preferred estimator in (7) if unobserved characteristics and tenure are uncorrelated.

¹⁸ Jobs were categorized as "union" if the respondent indicated union membership in more than half of the years of the job. Other definitions were also tried, but the results were not sensitive to these changes.

TABLE 5
ESTIMATED RETURNS TO JOB SENIORITY BY OCCUPATIONAL CATEGORY AND
UNION STATUS, TWO-STEP ESTIMATOR

MAIN EFFECTS	PROFESSIONAL AND SERVICE	CRAFTSMEN, OPERATIVES, AND LABORERS	
		Nonunion	Union
Experience (β_1)	.0707 (.0288)	.1066 (.0342)	.0592 (.0338)
Tenure (β_2)	.0601 (.0127)	.0513 (.0146)	.0399 (.0147)
$\beta_1 + \beta_2$.1309 (.0254)	.1520 (.0311)	.0992 (.0297)

ESTIMATED CUMULATIVE RETURNS TO TENURE AT:	PROFESSIONAL AND SERVICE (1)	CRAFTSMEN, OPERATIVES, AND LABORERS		
		Nonunion (2)	Union Relative to:	
			Union Sector (3)	Nonunion Sector (4)
5 years	.1887 (.0388)	.1577 (.0428)	.1401 (.0437)	.2299 (.0931)
10 years	.2400 (.0560)	.2073 (.0641)	.2033 (.0620)	.3286 (.0854)
15 years	.2527 (.0656)	.2480 (.0802)	.2384 (.0739)	.4111 (.0855)
20 years	.2841 (.0663)	.3295 (.0914)	.2733 (.0783)	.4904 (.0957)
Number of observations	4,946	2,642		2,741

NOTE.—The effects of higher-order terms in experience and tenure ($X^2, X^3, X^4, T^2, T^3, T^4$) are estimated from models of within-job wage growth in each occupation. These are not reported separately. Other second-step regressors are as listed in the note to table 4. For union workers, the column labeled "union sector" measures $y^i(X, T) - y^i(X, 0)$, so it is the wage premium relative to starting a new union job. The column labeled "nonunion sector" measures $y^i(X, T) - y^i(X, 0)$, so it is the premium relative to a new nonunion job.

as well as cumulative returns to various levels of tenure, for each occupational group. The main finding is that estimated returns to tenure are quite similar across broad occupational categories. Differences in returns between white- and blue-collar professionals are trivial, as are differences in returns between union and nonunion workers within blue-collar professions. In fact, aggregation of the three wage profiles cannot be rejected.

Conceptually, the cumulative returns shown in table 5 measure the return to T years of job seniority in sector i as $y^i(X, T) - y^i(X, 0)$, the difference between the wage at tenure T and the wage in the typical alternative job in the same sector. This comparison may be inappropriate for union members. Since union jobs (u) are normally rationed, the relevant alternative may be employment in a nonunion job (n).

In this case the correct estimate of the return to T years of seniority is $y^u(X, T) - y^n(X, 0)$. This estimate will differ from the return to seniority for unionized workers because (i) unionized workers earn a premium that is lost in moving to the nonunion sector, and (ii) the returns to total market experience differ between the union and nonunion sectors. This point is demonstrated by comparing the two columns of cumulative returns for union members. When measured relative to another union job, $y^u(X, 0)$, estimated returns are essentially identical to those in other sectors. But measured relative to the nonunion alternative, $y^n(X, 0)$, returns are both larger and rising.¹⁹ According to the estimates in the last column, the typical union worker with 15 years of seniority would suffer a 50 percent ($e^{.4111} - 1$) wage cut if his current job were to end and he was forced to seek employment in the nonunion sector. This estimate of what a union worker would lose if his job were to end combines the union seniority effect, $y^u(X, T) - y^u(X, 0)$, and the union wage premium for new workers, $y^u(X, 0) - y^n(X, 0)$.²⁰

V. Comparisons with Other Research

My results and conclusions are substantially different from those reported in recent research. Specifically, the PSID data have also been analyzed by Altonji and Shakotko (1987) and by Abraham and Farber (1987), who conclude that the true returns to job seniority are minor. This difference in results cannot be attributed to the samples analyzed since they are virtually the same. An accounting of the reasons for our different findings is therefore warranted.

Altonji and Shakotko apply an instrumental variables procedure to a model like (1). They note that the deviation of job tenure from its observed, job-specific average is orthogonal to factors that are fixed within a job. If job effects are not time varying ($\phi_{ijt} = \phi_{ij}$), then $\sum(T_{ijt} - \bar{T}_{ij})(\phi_{ij} + \mu_i) \equiv 0$, and so $DT_{ijt} \equiv T_{ijt} - \bar{T}_{ij}$ is a valid instrumental variable.²¹ They therefore estimate a version of (1) by instrumental

¹⁹ The typical union member in these data started his job at $X_0 = 10$ years of labor market experience. For these calculations, I assume $X_0 = 10$ and allow both experience and tenure to accumulate from that point. Thus a person with 5 years of job tenure also has 15 years of labor market experience, and so on. Effects of other regressors (education etc.) also differed between union and nonunion jobs. The calculations refer to a person with average characteristics, so these differences are reflected in the estimates.

²⁰ A referee has pointed out that an estimate of the union wage premium at various levels of tenure is the difference in the cost of displacement for union and nonunion workers, assuming that all find nonunion jobs. This is $[y^u(X, T) - y^n(X, 0)] - [y^n(X, T) - y^n(X, 0)] = y^u(X, T) - y^n(X, T)$. This is the difference between col. 4 and col. 2 of table 5. Notice that this measure of the union wage premium rises with tenure.

²¹ The preceding evidence that the evolution of the wage within jobs follows a random walk is not consistent with fixed job effects unless the entire random walk component occurs in general human capital.

variables, using $Z = (X, DT)$ as instruments. It turns out that this instrumental variables estimator is a variant of the two-step procedure outlined above, which facilitates comparison. Let $W = (X, T)$, so the instrumental variables estimator of (1) is $(\hat{\beta}_1^{IV}, \hat{\beta}_2^{IV})' = (Z'W)^{-1}Z'y$. When these moments are written out, some algebra establishes that

$$\hat{B}^{IV} = (T'D'DT)^{-1}T'D'Dy, \quad (15a)$$

$$\hat{\beta}_1^{IV} = (X'X_0)^{-1}X'(Y - T\hat{B}^{IV}). \quad (15b)$$

Notice that (15a) is simply the least-squares estimator of $B = \beta_1 + \beta_2$ using deviations from within-job means rather than wage changes as a first-step model. This estimator of B is consistent when there are fixed job effects. The estimator of β_1 in (15b) is equivalent to using $X \equiv X_0 + T$ as an instrument for X_0 in the second-step model (7). Given a consistent estimator \hat{B} of B , this instrumental variables procedure is equivalent to the test used above for the importance of individual effects. Straightforward calculations yield

$$E\hat{\beta}_1^{IV} = E\hat{\beta}_1^{IS} + \left(\frac{\gamma_{XT}}{1 - \gamma_{XT}} - \gamma_{X_0T} \right) (b_1 + b_2), \quad (16)$$

where $\hat{\beta}_1^{IS}$ is the second-step estimator of β_1 found by applying least squares to (7), and γ_{XT} and γ_{X_0T} are the least-squares coefficients from regressions of tenure (T) on X and X_0 , respectively. Empirically, $\gamma_{XT} = .50$ and $\gamma_{X_0T} = -.25$. Since $b_1 + b_2 > 0$, this means that the instrumental variables procedure produces a greater upward bias in the return to experience and, so, a greater downward bias in the return to tenure. This is one reason for the small tenure effects estimated by Altonji and Shakotko.

A second reason for our different results is measurement error in recorded job tenure. As I noted earlier, job tenure in the PSID contains a large number of inconsistencies. For individuals used here, within-job, year-to-year changes in recorded job tenure range from -31 years to 7.5 years, and 36 percent of all changes in tenure fall below the theoretical value of 1.0. Because tenure is recorded in intervals, many jobs last several years with no change in reported tenure. This measurement error is magnified when within-job changes in tenure are used to estimate parameters of wage growth, so that estimated values of $\beta_1 + \beta_2$ will be biased down. In fact, the measurement error problem is so serious that reasonable estimates of the parameters of wage growth cannot be derived from the uncorrected data.

A final reason for the difference in our findings is that I use different methods to control for aggregate changes in real wages. As noted earlier, my estimates are based on wage data that are deflated by a real wage index calculated from cross sections of the CPS. This means

that wages in different years are expressed in comparable units. In contrast, Altonji and Shakotko control for changes in real wages by including a time trend in their regressions. If aggregate wage growth is truly linear and if cross sections of the panel are random samples of the population at each point in time, then this method is appropriate. Then because experience, tenure, and time change at the same rate during a job, within-job wage growth provides an estimate of $\beta_1 + \beta_2 + \beta_t$, where β_t is the trend rate of growth of aggregate wages. Comparison of average sample wage *levels* over time identifies β_t separately. Problems arise if the average “quality” of the sample improves through time; the data indicate that this is the case in the PSID. Then the average sample wage grows during the panel even if $\beta_t = 0$, causing β_t to be overestimated. This causes an additional downward bias in estimated returns to job tenure because $\hat{\beta}_2 = (\beta_1 + \beta_2 + \beta_t) - \hat{\beta}_1 - \hat{\beta}_t$.

Table 6 documents each of these points. Column 1 of the table reproduces the basic findings of Altonji and Shakotko, using the uncorrected PSID data on current job tenure, as well as their instrumental variables procedure and specification. For these estimates, all terms in job tenure, such as T^2 , are instrumented by deviations from within-job means, for example, $T^2 - \bar{T}^2$, but the levels of all terms in experience, such as X and X^2 , are treated as exogenous. The estimates confirm the small return to job tenure that was found by Altonji and Shakotko. Column 2 reproduces this specification in the corrected data, where tenure rises by one in each year of a job. In these data, estimated returns are more substantial: at 20 years of seniority the cumulative return from the corrected tenure data is roughly triple the estimate from the error-ridden data, though still smaller than in the two-step procedure set out above.

Column 3 takes the next step, replacing current experience with initial experience in the list of instrumental variables. Since higher-order terms in experience are also endogenous, they are also instrumented by deviations from job-specific means. For example, $X^2 - \bar{X}^2$ serves as an instrument for X_2 . With fixed job effects, this means that the effects of higher-order terms in experience and tenure are consistently estimated. With this adjustment in the instrument list, estimated returns are larger still. Finally, column 4 drops the endogenous time trend from the list of instrumental variables, which results in estimated returns that are roughly equivalent to those produced by the two-step method employed above. This is not surprising since (18) indicates that the instrumental variables specification is essentially equivalent to a two-step procedure.

In contrast, the very small effects of seniority estimated by Abraham and Farber (1987) are caused solely by differences in methodol-

TABLE 6
EFFECTS OF MEASUREMENT ERROR AND ALTERNATIVE INSTRUMENTAL VARIABLES ON THE ESTIMATED RETURNS TO JOB TENURE

BASIC INSTRUMENTS	ORIGINAL TENURE DATA:		CORRECTED TENURE DATA	
	($X, T - \bar{T}, \text{Time}$) (1)	($X, T - \bar{T}, \text{Time}$) (2)	($X^0, T - \bar{T}, \text{Time}$) (3)	($X^0, T - \bar{T}$) (4)
Main effect of job tenure (β_2)	.030 (.007)	.032 (.006)	.035 (.007)	.045 (.007)
Cumulative returns at tenure:				
5 years	.078 (.0206)	.098 (.017)	.121 (.019)	.155 (.021)
10 years	.074 (.025)	.122 (.024)	.177 (.022)	.223 (.025)
15 years	.052 (.031)	.131 (.028)	.211 (.020)	.264 (.024)
20 years	.052 (.039)	.161 (.035)	.252 (.018)	.316 (.024)

NOTE.—The basic specification is identical to that in earlier tables. Other instruments are as follows. In all models: $T^2 - \bar{T}^2$, $T^3 - \bar{T}^3$, $T^4 - \bar{T}^4$, education, union membership, disability, residence in an SMSA, census region, and married. In cols. 1 and 2: X^2 , X^3 , and X^4 . In cols. 3 and 4: $X^2 - \bar{X}^2$, $X^3 - \bar{X}^3$, $X^4 - \bar{X}^4$.

ogy. They argue that completed tenure (the ultimate duration of the job) is a good proxy for unobserved dimensions of job or worker quality when either good jobs survive or able persons are less mobile. Since completed duration is unobserved for most observations in available panel data (the data end during each person's last observed job, which tends to be his longest), they fill in "expected" completed job tenure for censored observations based on the frequency of job endings in the data. Call the ultimate estimate of completed duration $T^* = T^L + R$, where T^L is the last observed job tenure for a particular job and R is the predicted residual life of the job (= 0 for uncensored spells).

The procedure they propose for estimating β_2 is to include T^* as a regressor in an augmented version of (1):

$$y_{ijt} = X_{ijt}\beta_1 + T_{ijt}\beta_2 + T_{ijt}^*\theta + \xi_{ijt}^1 \quad (17)$$

Intuitively, T^* is meant to capture the effects of unobservables, ϕ and μ , and so to reduce the bias in the least-squares estimate of β_2 . Again if \bar{T}_j denotes the average observed value of tenure on job j , (17) is equivalent to

$$y = X_0\beta_1 + (T - \bar{T})(\beta_1 + \beta_2) + \bar{T}(\beta_1 + \beta_2) + T^*\theta + \xi^1. \quad (18)$$

Least squares applied to (17) is equivalent to estimating (18) and imposing the restriction that the coefficients on $T - \bar{T}$ and \bar{T} are identical. Notice that $X_0 \equiv X - T$ and \bar{T} are fixed within a job, so $T - \bar{T}$ is orthogonal to both of them by construction. Thus estimates of (18) *without* the implied parameter restriction will yield a consistent estimate of $\beta_1 + \beta_2$ in the case of fixed job effects. But the selection problem being addressed is that job-specific variables such as \bar{T} will be correlated with the unobservables, so the least-squares estimate of $\beta_1 + \beta_2$ multiplying \bar{T} will be biased. Thus imposing the cross-parameter restriction implicit in (17) yields an inconsistent estimator of $\beta_1 + \beta_2$.²²

²² This inconsistency is a short panel bias caused by the fact that the PSID (and other data sources) contains incomplete longitudinal histories. To see this, let T^0 be the first observed value of tenure on a job ($T^0 = 0$ for jobs that begin during the panel). Then $\bar{T} = (T^0 + T^L)/2$. Substitute this and $T^* = T^L + R$ into (18), yielding

$$y = X_0\beta_1 + (T - \bar{T})B + T^L(\theta + .5B) + R\theta + .5T^0B + \xi^1. \quad (19)$$

Again, T^L , R , and T^0 are fixed within jobs, and they are orthogonal to $T - \bar{T}$ by construction. According to the theory that motivates this approach, they will be correlated with the unobservables because of mobility decisions, so the restricted estimate of B will be inconsistent. With complete longitudinal histories, this source of bias vanishes because $T^0 = R = 0$ when the beginning and end of each job are observed. In this case, least-squares estimation of (19) is equivalent to a two-step procedure given by the deviations from means estimator of B and

$$y - T\hat{B} = X_0\beta_1 + T^L\theta + \xi^2. \quad (20)$$

TABLE 7
LEAST-SQUARES MODELS CONDITIONING ON (Estimated) COMPLETED JOB TENURE,
PSID WHITE MALES

	(1)	(2)	(3)	(4)	(5)
Experience	.0418 (.0013)	.0379 (.0014)	.0345 (.0015)	.0397 (.0013)	.0401 (.0014)
Experience ²	-.00079 (.00003)	-.00069 (.000032)	-.00072 (.000069)	-.00074 (.000030)	-.00073 (.000069)
Tenure	.0138 (.00052)	-.0015 (.0015)	.0137 (.0038)	.0060 (.00073)	.0163 (.0038)
Imputed completed tenure0053 (.00036)	.0067 (.00042)
Observed completed tenure0165 (.0016)	.0316 (.0022)
× censor	...	-.0025 (.00073)	-.0024 (.00073)
Experience ²	-.00061 (.000036)	...	-.00075 (.000033)
Tenure0142 (.0033)0429 (.0016)
R ²	.422	.428	.432	.433	.435

NOTE.—See note to table 4 for other regressors. Dependent variable is log average hourly earnings. Standard errors are in parentheses. Cols. 2 and 4 implement versions of the restricted model given by eq. (17). Estimates in cols. 3 and 5 are based on the unrestricted model (18).

The evidence on these points is presented in table 7. For purposes of comparison, the models in table 7 include only a quadratic in experience and only a linear effect of tenure, which is the functional form used by Abraham and Farber.²³ Because the estimated residual life of a job is a (nonlinear) function of the observables,²⁴ I also report estimates that control for the *observed* completed duration of a job, T^L , plus the interaction of T^L with an indicator that is one for jobs that censor at the end of the panel. Column 1 reports least-squares estimates, while columns 2 and 4 report the restricted estimates that include measures of completed tenure in the regression. The unrestricted models that are not subject to the bias just described are in columns 3 and 5. These estimates are derived by applying (18) and solving for β_2 from estimates of $\beta_1 + \beta_2$ and β_1 .

The estimates in columns 2 and 4 are qualitatively the same as

Equation (20) is in the form of the second-step model (7), augmented by the proxy variable T^L . If T^L is a positive predictor of the unobservables ($\theta > 0$) and more durable jobs occur later in careers ($\text{cov}[X_0, T^L] > 0$), then inclusion of T^L in the model will reduce the upward bias in estimating β_1 and raise the estimated return to seniority.

²³ Basic conclusions are unchanged for less restrictive functional forms.

²⁴ To model job endings, I estimated a discrete time proportional hazards model in which the hazard rate is $\lambda = \exp(Z\gamma)$, and Z includes the full vector of regressors used in the wage models. The estimates are available on request.

those produced by Abraham and Farber, showing negligible effects of tenure in comparison to column 1. Columns 3 and 5 show that the implicit restrictions in columns 2 and 4 are decisively rejected, however, and that relaxing these restrictions changes the results. In these models, $\beta_1 + \beta_2$ is consistently estimated and the estimated return to seniority is of the same magnitude as the least-squares estimate in column 1. The effect of completed job tenure is larger than in columns 2 and 4 as well. Thus the estimates are consistent with the notion that good jobs last longer, in the sense that long jobs pay high wages throughout; yet this fact does not reduce the estimated returns to seniority. As above, the returns to job seniority are substantial.

The main reason for this difference in results is a severe underestimate of within-job wage growth from the restricted model: evaluated at the sample mean level of experience (18.4 years), the restricted estimates in column 4 yield a predicted annual rate of wage growth of only 1.9 percent.²⁵ The corresponding estimate from column 5 is 3.0 percent. The reason for the bias is apparent from the estimated impact of \bar{T} , which is a biased estimate of $\beta_1 + \beta_2$. For example, in column 3, this estimate is .0142, compared to an unrestricted estimate of $.0345 + .0137 = .0482$. Thus the restricted estimates of $\beta_1 + \beta_2$ are biased down because they combine these two effects. This underestimate of within-job growth accounts for most of the lost value of β_2 in the restricted model.

VI. Conclusion

The idea that compensation rises with job tenure or seniority is the most fundamental prediction of the theory of specific human capital. It is also a key prediction of other contracting models in which the timing of compensation over the life of a job plays a strategic role in recruiting and motivating employees. Estimates of the return to seniority based on survey data have tended to support this class of theories, though these estimates have ignored potential biases generated by individuals' mobility decisions. Theory provides only limited guidance on the direction of these biases, and virtually none on their importance. When I correct for these biases in longitudinal data, my estimates imply a very strong connection between job seniority and wages in the typical employment relationship: other things constant, 10 years of job seniority raise the wage of the typical worker by over 25 percent. For the procedures that I have used, theory and related evidence suggest that this estimate is a lower bound on the true return to job seniority.

²⁵ The corresponding estimate from Abraham and Farber (1987) is 1.7 percent.

These results conform to several related facts about wages and the durability of jobs. For example, turnover rates are substantially lower among senior workers, even when individual and job-specific factors that affect mobility are controlled for (Topel and Ward, in press), and the typical employment relationship in the United States is remarkably durable (Hall 1982). These observations are difficult to explain in the absence of rising wages and accumulating specific capital. Further, estimates of the "costs" of displacement and unemployment indicate that the wage losses from these events are substantially larger for workers who had held their jobs longer (Carrington 1990; Topel, in press). Results in this paper imply this, but also the period of recovery from an initial wage loss may be fairly short.

These conclusions must be tempered by the fact that tenure measures time only in a particular job and may be only remotely related to the relevant concept of human capital. In one sense this measurement error implies that true returns may be even larger. Yet if human capital is specific to industries or sectors of the economy, and not to jobs, then job tenure may easily capture the returns to the broader concept of human capital, especially when job changes are infrequent. Nevertheless, the evidence presented here offers no support for the view that seniority has a negligible impact on wages.

Appendix

The Data

The data used in this study come from the first 16 waves of the Panel Study of Income Dynamics. The sample is restricted to white male heads of households who had positive earnings during the previous calendar year and who were between the ages of 18 and 60 at the survey date. Persons from Alaska and Hawaii were excluded. Jobs were excluded if the respondent reported that he was self-employed at any time during the job, if he worked for the government, if he reported agricultural employment, or if the observation came from the poverty subsample of the PSID. Finally, since wages refer to average hourly earnings in the year preceding the survey, observations for which current job tenure was less than 1 year were deleted. Other exclusions based on reported job tenure are described below.

Job tenure is the key variable in the analysis. Measured job tenure in the data is often recorded in wide intervals, and a large number of observations are lost because tenure is missing. Further, a large number of inconsistencies occur in the data. For example, reported tenure may fall by 10 years or more between years of a single job, and periods of missing tenure are followed by years in which a respondent reports more than 20 years of seniority for the remainder of the job. In the recorded tenure data, the year-to-year changes in job tenure range from -31 years to 7.5 years. In 324 cases (3.8 percent), measured tenure declines between years of a job, and in 51 cases the decline is greater than 5 years. Because tenure is recorded in intervals, 36 percent of all year-to-year changes in tenure fall below the theoretical value of 1.0.

In light of these errors, I reconstructed job tenure as follows. For jobs that

begin in the panel, tenure is started at zero and incremented by one for each year in which a person works. Thus experience and tenure progress at the same rate. For jobs that were in progress at the beginning of a person's record, I gauged starting tenure relative to the period in which the person achieved his maximum reported tenure on a job. Again, tenure and experience increment by one for each year in which the person works.

Even with this procedure, there are many ambiguities about starting and ending dates of jobs. In many cases the recorded sequence of job tenures seems to indicate a job change (e.g., tenure falls to zero and then rises smoothly for the remainder of the job, or the worker indicates unemployment due to a permanent layoff), though no change of employer is recorded in the data. I considered a large number of such circumstances generated by numerous cross-checks on the data. Basically, I deleted all jobs for which significant ambiguities occurred. In practice, these deletions had very minor effects on the results and none on the conclusions.

All the sample selection criteria are documented in the programs and output underlying this research. These are available at cost. Summary statistics are reported in table A1.

TABLE A1
VARIABLE DEFINITIONS AND SUMMARY STATISTICS, PSID WHITE MALES, 1968-83

Variable	Definition	Mean	Standard Deviation
Real wage	Log average hourly earnings deflated by CPS wage index and GNP price deflator	1.131	.497
Experience	Years in labor market	20.021	11.045
Tenure	Years of current job seniority	9.978	8.944
Education	Years of completed schooling	12.645	2.809
Married	1 if currently married, spouse present	.925	.263
Union	1 if union member	.344	.473
SMSA	1 if resides in SMSA	.644	.478
Disabled	1 if currently reporting disability	.074	.262

Survey Year	Percentage of Sample	CPS Real Wage Index
1968	.052	1.000
1969	.050	1.032
1970	.051	1.091
1971	.053	1.115
1972	.057	1.113
1973	.058	1.151
1974	.060	1.167
1975	.061	1.188
1976	.065	1.117
1977	.065	1.121
1978	.069	1.133
1979	.071	1.128
1980	.073	1.128
1981	.072	1.109
1982	.071	1.103
1983	.068	1.089

References

- Abraham, Katharine G., and Farber, Henry S. "Job Duration, Seniority, and Earnings." *A.E.R.* 77 (June 1987): 278-97.
- Altonji, Joseph G., and Shakotko, Robert A. "Do Wages Rise with Job Seniority?" *Rev. Econ. Studies* 54 (July 1987): 437-59.
- Becker, Gary S. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. New York: Columbia Univ. Press (for NBER), 1964.
- Becker, Gary S., and Stigler, George J. "Law Enforcement, Malfeasance, and Compensation of Enforcers." *J. Legal Studies* 3 (January 1974): 1-18.
- Borjas, George J., and Rosen, Sherwin. "Income Prospects and Job Mobility of Young Men." In *Research in Labor Economics*, vol. 3, edited by Ronald G. Ehrenberg. Greenwich, Conn.: JAI, 1980.
- Brown, James N. "Why Do Wages Increase with Tenure? On-the-Job Training and Life-Cycle Wage Growth Observed within Firms." *A.E.R.* 79 (December 1989): 971-91.
- Burdett, Kenneth. "A Theory of Employee Job Search and Quit Rates." *A.E.R.* 68 (March 1978): 212-20.
- Carrington, William. "Specific Human Capital and Worker Displacement." Ph.D. dissertation, Univ. Chicago, 1990.
- Doeringer, Peter B., and Piore, Michael J. *Internal Labor Markets and Manpower Analysis*. Lexington, Mass.: Heath, 1971.
- Freeman, Smith. "Wage Trends as Performance Displays Productive Potential: A Model and Application to Academic Early Retirement." *Bell J. Econ.* 8 (Autumn 1977): 419-43.
- Hall, Robert E. "The Importance of Lifetime Jobs in the U.S. Economy." *A.E.R.* 72 (September 1982): 716-24.
- Harris, Milton, and Holmstrom, Bengt. "A Theory of Wage Dynamics." *Rev. Econ. Studies* 49 (July 1982): 315-33.
- Heckman, James J. "The Common Structure of Statistical Models of Truncation, Sample Selection and Limited Dependent Variables and a Simple Estimator for Such Models." *Ann. Econ. and Soc. Measurement* 5 (Fall 1976): 475-92.
- Jovanovic, Boyan. "Firm-specific Capital and Turnover." *J.P.E.* 87 (December 1979): 1246-60. (a)
- . "Job Matching and the Theory of Turnover." *J.P.E.* 87, no. 5, pt. 1 (October 1979): 972-90. (b)
- . "Matching, Turnover, and Unemployment." *J.P.E.* 92 (February 1984): 108-22.
- Lang, Kevin. "Reinterpreting the Return to Seniority." Manuscript. Boston: Boston Univ., November 1987.
- Lazear, Edward P. "Agency, Earnings Profiles, Productivity, and Hours Restrictions." *A.E.R.* 71 (September 1981): 606-20.
- Lazear, Edward P., and Rosen, Sherwin. "Rank-Order Tournaments as Optimum Labor Contracts." *J.P.E.* 89 (October 1981): 841-64.
- Lewis, H. Gregg. *Union Relative Wage Effects: A Survey*. Chicago: Univ. Chicago Press, 1986.
- Marshall, Robert C., and Zarkin, Gary A. "The Effect of Job Tenure on Wage Offers." *J. Labor Econ.* 5 (July 1987): 301-24.
- Mincer, Jacob. "Wage Changes in Job Changes." In *Research in Labor Economics*, vol. 8, pt. A, edited by Ronald G. Ehrenberg. Greenwich, Conn.: JAI, 1986.
- . "Job Training, Wage Growth and Labor Turnover." Manuscript. New York: Columbia Univ., 1988.

- Mincer, Jacob, and Jovanovic, Boyan. "Labor Mobility and Wages." In *Studies in Labor Markets*, edited by Sherwin Rosen. Chicago: Univ. Chicago Press (for NBER), 1981.
- Mortensen, Dale T. "Matching: Finding a Partner for Life or Otherwise." *American J. Sociology* 94 (suppl., 1988): S215-S240. (a)
- . "Wages, Separations, and Job Tenure: On-the-Job Specific Training or Matching?" *J. Labor Econ.* 6 (October 1988): 445-71. (b)
- Murphy, Kevin M., and Topel, Robert H. "Estimation and Inference in Two-Step Econometric Models." *J. Bus. and Econ. Statis.* 3 (October 1985): 370-79.
- Murphy, Kevin M., and Welch, Finis. "The Structure of Wages." Manuscript. Chicago: Univ. Chicago, 1987.
- Rosen, Sherwin. "Prizes and Incentives in Elimination Tournaments." *A.E.R.* 76 (September 1986): 701-15.
- . "Transactions Costs and Internal Labor Markets." *J. Law, Econ., and Organization* 4 (Spring 1988): 49-64.
- Salop, Joanne, and Salop, Steven. "Self-Selection and Turnover in the Labor Market." *Q.J.E.* 90 (November 1976): 619-27.
- Shleifer, Andrei, and Summers, Lawrence H. "Breach of Trust in Hostile Takeovers." In *Corporate Takeovers: Causes and Consequences*, edited by Alan J. Auerbach. Chicago: Univ. Chicago Press (for NBER), 1988.
- Topel, Robert H. "Job Mobility, Search, and Earnings Growth: A Reinterpretation of Human Capital Earnings Functions." In *Research in Labor Economics*, vol. 8, pt. A, edited by Ronald G. Ehrenberg. Greenwich, Conn.: JAI, 1986.
- . "Specific Capital, Mobility, and Wages: Wages Rise with Job Seniority." Working Paper no. 3294. Cambridge, Mass.: NBER, March 1990.
- . "Specific Capital and Unemployment: Measuring the Costs and Consequences of Worker Displacement." *Carnegie-Rochester Conf. Ser. Public Policy* (in press).
- Topel, Robert H., and Ward, Michael. "Job Mobility and the Careers of Young Men." *Q.J.E.* (in press).
- Welch, Finis. "Effects of Cohort Size on Earnings: The Baby Boom Babies' Financial Bust." *J.P.E.* 87, no. 5, pt. 2 (October 1979): S65-S98.