Chapter 37

MOBILITY AND STABILITY: THE DYNAMICS OF JOB CHANGE IN LABOR MARKETS

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Abstract

Three central facts describe inter-firm worker mobility in modern labor markets: (1) long-term employment relationships are common; (2) most new jobs end early; and (3) the probability of a job ending declines with tenure. Models based on firm-specific capital provide a parsimonious explanation for these facts, but it also appears that worker heterogeneity in mobility rates can account for much of what we observe in these data. I investigate tests of the specific capital model and consider whether these tests are successful in distinguishing the specific capital model from a model based on heterogeneity. One approach uses longitudinal data with detailed mobility histories of workers. These analyses suggest that both heterogeneity and specific capital (implying true duration dependence in the hazard of job ending) appear to be significant factors in accounting for mobility patterns. A second approach is through estimation of the return to tenure in earnings functions. This is found to have several weaknesses including the endogeneity of tenure and the lack of tight theoretical links between tenure and accumulated specific capital and between productivity and wages. A third approach is to use data on the earnings experience of displaced workers. Several tests are derived based on these data, but there is generally an alternative heterogeneity-based explanation that makes interpretation difficult. Nonetheless, firms appear willing to pay to encourage long-term employment relationships, and they may do so because it is efficient to invest in their workforce. On this basis, I conclude that, while deriving convincing direct evidence for the specific capital model of mobility is difficult, it appears that specific capital is a useful construct for understanding worker mobility and wage dynamics. © 1999 Elsevier Science B.V. All rights reserved.

JEL codes: J41; J63

1. Introduction

It is evident to even the casual observer that the labor market in the United States and other developed countries is not primarily a spot market characterized by short-term employment relationships between workers and firms. There is not high frequency movement by workers from firm to firm or, put another way, high frequency movement by firms from worker to worker. For example, in February 1996, 35.4% of workers aged 35–64 in the United States had been with their current employer for at least 10 years, and 20.9% of workers aged 45–64 had been with their current employer for at least 20 years.¹ On the other hand, neither is the labor market static in the sense that workers and firms are irrevocably bound to each other. For example, at the same date, 19.1% of American workers 20–64 had been with their current employer for less than 1 year.² Given a civilian employment level of 125.7 million workers in the United States, this suggests that about 24 million new employment relationships existed in March 1997 that did not exist in February 1996.³ And the number of new

¹ These statistics are based on tabulations of data from the mobility supplement to the February 1996 Current Population Survey (CPS). See Farber (1997b) for details.

² These statistics are based on tabulations of data from the mobility supplement to the February 1996 Current Population Survey (CPS). See Farber (1997c) for details.
employment relationships that started between March 1995 and February 1996 is almost surely much larger than 24 million because many new employment relationships started during this time period did not survive until February 1996. Since overall employment in the United States increased by only 700,000 jobs over the same period, virtually all of these new employment relationships involve job change.

Both the high incidence of long-term employment relationships and the high level of job change, defined here as change of employer, are important features of modern labor markets. In this chapter I attempt to place these facts in perspective highlighting the potential role of firm-specific capital and heterogeneity across workers. The next section contains a description of various sources of data on job mobility. This discussion focuses on the United States because of the availability there of consistent data on tenure and mobility over a fairly substantial period of time. In Section 3, I outline a set of important facts on job change that a theory of worker mobility needs to explain. These facts are that (1) long-term employment relationships are common, (2) most new jobs end early, and (3) the probability of a job ending declines with tenure. I present evidence mainly for the United States but also for other countries that establishes the basis for these facts. I also review some recent literature investigating whether job stability has been declining in the United States. Section 4 contains a discussion of models based on firm-specific capital, including match quality, as an explanation for the stylized facts. In Section 5, I establish that worker and job heterogeneity in mobility rates can largely account for the facts, without resort to the existence of specific capital.

In Section 6, I discuss some tests of the relative importance of heterogeneity and specific capital in explaining mobility rates based on the relationship of mobility rates with experience, tenure, and more detailed mobility histories. Section 7 contains a discussion of testing the specific capital model through estimation of the return to tenure in earnings functions. I also discuss the weaknesses of this approach, including endogeneity of tenure and the lack of tight theoretical links between tenure and accumulated specific capital and between productivity and wages are discussed. The recent literature on estimating the return to tenure is reviewed in some detail. In Section 8, I discuss the use of data on the earnings experience of displaced workers to test the specific capital model. Several tests are derived, but there is generally an alternative heterogeneity-based explanation that makes testing difficult. Section 9 contains some final remarks.

2. Sources of data on job mobility

The discussion in this section focuses on the United States because, in contrast to most other countries, data on worker mobility are available over a substantial period of time. As a

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3 The employment statistics are taken from US Bureau of Labor Statistics Series ID LFS11000000. This is the seasonally adjusted civilian employment level derived from the Current Population Survey for workers aged 16 and older.

4 I present evidence below on the high hazard of jobs ending during the first year.
result, not surprisingly, most of the literature on worker mobility, analyzes the American experience. However, the issues of data quality and needs are of more general applicability.

2.1. The current population survey data on tenure

Much of what we know about the tenure distribution in the United States comes from the Current Population Survey (CPS). At irregular intervals, the Census Bureau has appended mobility supplements to the January Current Population Survey. The years in which they did so include 1951, 1963, 1966, 1968, 1973, 1978, 1981, 1983, 1987, and 1991. Mobility supplements were also appended to the February 1996 and February 1998 CPSs. These supplements contain information that can be used to compute job tenure, defined as time with the current employer.\(^5\) Information on job tenure is also available in pension and benefit supplements to the CPS in May of 1979, 1983, and 1988, and in April 1993. Finally, data on job tenure can be derived from the contingent and alternative employment arrangement supplements (CAEAS) to the CPS in February 1995 and February 1997.

Important problems of comparability of data over time exist because of substantial changes in the wording of the central question about job duration. The early mobility supplements (1951–1981) asked workers what year they “...started working at their present job or business.” The mobility supplements in 1983, 1997, and 1991 asked workers how many years they have “...been working continuously for the present employer...” The most recent mobility supplements (1996 and 1998) asked workers how long they have “...been working continuously for the present employer...” and let the respondent define the time units. The pension and benefit supplements to the CPS in May of 1979, 1983, and 1988, and in April 1993 asked workers “How many years have you worked for your present employer?” The question goes on to say “If there has been an interruption of one year or more, count only the years since that interruption.” The CAEASs to the CPS in February 1995 and February 1997 asked workers how long they had been working for their present employer with no reference to continuity of employment and allowed the respondents define the time units. These differences affect the comparability of the responses.

First and most obviously, the early mobility supplement question refers to time on the present job rather than time with the present employer. If workers change jobs without changing employers (e.g., promotion or reassignment), then time on the job will be shorter than time with employer. Second, different groups of supplements handle interrupted spells differently. The recent mobility supplements ask about continuous spells without elaborating on what constitutes continuity, while the pension and benefit supplements direct the respondent to ignore interruptions of less than 1 year. There is no mention of continuity in the early mobility supplement question or in the CAEASs. Assuming that the natural inclination of workers will be to ignore interruptions of “reasonable” length if no mention is made of continuity, it appears that these differences will reduce reported

\(^5\) Only the mobility supplements since 1973 are available in machine-readable form.
durations in the later mobility supplements and the CAEASs relative to both the early mobility supplements and the pension and benefit supplements. Third, there is likely to be heaping of responses at round numbers that will be different for the early mobility supplements (which ask for a calendar year) than for the later mobility supplements and the pension and benefit supplements (which ask for a number of years or simply how long). In the early mobility supplements question, the spikes occur at round calendar years (1960, 1965, etc.). In the other supplements, the spikes occur at round counts of years (5, 10, 15, etc.). Additionally, an inquiry about when the job started may evoke systematically different responses than a question about length of employment.

2.2. Data from the March CPS on job change

An alternative, and rarely used source of information on worker mobility in the United States is the Annual Income Supplement to the March CPS. This supplement collects information on employment and income in the previous calendar year. Since 1976, the supplement has contained a question asking how many employers the individual had during the previous year, not counting jobs held simultaneously. In other words, dual job holders are not counted as having multiple jobs. The underlying concept is to derive a measure of the number of main jobs. The response is coded as zero, one, two, or three-or-more jobs.

The response to the question on number of jobs held last year can be used in a straightforward way to derive a lower bound estimate of the fraction of individuals who changed jobs in the previous year (and hence are in new jobs). This is computed as the number of individuals reporting more than one job divided by the number of individuals reporting at least one job. This estimate is a lower bound because some individuals may have lost a job they had held all year shortly before the year in question ended and did not find a job until early the following year. They would be counted as non-changers, but, in fact, the jobs they held did end. Stewart (1997) presents the only analysis of which I am aware that has used these data to compute rates of job change. I present my own analysis of these data in the next section.

2.3. Longitudinal data from the PSID and NLS

Longitudinal data provide important obvious advantages for analyzing worker mobility. By following workers over time, the timing of job change can be observed and, in contrast to cross-sectional data, completed job durations can be observed directly. Another advantage is that successive jobs held by individuals can be observed so that more dynamic approaches to modeling mobility can be investigated. Finally, the data generally contain information on the reason for job change, and most changes can be classified as voluntary.

Ureta (1992) presents the only analysis of which I am aware that explicitly addresses the rounding and heaping problems. In the recent supplements where the respondent selects the time units, virtually all responses greater than 2 years are reported in years and there is heaping at the usual round numbers.
The standard longitudinal data sets have been used to study mobility in the United States. These include the four original cohorts of the National Longitudinal Surveys (NLS), the National Longitudinal Survey of Youth (NLSY), and the Panel Study of Income Dynamics (PSID). These data are described in more detail in the chapter in this volume by Angrist and Krueger (1998).

The longitudinal data also have some weaknesses. One is the relatively small sample sizes they offer, at least in comparison with the CPS. Another is difficulty in timing job changes. This is particularly troublesome in data sets like the PSID and original NLS cohorts that are not designed for the study of employment dynamics. For example, the PSID has surveyed individuals each year, and, apart from changes in question wording over time, contains information sufficient to determine if the main job held at the survey date is the same job as the main job held at the survey date the previous year. However, simple tabulation shows that inconsistencies and ambiguities in responses are common.

The NLSY has some advantages for the study of employment dynamics because it codes the starting and ending dates of all jobs held (to a maximum of five per year). This allows more precise timing of job changes as well as the observation of jobs other than the main job held at the survey date. These advantages are particularly important when studying mobility early in jobs because a substantial fraction of jobs end within the first year. Thus, surveys that can only observe jobs that are in existence at a particular survey date, like the PSID and original NLS cohorts, are likely to miss much of the detailed structure of mobility.

2.4 Aggregate turnover data from employment and earnings

Prior to the availability of large-scale cross-sectional data sets from the CPS and longitudinal data sets from the PSID and NLS in the late 1960s and early 1970s, the central source of data on worker mobility was from turnover rates collected at the establishment level as part of the Employment and Earnings series of the U.S. Bureau of Labor Statistics. These data were collected monthly, available aggregated to the industry level, and measured quit rates and layoff rates separately along with the total separation rate. The data also contained information on new hires and rehires along with the total hiring rate. Unfortunately, this series was discontinued in the early 1980s.

Important early studies of turnover behavior (e.g., Pencavel, 1970, 1972) relied on these data. Unfortunately, with the discontinuation of this series, no large scale survey administered at regular intervals contains information about job change by cause (voluntary versus involuntary). The Displaced Workers Surveys, conducted since 1984 as supplements to the CPS, contain information on whether workers have changed jobs involuntarily for

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7 Brown and Light (1992) present a detailed useful discussion of problems in using the PSID and NLS original cohorts to time job changes. Abraham and Farber (1987) discuss some of the problems in using the PSID to develop a continuous measure of job tenure over time.

8 See, for example, Farber (1994). Evidence on the high hazard of jobs ending in the first year is presented in the next section.
certain reasons, but there is no comparable source for data on voluntary job change or on involuntary job change generally.

2.5. The Displaced Workers Surveys

The Displaced Workers Surveys (DWSs) have been administered every 2 years since 1984 as a supplement to the monthly Current Population Survey (CPS). Each Displaced Workers Survey from 1984–92 asks workers if they were displaced from a job at any time in the preceding 5-year period. The 1994 and 1996 DWSs ask workers if they were displaced from a job at any time in the preceding 3-year period. Displacement is defined in the interviewer instructions to the relevant Current Population Surveys as involuntary separation based on operating decisions of the employer. Such events as a plant closing, an employer going out of business, a layoff from which the worker was not recalled are considered displacement. Workers who are laid off from a job and rehired in a different position by the same employer are considered to have been displaced. Other events including quits and being fired for "...poor work performance, disciplinary problems, or any other reason that is specific to the individual alone..." are not considered displacement (US Department of Commerce, 1988, Section II, p. 4). Thus, the supplement is designed to focus on the loss of specific jobs that result from business decisions of firms unrelated to the performance of particular workers.

The DWS has been used extensively in recent years to measure rates of job loss. Additionally, since those individuals identified as job losers are asked a series of followup questions about the reason for their job loss, the characteristics of their lost job, and their labor market experience subsequent to job loss, the DWS has also been used to examine the consequences of job loss. I survey some of this literature below as part of an evaluation of theories of mobility.

2.6. A proposal for improved data on mobility from the CPS

While the DWSs provide information at regular intervals on the rate of job loss for a large representative cross-section, no comparable source for data on quits or job change generally exists in the United States. The February 1996 and 1998 CPSs represent an improvement over earlier CPSs with mobility supplements or DWSs in that they include mobility supplements with data on tenure on the current job as well as DWSs with data on job loss in the previous 3 years. This greatly facilitates investigating the relationship between rates of job loss and tenure. However, the information is inadequate to investigate the relationships between quit rates and tenure and the rate of overall job change and tenure.

9 Data from the February 1998 CPS were not yet available as this was being written.

10 Farber (1993) uses the earlier DWSs to investigate how the probability of job loss is related to tenure, but he was forced to do this indirectly by aggregating data from the job losers in the DWS into cells defined by demographic and job characteristics (including tenure) and data from mobility supplements to the CPS aggregated in the same way.
The data available to analyze voluntary job change are relatively sparse, particularly since the discontinuation of the turnover series that was part of Employment and Earnings.

One useful and relatively low cost proposal to improve the data available to analyze worker mobility in the United States would be to recast the mobility and displaced workers supplements (currently administered together) as a single mobility and job-change supplement. This supplement would ask for information on current job tenure as well as on job change during some time period (e.g., 2 years) prior to the survey. Job change would be defined to include voluntary job change and job loss “for cause” as well as displacement. For individuals who report a job change, information would be collected on the reason for the job change, the characteristics of the lost job, and labor market experience since the job change. This is a straightforward generalization of the current combined mobility and DWS supplements that are scheduled to be administered every 2 years, and it would greatly enrich the information available to study labor market dynamics at relatively low cost.

Similar surveys could also be carried out as supplementary parts of the household surveys in other countries. There are important institutional differences in the labor markets across countries that may have important effects on worker mobility. Having a consistent source of information on worker mobility over time is an important precondition both for understanding mobility in modern labor markets and for investigating the role of institutions in the allocation of labor.11

3. A set of facts on job change

In this section, I present evidence largely from US data establishing three central facts regarding job change. They are that (1) long-term employment relationships are common, (2) most new jobs end early, and (3) the probability of job change declines with tenure. These facts have strong empirical support, and they represent the core facts that theories of job change need to explain.

3.1. Long-term employment relationships are common

Other than a series of reports issued by the Bureau of Labor Statistics (BLS) summarizing the tenure data from the CPS mobility supplements, Hall (1982) seems to have been the first to use the CPS data to analyze job tenure. In an influential analysis of long-term employment that relied on published tabulations from the January 1968 and 1978 CPS mobility supplements, Hall summarized the cross-sectional distribution of job tenure. He also used these data to calculate the probability that workers with given amounts of tenure will remain in their jobs. He approached this in two ways. First, he used the 1968 and 1978

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11 Burgess (1998) presents an multi-country analysis of long-term employment that relies on tabulations supplied by researchers in ten countries. However, the data are not collected on a consistent basis across countries, making interpretation difficult.
CPS tenure data to compute retention rates for workers over the 10-year period. He accomplished this using artificial cohorts of workers who are age $x$ in 1968 and age $x + 10$ in 1978. The retention rate is defined as the ratio of the number of workers who are age $x + 10$ with tenure $t + 10$ in 1978 to the number of workers who are age $x$ with tenure $t$ in 1968. This is an estimate of the probability that a worker age $x$ with tenure $t$ in 1968 retains his job until 1978. Hall also computed contemporaneous retention rates using individual CPS mobility supplements by comparing the fraction of those workers who are age $x$ in 1978 who had tenure $t$ with the fraction of those workers who are age $x + y$ in 1978 who had tenure $t + y$. Relying mainly on this analysis, Hall found that (1) while any particular new job is unlikely to last a long time, a job that has already lasted 5 years has a substantial probability of lasting 20 years, (2) a substantial fraction of workers will be on a "lifetime" job (defined as lasting at least 20 years) at some point in their life, and (3) men are substantially more likely than women and whites are substantially more likely than blacks to have such a lifetime job. Ureta (1992) used the January 1978, 1981, and 1983 mobility supplements to recompute retention rates using artificial cohorts rather than contemporaneous retention rates. Like Hall, she found that lifetime jobs are an important feature of the US labor market, but she finds smaller differences by sex.

There is a substantial body of recent work that has focused on whether or not the tenure distribution has shifted over time. Specifically, there is concern that a change in employer behavior, perhaps motivated by changing market conditions, has led to a decrease in job stability. I review a portion of that literature here.

A study by Swinnerton and Wial (1995), using CPS mobility data from 1979 to 1991, analyzed job retention rates computed from artificial cohorts and concluded that there has been a secular decline in job stability in the 1980s. In contrast, Diebold et al. (1994), using CPS data on tenure from 1973 to 1991 to compute retention rates for artificial cohorts, found that aggregate retention rates were fairly stable over the 1980s but that retention rates declined for high school dropouts and for high school graduates relative to college graduates over this period. A direct exchange between Diebold et al. (1996) and Swinnerton and Wial (1996) seems to support the view that the period from 1979 to 1991 is not a period of generally decreasing job stability. I have also investigated this issue (Farber, 1998a), and, using CPS data on job tenure from 1973 to 1993, I find that the prevalence of long-term employment has not declined over time but that the distribution of long jobs has shifted. I find that less-educated men are less likely to hold long jobs than they were previously but that this is offset by a substantial increase in the rate at which women hold long jobs.

In another study (Farber, 1997b), I used CPS mobility data from 1979 to 1996 to compute the fraction of workers with more than 10 and more than 20 years of tenure, and I present some of those tabulations here. The first column of Table 1 contains the

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12 These mobility and benefit supplements are used because they contain comparable questions asking how long workers have been with their current employer. The mobility supplements prior to 1983 asked about year of starting the current job.
fraction of workers aged 35–64 who report more than 10 years of tenure, and the first column of Table 2 contains the fraction of workers aged 45–64 who report more than 20 years of tenure. These tabulations establish that a substantial fraction of workers are in long-term employment relationships. The most recent data in the tables (February 1996) show that about 35% of workers aged 35–64 had worked for the same employer for at least 10 years and about 21% of workers aged 45–64 had worked for the same employer for at least 20 years.

There is interesting time-series movement in these data. The incidence of long-term employment as measured by these fractions fell substantially after 1993 to its lowest level since 1979. However, in my earlier work based on the DWS (Farber, 1997b), I found that the decline in the incidence of long-term employment relationships for all workers was not mirrored in an increase in the incidence of long-term employment on lost jobs (jobs from which workers were laid off). Thus, the evidence is not consistent with the view that the decline in long-term employment relationships is the result of employers targeting long-

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Note: These data are derived from mobility or pension and benefit supplements to the indicated Current Population Surveys. All analyses are weighted by the CPS sampling weights. Standard errors are in parentheses. These tabulations are taken from Farber (1997b).
term employees for layoff. In fact, the share of displaced men who are displaced from long-term employment relationships has declined since 1979.

It is possible that the decline in the fraction of workers in long-term employment relationships in the mid-1990s could be accounted for by the strong labor market and expanding employment over this period. Indeed, similar declines are in evidence in the expansion of the mid-1980s. However, Neumark et al. (1999) using CPS data from the recent mobility supplements and February 1995 CAEAS, find that retention rates at higher tenure levels have declined in the 1990s. This decline in retention rates cannot be accounted for by expanding employment, but the authors conclude that the overall pattern of evidence does not support a long-term trend decline in aggregate job stability.

Others have used data from the Panel Study of Income Dynamics (PSID) to study the incidence of long-term employment. Rose (1995) measured job stability by examining the fraction of male workers who report no job changes in a given time period, typically 10 years. Rose found that the fraction of workers who reported no job changes in given length of time was lower in the 1980s than in the 1970s. He argued that this is evidence of decreasing stability of employment. Jaeger and Stevens (1999) used data from the PSID and the CPS mobility and benefit supplements on (roughly) annual rates of job change to try to reconcile evidence from the CPS and PSID on job stability. They find little evidence

\[ \begin{array}{cccccccc}
\text{Survey} & \text{Pooled} & \text{Male} & \text{Female} & \text{ED} < 12 & \text{ED} = 12 & \text{ED} 13-15 & \text{ED} \geq 16 \\
\hline
\text{May79} & 0.251 & 0.338 & 0.131 & 0.225 & 0.263 & 0.255 & 0.266 \\
& (0.005) & (0.008) & (0.007) & (0.010) & (0.009) & (0.014) & (0.008) \\
\text{Jan83} & 0.232 & 0.325 & 0.111 & 0.215 & 0.235 & 0.218 & 0.256 \\
& (0.004) & (0.005) & (0.004) & (0.007) & (0.006) & (0.009) & (0.008) \\
\text{May83} & 0.236 & 0.322 & 0.128 & 0.198 & 0.245 & 0.224 & 0.271 \\
& (0.005) & (0.008) & (0.007) & (0.011) & (0.009) & (0.014) & (0.012) \\
\text{Jan87} & 0.211 & 0.294 & 0.109 & 0.193 & 0.203 & 0.209 & 0.243 \\
& (0.003) & (0.005) & (0.004) & (0.008) & (0.005) & (0.008) & (0.008) \\
\text{May88} & 0.237 & 0.314 & 0.145 & 0.218 & 0.238 & 0.215 & 0.267 \\
& (0.005) & (0.008) & (0.006) & (0.011) & (0.008) & (0.012) & (0.011) \\
\text{Jan91} & 0.226 & 0.305 & 0.135 & 0.187 & 0.225 & 0.217 & 0.258 \\
& (0.004) & (0.005) & (0.004) & (0.009) & (0.006) & (0.008) & (0.007) \\
\text{Apr93} & 0.232 & 0.307 & 0.147 & 0.173 & 0.226 & 0.242 & 0.259 \\
& (0.005) & (0.008) & (0.006) & (0.013) & (0.008) & (0.010) & (0.010) \\
\text{Feb96} & 0.209 & 0.270 & 0.143 & 0.198 & 0.220 & 0.192 & 0.217 \\
& (0.003) & (0.005) & (0.004) & (0.009) & (0.006) & (0.006) & (0.006) \\
\text{N} & 83224 & 45010 & 38214 & 15326 & 32124 & 16013 & 19761 \\
\hline
\end{array} \]

\( ^a \)Note: These data are derived from mobility or pension and benefit supplements to the indicated Current Population Surveys. All analyses are weighted by the CPS sampling weights. Standard errors are in parentheses. These tabulations are taken from Farber (1997b).
in either survey of a trend in job stability though the estimates from the PSID are rather imprecise.\textsuperscript{14} Unfortunately, due to the design of the PSID, neither of these studies examine the mobility experience of women.

The Displaced Workers Surveys have also been used to investigate changes in job stability by examining rates of job loss. I used the five Displaced Workers Surveys (DWSs) from 1984 to 1992 to examine changes in the incidence and costs of job loss over the period from 1982 to 1991 (Farber, 1993). I found that there were slightly elevated rates of job loss for older and more educated workers in the slack labor market in the latter part of the period compared with the slack labor market of the earlier part of the period. But job loss rates for younger and less educated workers were substantially higher than those for older and more educated workers throughout the period. These findings are consistent with the long-standing view that younger and less educated workers bear the brunt of recessions.

Gardner (1995) used the six DWSs from 1984 to 1994 to examine the incidence of job loss from 1981 to 1992. While she found roughly comparable overall rates of job loss in the 1981–1982 and 1991–1992 periods, she found that the industrial and occupational mix of job loss changed over this period. There was an decreased incidence of job loss among blue-collar workers and workers in manufacturing industries and an increase in job loss among white-collar workers and workers in non-manufacturing industries.

In more recent work (Farber, 1997a, 1998b), I used the seven DWSs from 1984 to 1996 to examine incidence of job loss from 1981 to 1995. I found that rates of job loss followed the expected counter-cyclical pattern through the early 1990s. Job loss rates were high during the slack labor market of the early 1980s, subsequently decreasing during the expansion later in the decade. Job loss rates than increased during the slack labor market of the late 1980s and early 1990s. However, job loss rates did not decline and probably increased in the 1990s despite the strong labor market recovery after the slackness early in the decade. Fig. 1 contains plots of adjusted 3-year job loss rates computed from each of the seven DWSs from 1984 to 1996.\textsuperscript{15} These stacked-bar graphs provide information on not only on overall job loss rates (the total height of each bar) but also on job loss rates by reason (the shaded segments of each bar). Four classifications of reason are presented: (1) plant closing; (2) slack work; (3) position or shift abolished; and (4) other.\textsuperscript{16}

There are some interesting patterns in these graphs. First, the rate of job loss due to plant closings seems to have been relatively constant with a smaller secular decline over time. In contrast, the rate of job loss due to slack work seems to have a larger cyclical component combined with a smaller secular increase over time. One possible interpretation of this

\textsuperscript{14} Valletta (1997) presents an analysis of data from the PSID which suggests that while the probability of job change for men has not increased, the negative relationship of the probability of job change with tenure has weakened over time.

\textsuperscript{15} This figure is taken from Farber (1997a). Details of its construction are presented there.

\textsuperscript{16} The “other” category I use merges the “seasonal job ended”, “self-employment ended”, and “other” categories as coded in the DWS.
result is that plant closings are a response to secular declines in demand for specific
products while job loss due to slack work is a typical response to cyclical fluctuations
in demand where only marginal adjustments to output are required. Second, and more
interesting from the standpoint of a secular increase in instability, is that the rates of job
loss due to "position or shift abolished" and "other" were relatively constant through the
1989–1991 period but have risen substantially since then and account for all of the
increase in job loss in the 1990s.

In Farber (1998b), I present an analysis of what comprises the "other" category based
on debriefing questions appended to the February 1996 DWS, and it appears that much of
the job loss classified as "other" is, in fact, voluntary job change. On this basis, Fig. 2
contains rates of job loss over time with a liberal discount applied to the other category in
all years. These adjusted rates of job loss are roughly stable since the 1989–1991 period.
Given the tightening labor markets after 1991, rates of job loss would have been expected
to decline. The fact that they did not do so may reflect some decrease in job stability.

Returning to the tabulations of tenure data from the CPS in Tables 1 and 2, the second
and third columns of the tables contain measures of the incidence of long-term employ-
ment separately by sex. There is an important contrast between males and females in the
incidence of long-term employment. Over the time period studied, the incidence of long-
term employment fell substantially for men (10-year jobs by about 10 percentage points
and 20-year jobs by about 7 percentage points). In contrast, the incidence of long-term

\[\text{See Farber (1998b) for details of the construction of Fig. 2. My analysis of the debriefing data yields the}
\]result that only 23.6% of the "other" job loss in the February 1996 DWS likely reflects involuntary job loss. Thus,
\[\text{Fig. 2 differs from Fig. 1 by discounting the "other" job loss in Fig. 1 by 76.4% in all years.}\]
employment grew slightly for women over the same period. This difference suggests that there has been a general decline in male long-term employment that is offset by the increasing fraction of women in more recent cohorts who have continuous labor-force participation over the life-cycle. Another related interpretation is that part of the decline in long-term employment for men is due to competition from women with greater attachment to the labor force.

The last four columns of Tables 1 and 2 break down the incidence of long-term employment by educational category. Two facts are clear from these statistics. First, there has been a decline in the incidence of long-term employment in all four educational categories, and the declines are of roughly similar magnitude. As noted earlier, this may reflect nothing more than the strong labor market of the mid-1990s. Rapidly expanding employment necessarily implies and increase in the share of workers with low levels of tenure. There is not sufficient evidence to conclude that there has been a secular decline in long-term employment. A second fact evident in the tables is that the relationship between education and the likelihood of long-term employment is non-monotonic. Workers with less than 12 years of education and workers with some college (13–15 years of education) are less likely to be in long-term employment relationships than are workers with either high school (workers with 12 years of education) or college graduates (workers with at least 16 years of education).

The extensive literature surveyed here and supported by the statistics in Tables 1 and 2 establish that long-term employment relationships have been and continue to be an important feature of the US labor market. It is also the case that long-term employment relationships are an even more important feature of labor markets in other developed countries. Gregg and Wadsworth (1998) present evidence for the United Kingdom supporting this
view. As in the United States, they find that the aggregate job tenure statistics have not changed very much over the past two decades but that there have been larger changes across subgroups with the incidence of long-term employment in the United Kingdom declining somewhat among men and increasing among women. Burgess (1998) presents a survey of evidence from ten countries (France, Germany, Holland, Italy, Japan, Poland, Spain, Sweden, the United Kingdom, and the United States) which shows that the fraction of workers in long-term jobs in the 1990s is higher than in the United States in virtually all of the other countries surveyed. This contrast between the United States and the other countries may be due to the relatively low level of government regulation of the employment relationship in the United States. In particular, the costs to employers of shedding workers is lower in the United States than in most other modern economies.

3.2. Most new jobs end early

While a substantial fraction of the workforce is in long-term employment relationships, most jobs last only a short time. This distinction reflects the different sampling bases when considering workers and when considering jobs. When individuals are sampled, the employment relationships of which they are part are those employment relationships that have survived, and these are disproportionately the longer jobs. Longitudinal data are required in order to investigate the durations of all jobs. Mincer and Jovanovic (1981) present evidence based on the National Longitudinal Surveys (NLS) of Young Men and Older Men which shows that over half of all workers in jobs with tenure less than 1 year change jobs within 2 years. My analysis of data on job durations from the more-recent National Longitudinal Survey of Youth (NLSY), finds that about one-third of all new full-time jobs end in the first 6 months, one-half of all new full-time jobs end in the first year, and two-thirds of new full-time jobs end within 2 years (Farber, 1994).

Fig. 3 contains an updated version of my product-limit estimate of the survivor function for full-time jobs (Farber, 1994). This figure uses data from the NLSY for the period 1979–1991. These estimates, because they rely on younger workers, may overstate the likelihood of new jobs ending early in the workforce as a whole. On the other hand, these data refer only to full-time jobs, and full-time jobs are of longer duration on average than are part-time jobs.

Another approach to establishing that most new jobs end early is to measure the fraction of the workforce in new jobs at any point in time. In some recent work (Farber, 1998a), I present evidence from the CPS which shows that over 28% of the workforce reports having been on their job for 1 year or less over the 1973–1993 period. Farber (1997c) presents evidence from the CPS which shows that over 18% of the workforce reports having been on their job for less than 1 year over the 1979–1996 period. More detailed

18 This is the usual problem of length bias in sampling duration data at a single point in time.
19 Farber (1994) used NLSY data covering the period from 1979 to 1988. The data underlying Fig. 3 cover the period to 1991, but they are otherwise as described in Farber (1994).
information is in Table 3, which contains tabulations from CPS supplements between 1979 and 1996 with information on tenure of the fraction of the workforce who report having been in their job for less than 1 year.

Table 3
Fraction of workforce with less than 1 year of tenure by sex and education

<table>
<thead>
<tr>
<th>Year</th>
<th>N</th>
<th>Overall</th>
<th>Male</th>
<th>Female</th>
<th>ED &lt; 12</th>
<th>ED = 12</th>
<th>ED 13–15</th>
<th>ED ≥ 16</th>
</tr>
</thead>
<tbody>
<tr>
<td>May79</td>
<td>22164</td>
<td>0.205</td>
<td>0.177</td>
<td>0.243</td>
<td>0.201</td>
<td>0.201</td>
<td>0.235</td>
<td>0.187</td>
</tr>
<tr>
<td>Jan83</td>
<td>52375</td>
<td>0.186</td>
<td>0.172</td>
<td>0.203</td>
<td>0.176</td>
<td>0.178</td>
<td>0.212</td>
<td>0.182</td>
</tr>
<tr>
<td>May83</td>
<td>23417</td>
<td>0.172</td>
<td>0.157</td>
<td>0.191</td>
<td>0.179</td>
<td>0.169</td>
<td>0.199</td>
<td>0.150</td>
</tr>
<tr>
<td>Jan87</td>
<td>54778</td>
<td>0.190</td>
<td>0.173</td>
<td>0.210</td>
<td>0.202</td>
<td>0.184</td>
<td>0.211</td>
<td>0.173</td>
</tr>
<tr>
<td>May88</td>
<td>22932</td>
<td>0.196</td>
<td>0.178</td>
<td>0.217</td>
<td>0.234</td>
<td>0.197</td>
<td>0.212</td>
<td>0.162</td>
</tr>
<tr>
<td>Jan91</td>
<td>51661</td>
<td>0.180</td>
<td>0.163</td>
<td>0.199</td>
<td>0.188</td>
<td>0.181</td>
<td>0.193</td>
<td>0.165</td>
</tr>
<tr>
<td>Apr93</td>
<td>22331</td>
<td>0.167</td>
<td>0.162</td>
<td>0.173</td>
<td>0.210</td>
<td>0.171</td>
<td>0.173</td>
<td>0.139</td>
</tr>
<tr>
<td>Feb96</td>
<td>42599</td>
<td>0.191</td>
<td>0.180</td>
<td>0.203</td>
<td>0.227</td>
<td>0.187</td>
<td>0.207</td>
<td>0.166</td>
</tr>
<tr>
<td>All</td>
<td>292257</td>
<td>0.186</td>
<td>0.171</td>
<td>0.204</td>
<td>0.200</td>
<td>0.183</td>
<td>0.204</td>
<td>0.167</td>
</tr>
</tbody>
</table>

Note: The numbers in the column labeled "N" are sample sizes. The numbers in the remaining columns are fractions of the relevant group with tenure less than 1 year based on weighted tabulations of data from the relevant supplements to the CPS. The numerator is the number of those employed who report working continuously for their current employer for less than 1 year. The denominator is the total number employed. All counts are weighted by the CPS final sampling weights. Unincorporated self-employed workers and workers with missing data on tenure are not included in the analysis. Workers 20 years of age and older are included. These tabulations are taken from Farber (1997c).
The first column of Table 3 contains the fraction of workers in new jobs in the workforce as a whole. This averages 18.6% over the period covered and moves cyclically in the sense that new-job rates are highest in tight labor markets (1979, 1987–1988, 1996). This cyclical nature of the new-job rate is not surprising given the fact that new hiring in an expansion implies that there will be an increase in employment at low-tenure levels. Additionally, to the extent that layoffs are concentrated among low-tenure workers and layoffs increase in slack labor markets, there will be a decrease in employment at low tenure levels (Abraham and Medoff, 1984; Farber, 1993). While it is difficult to separate secular changes from cyclical movements in a relatively short time series, there is no evidence of a systematic increase in the incidence of new jobs over the 1979–1996 period. The remaining columns of Table 3 contain the new-job rates broken down by sex and by education.

As noted in the previous section, the data from the Annual Income Supplement to the March CPS on the number of main jobs held in the prior year can also be used to derive a lower bound estimate of the fraction of individuals who changed jobs in the previous year (and hence are in new jobs). This estimate of the rate of job change is computed as the number of individuals reporting more than one job divided by the number of individuals reporting at least one job.

I calculated weighted rates of job change, as described in the preceding paragraph, over the 1975–1995 period using the 1976–1996 March CPSs. These are plotted in Fig. 4. The average value over the 21-year period is 15.3%. After taking account the downward bias noted above, this corresponds well to the tabulations in Table 3 of the fraction of workers who have been in their job less than 1 year. The latter fraction averaged 18.6% over the 1979–1996 period (though without observations every year). There is no trend apparent in Fig. 4, but there does appear to be cyclical movement. The fraction of job changers is higher in the strong labor markets of the late 1970, late 1980s, and mid-1990s than in the weak labor markets of the early 1980s and early 1990s.

Burgess (1998) presents a survey of comparable statistics measuring the fraction of workers who had been in their jobs for 1 year or less in the 1990s for the ten developed countries listed earlier. This fraction is substantial in most of the countries in all age categories, but it is among the highest in the United States. For example, Burgess reports that approximately 20% of employed males in the United States ages 26–45 were on their

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20 Standard errors for these fractions are on the order of 0.25% so that differences of 0.7% are statistically significant at the 0.05 level.

21 Stewart (1997) has used these data in combination with data on industry and occupation both on the main job last year and at the survey date as part of a calculation of the rate at which individuals change jobs, where job change is meant to include change of jobs within employers (e.g., promotions, new task assignments). This is a broader concept of job change than the definition of employer change I am focusing on here. Stewart defines a job change as having occurred if the individual was employed during the reference week in March and met one of three conditions: (1) had more than one employer during the previous year; (2) experienced more than one spell of unemployment; or (3) worked for only one employer in the previous year but had a change in 3-digit industry or occupation codes between the main job last year and the main job held at the survey date.
jobs for 1 year or less. Comparable statistics for other countries are 20% in Holland, 11.4% in France, 16.3% in Germany, 8.2% in Italy, and 4.3% in Japan (the lowest).

Burgess's results show an interesting cross-national pattern when comparing the United States to the other nations surveyed. The United States has a relatively low (though still substantial) share of workers in long-term jobs and a relatively high share of workers in new jobs. As I noted above, this may be related to the low level of employment protection legislation in the United States compared to the other countries. The lack of institutional restrictions on hiring and firing of workers could contribute to the higher level of labor mobility in the United States implicit in these statistics.

The contrasts between the United States and other countries in worker mobility likely reflect a broader set of differences. More generally, it has been argued that the lack of institutional restrictions on hiring and firing in the United States has contributed to the relatively low unemployment rate and relatively high rate of net employment growth in the United States. This might also contribute to the relatively low level of employer and worker investment in on-the-job training in the United States relative to many European countries. Or it might be that the cross-national differences in institutions might be responses to different labor market structures. For example, the relatively high penalties for worker mobility in Germany might be a result of a labor market structure designed to encourage investment by both workers and firms in substantial amounts of on-the-job training.

The goal of this subsection was to demonstrate the fact that most jobs are short. I supplemented the direct evidence on this question from longitudinal data with indirect
evidence based on the fraction of workers in new jobs that are 1 year old or less. But finding a large fraction of workers in new jobs is not sufficient to conclude that most jobs are short. New jobs start whenever (1) new entrants to the labor force take jobs, (2) workers who previously exited employment (either to unemployment or to out-of-labor-force status) are reemployed or (3) workers change jobs. First, employment growth rates in the United States have averaged about 2% per year since 1979 so that new entrants cannot have made up even close to 20% of the labor force over the last 20 years. Second, while there may be substantial gross flows of workers among employment, unemployment, and out of the labor force, the flows out of employment must be from workers who are early in their jobs in order to maintain a substantial number of workers in long jobs. There cannot be enough long jobs generated on a sustained basis in the economy if they are ending at a rate sufficient to generate 15–20% new jobs. Thus, new jobs are primarily the result of job-to-job transitions, sometimes with intervening spells of non-employment, by workers in short jobs. This implies that new jobs are ending at a high rate and that most new employment relationships are short lived.

3.3. The probability of job change declines with tenure

The fact that the probability of job change declines with tenure is in important ways implied by the stylized facts that (1) long-term employment relationships are common and (2) most jobs end early. The fact that most jobs end early implies high probabilities of job change at low tenure levels. But long-term jobs can only develop if the probability of job loss moderates as tenure accumulates.

There are many studies that find a decline in the probability of job change with tenure, and I mention only a handful here. Parsons (1972) presents evidence based on industry-level data from the Employment and Earnings series of the BLS that both quit rates and layoff rates are strongly inversely related to tenure. Specifically, workers with less than 6 months tenure had higher quit rates and layoff rates than did workers with more than 6 months tenure. Hall (1972) presents evidence for older men using the NLS. He too finds that the probability of both quits and layoffs declines sharply with tenure. These findings are corroborated by Blau and Kahn (1981a,b) using the NLS of young men and young women and by Mincer and Jovanovic (1981) using the NLS of young men and mature men and the PSID. Abraham and Farber (1987) estimate a Weibull hazard model of the probability of job change using the PSID, and they find that the hazard declines sharply with tenure. McLaughlin (1991) also finds that job separation rates decline sharply with tenure in the PSID. My own work (Farber, 1993), using the DWS, finds that rates of job loss decline sharply with tenure.

Virtually all of the literature uses annual data on job change to investigate the relationship between tenure and the probability of job change, and, without exception, finds a

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22 It is unclear precisely how large are these gross flows because small amounts of measurement error in reporting labor force status can create large spurious gross flows of workers across labor force states. See Abowd and Zellner (1985) and Poterba and Summers (1986) for careful analyses of this problem.
monotonic negative relationship. McCall (1990) and Farber (1994) use the fine-grained information available in the NLSY on dates of job starting and ending to investigate the relationship between tenure and job change at shorter time intervals, and the results differ importantly. Specifically, the hazard of job ending computed at higher frequencies increases with tenure early in jobs before beginning a long-term decline.

I present an updated summary of my results using NLSY data from 1979 to 1991 in Fig. 5. This figure contains empirical hazard functions for job ending at four frequencies using a sample of 19,336 full-time jobs for 4680 individuals from the NLSY. The four panels in the figure contain the hazard computed at four frequencies: annual, quarterly, monthly, and weekly. The upper-left panel contains the annual hazard function. Consistent with the earlier literature, this hazard is monotonically declining in tenure and shows the 0.5 hazard in the first year that is apparent from the survivor function in Fig. 3. The hazard falls monotonically to less than 0.1 by year 12. The upper-right panel contains the quarterly hazard function. This hazard is also monotonically declining. The decline is very sharp in the first year, with the hazard falling from greater than 0.2 in the first quarter to about 0.1 by the fourth quarter. Both the annual and quarterly hazards are monotonically declining, and it is evidence on the hazards at roughly these frequencies that has supported the fact that the probability of job change is monotonically declining with tenure.

A different picture emerges when the hazard is computed at higher frequencies. The lower-left panel of Fig. 5 contains the monthly hazard function. What is most striking about the hazard function in Fig. 5 is that the hazard is actually relatively low in the first
month at 0.06, rising to a peak of almost 0.10 at 3 months and declining sharply thereafter before leveling off at less than 0.02. The lower-right panel of Fig. 5 contains the weekly hazard function for the first 26 weeks on jobs. This hazard shows an increase from a low of less than 0.01 in the first week to a peak of about 0.025 in ninth week before declining to about 0.012 by week 19. These high-frequency hazards modify the standard finding about the relationship between tenure and the probability of job change. In fact, the probability of job change increases with tenure very early in jobs (less than 3 months) and declines subsequently. I return to this below when discussing theories of job change.

3.4. What accounts for these facts?

The central facts regarding worker mobility in modern labor markets are clear. To recapitulate: (1) long-term employment relationships are common; (2) most new jobs end early; and (3) the probability of job change declines with tenure (perhaps after increasing during the first few months of employment). I turn now to the task of putting these facts in some theoretical perspective. I begin by discussing the implications for mobility of standard models of accumulation of specific human capital. While this model fits the facts quite well, I go on to argue that the facts could also be accounted for by unmeasured worker heterogeneity in underlying probabilities of job change. I then discuss and evaluate some tests of the specific capital model with the goal of distinguishing the role of specific capital from the role of heterogeneity in determining rates of mobility.

4. Why are there long-term employment relationships: the role of specific capital

The existence of employer-specific human capital is an obvious explanation for the existence of long-term employment relationships. To the extent that there is something valuable in the particular match between the worker and the firm that has no value to either the worker or the firm outside their relationship, the worker’s productivity in the firm will be both higher than the worker’s productivity elsewhere and higher than the productivity of another worker if hired by the firm. This match-specific capital can be the result of investment in firm-specific skills that inhere in the worker. More generally, it could be the result of any non-recoverable expenditure in the employment relationship that has no value outside the employment relationship. Obvious examples are the fixed costs of searching for the worker and/or job and of hiring the worker. These may not enhance productivity directly, but they are costs that are borne initially, have no value elsewhere, and must be borne again in the formation of any new employment relationship. Another example is the training of the worker in particular skills that have no value elsewhere but

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23 McCall (1990) notes the same peak in the hazard using the NLSY data.
24 There is some possibility that the measured increase in the hazard early in jobs results from measurement error. If survey respondents are less likely to report holding jobs that ended very early, then the hazard computed very early in jobs will be attenuated. I discuss evaluate this possible explanation in Farber (1994).
25 See Becker (1964) for an early discussion of specific human capital.
are important to productivity within the firm. Specific capital can also take the form of information about the quality of the match between the worker and the firm (Jovanovic, 1979a).

Parsons (1972) presents a detailed model of specific capital accumulation and job change that suggests an inverse relationship between the likelihood of job change and the level of specific investment. Mortensen (1978) constructs a model of specific capital and job change that highlights both the important concepts and the inherent difficulty of generating clear testable predictions from models of specific capital. The core idea of these models is that the probability that one party or the other terminates an employment relationship depends on the value of that party's share of the specific capital inherent in the match. Jovanovic (1979b) combines specific capital and search models to analyze job separations and unemployment. I build on this work in presenting a simple illustrative model that captures the key concepts and problems in models of specific capital and turnover.

Denote the total value of the specific capital inherent in the match by $Y$. Consider the worker's decision first. Let $W_a$ represent the best alternative wage available to the worker. This is the market value of the bundle of general skills that the worker brings to the labor market. The wage paid to the worker by the current firm is

$$W = W_a + \lambda Y, \tag{1}$$

where $\lambda$ is the worker's share of the value of the specific capital. In the simplest world where there is complete information about worker productivity and no costs of mobility, the worker's best alternative wage will be $W_a$, and the worker will not quit as long as the firm pays the worker even a small amount more than $W_a$ ($\lambda > 0$).

In order to generate quits in this model, some randomness in the alternative wage needs to be introduced. Burdett (1978) presents a model of quits relying on job search by employed workers. A simplified version of this model has a wage offer, $W_o$, arriving each period drawn from some wage offer distribution with mean $W_a$. These search models are often vague regarding what generates the variation in wages that underlies the wage offer distribution, but one potential explanation is that there is ex ante observable variation in worker-firm match quality. This simple search model generates some testable implications that I discuss later.

Returning to the model of specific capital and job change, note that the wage offer can be expressed as $W_o = W_a + \theta$ where $\theta$ is a random variable with mean zero. A worker quits if the wage offer ($W_o$) exceeds the current wage ($W$), which, by the definitions of $W_o$ and $W$, implies a condition for quitting of

$$W_a + \theta > W_a + \lambda Y. \tag{2}$$

Simplification implies that a worker quits if

\[ W_a + \theta > W_a + \lambda Y. \]
Clearly, the larger the value of the worker’s share of the specific capital, the less likely it is that the wage offer will exceed the current wage and the less likely it is that the worker will quit.

An analogous model can be developed, with some relabeling of variables, regarding the firm’s decision whether to replace the worker (a layoff). In the simplest case, the firm must pay the equilibrium alternative wage, \( W_a \), to hire a replacement worker. The value to the firm of what the current worker produces is

\[
V = W_a + Y, \tag{4}
\]

which is the value of marginal product of an hour of this worker’s labor. The marginal cost of the hour is the wage as defined in Eq. (1). The firm will lay off the worker if and only if the wage exceeds the value of marginal product. Combining Eqs. (1) and (4), the layoff condition is \( \lambda > 1 \). In other words, the firm will lay off the worker only if the firm has to give the worker more than the specific value of the match. The profit the firm earns is

\[
\pi = V - W = (1 - \lambda)Y. \tag{5}
\]

As in the model of quits, some randomness is required in order to generate layoffs. While this could be due to randomness in the wage the firm needs to pay a replacement worker, a source of randomness consistent with the macroeconomic literature on employment fluctuations is based on demand or productivity shocks. Think of these shocks as being firm or sector specific. Suppose that there is a shock to the value to the firm’s output such that

\[
V = W_a + Y + \phi, \tag{6}
\]

where \( \phi \) is a random variable with mean zero. The profit the firm earns is

\[
\pi = V - W = (1 - \lambda)Y + \phi. \tag{7}
\]

The firm will lay off the worker if profit is negative, which occurs if

\[
\phi < -(1 - \lambda)Y. \tag{8}
\]

In other words, the worker will be laid off only if the shock is sufficiently negative to outweigh the value the firm’s share of the specific capital.\(^{27}\) Obviously, the larger the value of the firm’s share of the specific capital, the less likely it is that this condition will be met.

4.1. Efficient separations with specific capital

The specific capital model makes a clear statement about when separations are economically efficient and when they are not. It is efficient for an employment relationship to end if and only if the worker’s opportunity wage (implicitly equal to the worker’s marginal value

\(^{27}\) This is a key insight of Oi’s (1962) early work on specific capital.
product in the best alternative) is higher than the worker’s marginal value product within the firm. In terms of the model, the efficient separation condition is that \( W_0 > V \). Using the definitions of \( W_0 \) and \( V \) yields the efficient separation condition

\[
\theta - \phi > Y. \quad (9)
\]

In other words, an efficient separation occurs when, taken together, the random component of the draw from the distribution of wage offers (\( \theta \)) is sufficiently large and the demand or productivity shock (\( \phi \)) is sufficiently negative to offset the value of the specific capital (\( Y \)). Hall and Lazear (1984) present a clear analysis which shows that an ex ante fixed sharing rule of the type defined above will lead to excess separations. My analysis here is in the spirit of their model. In order to highlight the key points, I consider the quit and layoff decisions separately assuming that the other cannot occur. First consider quits. A worker will quit whenever \( \theta > \lambda Y \), where \( \lambda \) is the worker’s share of the value of the specific value of the match. Given some demand or productivity shock, \( \phi \), that does not result in layoff, the quit will be inefficient whenever \( \theta < Y + \phi \). Thus, inefficient quits happen whenever \( \lambda Y < \theta < Y + \phi \). This is because the worker does not consider the firm’s share of the value of the specific capital \((1 - \lambda)Y\) in making the quit decision. Similarly, there will be excess layoffs. The firm will lay off the workers whenever \( \phi < -(1 - \lambda)Y \). Given some value of \( \theta \) that does not result in a quit, the layoff will be inefficient whenever \( \phi > \theta - Y \). Thus, there will be inefficient layoffs whenever \( \theta - Y < \phi < -(1 - \lambda)Y \). This is because the firm does not consider the worker’s share of the value of the specific capital \((\lambda Y)\) in making the layoff decision.

The fixed sharing rule model implies that the quit rate will be inversely related to the value of the specific capital received by the worker and that the layoff rate will be inversely related to the value of the specific capital received by the firm. The first-best is achieved only if each side receives all of the specific value of the match, which, of course, is not possible. This is the standard problem in agency models of providing first-best incentives where there are other conflicting goals (e.g., costs of monitoring effort (Holmstrom, 1979) or the provision of insurance by a risk-neutral firm to risk-averse workers (Harris and Holmstrom, 1982). 28

An interesting question is why contracts with fixed sharing rules (fixed wages) seem to be the norm. It must be that these contracts provide other advantages. For example, a more complicated state-contingent contract would require detailed verifiable information about the state of product demand (\( \phi \)) or outside offers (\( \theta \)). Such information might be expensive or impossible to obtain. Alternatively, risk-aversion by workers, liquidity constraints, or problems of joint production may make it infeasible to sell the firm to the worker in order to internalize the problem. Finally, paying a piece rate requires the sort of verifiable information on demand or technology shocks that may not be feasible. A piece rate also

28 See Malcomson in this volume for a detailed discussion of contracting issues in the labor market that deals with agency problems and labor turnover.
has problems where output is the result of effort by groups of workers so that output cannot easily be attributed to individuals.

The specific human capital model is consistent with the major facts established in the previous section by providing an economic rationale for long-term employment relationships. To the extent that specific human capital accumulates with time on the job (tenure), the model implies that separation rates will start out high and decline with tenure. However, the simple model does not account for the initial increase in separation rates with tenure.

4.2. Match quality as specific capital

Jovanovic (1979a) presents a model of the employment relationship where the key feature is that the productivity of a particular worker-firm match varies and is not observable ex ante. The match quality is an experience good in that the quality of the match is revealed over time as tenure accumulates. More formally, think of output each period as a noisy signal of match quality. The worker’s and the firm’s common prior expectation about the quality of the match is updated each period based on the output signal. Each period both the worker and the firm have the option of ending the match and starting a new match (with the same ex ante expected value), but starting a new employment relationship has an explicit fixed cost attached. The model is closed by assuming that workers are paid their expected output in each period. In the notation of the model in the previous subsection, this is a fixed sharing rule where the worker receives the full value of the match-specific capital ($A = 1$). In this case, firms are indifferent to whether workers stay or leave, and all relevant decisions are quit decisions rather than layoff decisions. However, assigning all of the specific value of the match to the worker is arbitrary, and the implications of this sharing rule for such outcomes as wage growth, cannot be used to test the model.29

The more important assumption underlying Jovanovic’s model is that there is no randomness in the wage offer distribution or shocks to productivity or demand. While such considerations could be added to the model, all turnover is generated by the revelation of information about match quality. Nonetheless, the matching model generates several testable predictions regarding rates of job change.

The model yields a reservation match quality property where a worker quits if the updated expected match quality is lower than the reservation match quality. The separation rate is directly related to the reservation match quality which moves with tenure in a predictable way. Early in the match, the reservation match quality is low suggesting that separation rates start low. This is because there is option value in a new match (it might turn out to be very good). Uncertainty about the match quality is likely to be high early in the match, while quitting to take a new job is costly. Thus, a worker might stay despite some early signals of poor match quality because there remains a relatively high probability that match quality will turn out to be high. Over time, the reservation match quality

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29 McLaughlin (1991) presents an interesting model of efficient turnover based on matching considerations where the split of the specific value of the match affects whether separations are considered quits or layoffs.
increases as the variance of the updated beliefs about match quality falls and the option value decreases. At this point, separation rates increase. The bad matches are weeded out, and the remaining matches are high quality matches with low separation rates.

Match quality is a form of specific capital. Thus, like the specific capital model generally, the matching model accounts for the facts in general terms. However, the matching model does have some compelling features with regard to the data that the basic specific capital model lacks. First, the model accounts for the very high rate of job separation in the first year. Presumably, information about match quality is generally revealed quite early in jobs, and bad matches, therefore, will end relatively quickly once the low quality is established. In contrast, specific capital in the form of acquisition of firm-specific skills might accumulate more slowly and continuously. Second, the matching model provides an explanation for the early spike in the job-ending hazard noted in Fig. 5. The job-ending hazard in the matching model is low at the very beginning, reflecting relatively high reservation wages due to the remaining option value in the match. The hazard increases with tenure as the reservation match quality increases due to the reduction in option value in the match as the quality of the match is determined more precisely over time. The matches with a low expected quality end due to lack of a reasonable expectation that the match might, in fact, be a high-quality. The hazard subsequently declines as primarily high-quality matches remain.

5. Can heterogeneity account for the facts?

In this section, I abstract from structural variation in the probability of job change with tenure for a given worker (true duration dependence in the hazard of job ending, perhaps due to presence of specific capital) in order to focus on heterogeneity in mobility rates across workers. This is important for at least two reasons. First, heterogeneity in mobility rates across workers has the potential to provide a parsimonious alternative explanation for the stylized facts regarding mobility. Second, consistent estimates of the role of duration dependence in the probability of job change cannot be investigated without controlling for heterogeneity (e.g., Lancaster, 1979; Heckman and Singer, 1984).

In order to focus the discussion, consider a pure heterogeneity model with no duration dependence. Duration dependence as used here refers to a structural relationship between tenure and mobility of the sort implied by the specific human capital model.30 A simple generalization of the pure mover-stayer model due to Blumen et al. (1955) where there are two types of workers (high mobility and low mobility) serves to illustrate the important points. The analysis generalizes straightforwardly to \( k \) types with an arbitrary distribution of types. The two types of workers are differentiated only by their turnover probabilities, \( \lambda_1 \) and \( \lambda_2 \). Type 1 workers are relatively more mobile so that \( \lambda_1 > \lambda_2 \), and these turnover probabilities are fixed over time for each worker. The proportion of the population that is of type 1 is \( \theta \). The overall turnover rate at any point in time is simply the \( \theta \)-weighted

30 This analysis follows that of Farber (1984).
average of the individual turnover probabilities,

\[ P = \theta \lambda_1 + (1 - \theta) \lambda_2. \] (10)

This very simple model of mobility can account for the fact that long-term employment relationships are common. As long as there is a reasonable percentage of low-mobility workers and the low mobility workers are reasonably immobile, there will be a substantial percentage of jobs that last a long time. As a simple example, consider a fixed population of two types of workers as described above who all start jobs at a particular date. The probability that a job lasts at least as long as some arbitrary length \( t \) is

\[ P(t \geq t_i) = \theta (1 - \lambda_1)^t + (1 - \theta)(1 - \lambda_2)^t, \] (11)

where \( t \) represents tenure. Clearly this probability is positively related to the share of low-mobility workers \( 1 - \theta \) and negatively related to the probability of mobility of both types of workers \( \lambda_1 \) and \( \lambda_2 \).

The fact that most new jobs end early, can be accounted for by having a sufficiently large percentage of high-mobility (type 1) workers with sufficiently large probabilities of mobility. Using the same simple example, the probability that a job lasts less than some arbitrary length \( t_i \) is

\[ P(t < t_i) = 1 - \theta (1 - \lambda_1)^t - (1 - \theta)(1 - \lambda_2)^t. \] (12)

This probability is positively related to the share of high-mobility workers \( \theta \) and to the probability of mobility of the both types of workers \( \lambda_1 \) and \( \lambda_2 \).

The fact that the probability of job change declines with tenure requires a bit more work to establish. The intuition is straightforward, however. Type 1 (high mobility) workers are relatively unlikely to have accumulated substantial tenure. Thus, the population of workers with substantial tenure is disproportionately composed of type 2 (low mobility workers). The average mobility rate among these workers will be lower as a result.

More formally, consider the subgroup of the underlying population with \( t \) years of tenure. The probability, \( \theta_t \), that a worker with \( t \) years of tenure is type 1 (high mobility) is

\[ \theta_t = \frac{(1 - \lambda_1)^t \theta}{(1 - \lambda_1)^t \theta + (1 - \lambda_2)^t (1 - \theta)}. \] (13)

By definition, the probability that a worker with \( t \) years of tenure changes jobs is

\[ P_t = \theta_t \lambda_1 + (1 - \theta_t) \lambda_2, \] (14)

It is straightforward to show that \( \theta_t \) is decreasing in \( t \) so that as tenure increases the distribution of workers becomes more heavily weighted toward low mobility (type 2) workers and the probability of mobility declines.\(^{31}\)

The key to understanding the heterogeneity model is that with a fixed population the

\(^{31}\) There are three trivial cases where there is no heterogeneity (\( \lambda_1 = \lambda_2, \theta = 0, \) or \( \theta = 1 \)). In these cases \( \theta \), does not change with \( t \).
An overall mobility rate is constant over time. Any differences in mobility rates with tenure are due to sorting of the population into different tenure groups based on their underlying mobility. This implies that all that matters for the probabilities of turnover conditional on previous turnover history in this model is the number of prior periods with job changes (c) and the number of prior periods without job changes \((n - c)\). The order in which prior turnover took place is irrelevant. The probability, \(\theta_{nc}\), that a worker with c changes in n years is type 1 (high mobility) is

\[
\theta_{nc} = \frac{\lambda_1^c (1 - \lambda_1)^{n-c} \theta}{\lambda_1^c (1 - \lambda_1)^{n-c} \theta + \lambda_2 (1 - \lambda_2)^{n-c} (1 - \theta)}.
\]

On this basis, the probability that a worker with c changes in n years changes jobs is

\[
P_{nc} = \theta_{nc} \lambda_1 + (1 - \theta_{nc}) \lambda_2.
\]

It is clear that \(\theta_{nc}\) and hence \(P_{nc}\), is increasing in \(c\) holding \(n\) fixed and decreasing in \(n\) holding \(c\) fixed. In other words, considering workers with a given amount of experience \((n)\), the group of workers with more changes is disproportionately composed of high mobility (type 1) workers. This implies lower turnover probability among groups of workers with fewer job changes in a given period of time. High tenure, per se, is not related to mobility beyond indicating fewer job changes. The number of job changes in the worker's job history is a sufficient statistic for the probability of job change in the next period.

6. Distinguishing heterogeneity and duration dependence: using data on mobility histories

The discussion of the heterogeneity model in the previous section provides some scope for using data on mobility rates for testing for the importance of both heterogeneity and "true" duration dependence of the sort implied by models of specific capital. The very simple heterogeneity model of mobility developed in the previous section has several clear empirical implications in addition to the core facts that are implied by the specific capital model as well.

First, the heterogeneity model with fixed worker types implies that the probability of job change for any worker is strictly a function of his or her type. Thus, the average rate of job change is constant over time for any fixed sample of workers. In other words, the fraction of workers in a fixed sample who change jobs in any period should not vary with the age or labor market experience of the sample. Second, the heterogeneity model implies that, controlling for experience, the number of prior job changes is a sufficient statistic for the probability of job change. Controlling for experience and the number of prior job changes, the probability of job change should not be related to tenure.

The existing empirical evidence suggests that simple heterogeneity in mobility rates, while important, cannot account for all of the observed relationship between the probability of job change and tenure. Fig. 6 contains the monthly probability of job ending by
years of labor market experience using data on 19,336 jobs for 4680 workers from the NLSY for 1979–1991. It is clear that the probability of job change declines sharply with labor market experience. Note that this is not a statement about a structural relationship controlling for tenure or other characteristics. It is a statement about the unconditional bivariate relationship between rates of job change and labor market experience.

One interpretation of the decline in the overall job ending hazard that is consistent with a more general heterogeneity model is that worker types are changing over time so that workers are becoming more stable (less mobile). This could be characterized in terms of the model as $\theta$ falling over time (a larger fraction of workers of the less-mobile type as experience increases) or as decreases in $\lambda$ with experience (workers of a given type becoming less mobile as experience increases). Of course, allowing this sort of post hoc rationalization makes it impossible to test a general (unrestricted) heterogeneity model.

Another interpretation of the negative experience-mobility relationship in Fig. 6 is that it is a result of a true negative relationship between tenure and job change. To the extent that this is the case, the simple heterogeneity model cannot explain all of the important features of the data. My earlier work (Farber, 1994), using data from 1979 to 1988 from the NLSY, presents an analysis that addresses this question. Specifically, I estimated a model of the hazard of job ending that controls for heterogeneity of the sort described above in the sense that prior turnover is controlled for directly. Separate hazard functions were estimated for each level of labor market experience (effectively controlling for $n$), and

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32 These data are an updated version of those described in detail in Farber (1994).
dummy variables for the number of prior jobs (effectively controlling for $c$) are included in each model along with controls for tenure and demographic characteristics. The results are clear. The probability of a job ending is strongly related to the number of prior jobs controlling for tenure on the current job. This suggests that heterogeneity is an important factor in mobility rates. At the same time, the probability of a job ending is strongly related to tenure even controlling for prior mobility. This is clear evidence that the simple heterogeneity model alone cannot account for mobility patterns and that specific capital may play an important role.

There is some evidence in the NLSY data that worker types are changing over time, leaving open the possibility that a richer model of heterogeneity may fit the data. Specifically, I found that recent prior mobility is more strongly related to the probability job ending than mobility further in the past (Farber, 1994). This is consistent with workers changing types of over time by becoming less mobile on average. This could reflect maturation of young workers as they acquire families and settle into careers.\(^{33}\) Investigating this evolutionary process is an important area for further research.

7. Testing the specific capital model: using the return to tenure

There are at least two formidable obstacles to testing the specific human capital model directly. First, specific capital is not generally directly measurable or even observable. Second, only separation rates can help to distinguish the specific human capital model from plausible alternatives, and, of course, predictions for separation rates are operational only if the quantity of specific capital can be observed. A natural candidate for another outcome would be the wage rate, but the wage rate is indeterminate where there is specific capital. Essentially, the firm and the worker are in a bilateral monopoly position with respect to their specific capital, and the wage will not be market-determined without additional assumptions. In this section, I address these two issues, demonstrate how they are related, and investigate avenues for testing the specific human capital model.

The measurement issue has not yet been solved in a satisfactory way. By default, tenure has been used to index the quantity of specific capital, and the relationship between tenure and wages is used as a measure of the return to specific capital. The root of this appears to be reasoning by analogy to the use of labor market experience to index general human capital (Mincer, 1974; Willis, 1986). The idea is that if time in the labor market indexes accumulation of general skills then time with the firm indexes accumulation of firm-specific skills. There are several flaws in this analogy.

Consider the standard model of accumulation of general human capital through on the job training. A worker is willing to pay the cost of these investments because they receive the return. These investments enhance the workers productivity at all (or many firms). If any employer is not willing to pay the worker for this productivity (the return on the

\(^{33}\) Osterman (1980) presents an interesting analysis of this process of transition.
general human capital), the worker will find an employer who will pay. Furthermore, optimal investment behavior on the part of the worker suggests an experience log-earnings profile that is increasing and concave in experience. Thus, a standard "bare-bones" specification for an earnings function is

\[ \ln W = \beta_0 + \beta_1 ED + \beta_2 EXP + \beta_3 EXP^2 + \epsilon, \]  

(17)

where \( \ln W \) is the log wage rate, \( ED \) represents years of education, and \( EXP \) represents years of labor market experience. The \( \beta \)s are parameters to be estimated, and \( \epsilon \) is a random error. The expectations are that \( \beta_1 > 0, \beta_2 > 0, \) and \( \beta_3 < 0, \) and these have been verified in countless empirical studies. The derivative of this function with respect to experience, \( \beta_2 + 2\beta_3 EXP, \) is called the return to experience, and it is commonly understood to reflect the return to the underlying quantity of general human capital acquired through post-schooling training.

Now extend this empirical model to include tenure. The resulting earnings function is

\[ \ln W = \beta_0 + \beta_1 ED + \beta_2 EXP + \beta_3 EXP^2 + \beta_4 TEN + \beta_5 TEN^2 + \epsilon, \]  

(18)

where \( TEN \) represents years of tenure with the current employer. The analogous expectations with regard to the signs of the parameters are that \( \beta_4 > 0 \) and \( \beta_5 < 0, \) and, indeed, this is what empirical analyses show quite clearly. The derivative of this function with respect to tenure, \( \beta_4 + 2\beta_5 TEN, \) is called the return to tenure, and it is analogously interpreted as the return to the underlying quantity of specific human capital acquired through post-schooling training provided by the current employer.

There are at least three problems with this seemingly logical extension. First, it is not clear that tenure indexes accumulated specific capital in a direct way. In the standard model of investment in general human capital over the life-cycle, investment is positive and decreasing over the working life (Ben-Porath, 1967). This is what implies that the experience-earnings profile is concave. While it may be the case that an analogous optimizing model of worker and firm behavior would imply that investment in specific capital will be positive and declining throughout the employment relationship, this is not assured. Thus, it may not be that the tenure-earnings profile is concave.

The second problem is that the theoretical link between the wage and marginal product that exists in the general human capital model does not exist with respect to specific capital. Because specific capital has no value outside the employment relationship, the employer need not pay the worker for the part of productivity in excess of the workers' productivity in his/her next best employment alternative. On the other hand, if workers are not rewarded for their specific capital they will be more likely to quit (resulting in the sort of inefficiencies noted above). Additionally, it is not clear who will pay for the investment in specific capital. Consider three alternatives: (1) the firm pays for all of the specific investment and keeps all of the return; (2) the worker pays for all of the specific investment and keeps all of the return; and (3) the worker and firm share the cost of the specific

\[ 34 \] See, for example, Ben-Porath (1967), Mincer (1974) or Rosen (1977).
investment and share the return. Using reasoning precisely analogous to the discussion of fixed-wage contracts above, all of these schemes lead to inefficiently high levels of turnover. Additionally, the tenure-earnings profile need not bear any resemblance to either the quantity of accumulated specific capital or to the productive value of any specific capital.

The third problem is that tenure is endogenous to the wage determination process. Tenure is an outcome that is simply the inverse of job change. Standard search models of labor turnover have the implication that workers with higher wages (relative to their general human capital) are less likely to quit (Mortensen, 1986; and see the chapter by Mortensen, this volume). And it has long been noted in the literature on specific capital that firms will use the wage to influence turnover behavior in the presence of specific capital (Pencavel, 1972; Parsons, 1973; Salop, 1973). Firms employing workers with substantial specific capital will devise compensation structures that discourage workers from quitting. Empirical analyses of quit behavior (e.g., Freeman, 1980) regress the probability of quitting on the wage and find a negative relationship between the probability of quits and the wage. Since tenure is the result of a series of (non)quit decisions, an earnings function with tenure as an explanatory variable can be thought of as an inverse quit function in some respects. Thus, it is arbitrary to assign the wage as the dependent variable that is "explained" by tenure.

This discussion makes it clear that tenure, like the wage, is an outcome of optimization by firms and workers, and, as such, it cannot be used as an independent measure of accumulated specific capital in a way that can be used to test hypotheses about the role or importance of specific capital. For example, most versions of the specific human capital model predict that workers with more specific capital will change jobs with lower probability than will workers with less specific capital. It is also observed that workers with more tenure have lower probabilities of job change. By assuming that more tenure indicates more specific capital, this appears to be a test of the prediction that specific human capital implies less job change. But high tenure is the result of low probabilities of job change, whether they result from specific human capital or have some other source. Thus, the relationship between tenure and rates of job change cannot, by itself, be a test of the specific human capital model.

Still, the view that "...the volume of specific training in an individual increases along with the duration of his employment in a given job..." (Pencavel, 1970, p. 12) is intuitively appealing. Workers with more tenure are likely to have more specific capital than workers with less tenure. And workers with substantial specific human capital may earn higher wages as they accumulate more tenure than do workers with less specific capital because the compensation structure that high specific capital firms have selected, perhaps to minimize costs of turnover, is one which has wages increase relatively rapidly. But this makes it clear that it is not appropriate to interpret the coefficients on tenure in a standard earnings function ($\beta_4$ and $\beta_5$ in Eq. (18)) as a return to tenure.

Nonetheless, the usual approach in the literature has been to ignore some of the issues of interpretation discussed here and to take the regression coefficients at face value as the return to tenure. A better interpretation is that the tenure coefficients as part of a hedonic
earnings function measure how earnings vary with tenure controlling for labor market experience and other factors. This can serve as a useful summary of firm-level compensation structures and perhaps could shed some light on how compensation structures are related to turnover probabilities. I return to this question of interpretation after a review of some recent literature focused on deriving estimates of the return to tenure.

7.1. Estimating the “return” to tenure

There is a substantial literature on estimating the return to tenure in the context of a standard earnings function. A typical OLS specification of Eq. (16) finds an average return to tenure of about 0.02 per year.\(^\text{35}\) However, more recent work has focused on the interpretation of OLS estimates of the return to tenure derived in this fashion, worrying particularly about biases due to heterogeneity in workers and jobs and to the endogeneity of tenure.\(^\text{36}\) Many of these studies find a much smaller return to tenure than do the early studies (e.g., Topel, 1986; Abraham and Farber, 1987; Altonji and Shakotko, 1987; Marshall and Zarkin, 1987; Williams, 1991; Altonji and Williams, 1997; Manning, 1997), but some continue to find a substantial return (Topel, 1991). The issues raised in this literature are central to estimation of the return to tenure and interpretation of such estimates as evidence for the evaluation of the model of accumulation of firm-specific capital.

Altonji and Shakotko (1987) argue that the estimated return to tenure is biased upward due to the fact that tenure is correlated with omitted individual, job, or match specific factors that are correlated with earnings. The basic idea is that more stable workers or workers in more stable jobs are likely to be more productive workers or on more productive jobs. On this basis, one could recast the earnings function in Eq. (18) as

\[
\ln W_{ijt} = \beta_0 + \beta_1 E_D_{ijt} + \beta_2 E_EXP_{ijt} + \beta_3 TEMPL_\{H_{ijt} = \gamma_i + \delta_j + \epsilon_{ijt},\}
\]

where I have omitted the second order terms in experience and tenure for expositional convenience and where \(i\) indexes individuals, \(j\) indexes jobs, and \(t\) indexes time. There are now individual specific and job specific error components (\(\gamma_i\) and \(\delta_j\) respectively), and tenure is likely to be positively correlated with both of these. The result will be an upward biased estimate of the return to tenure (\(\beta_3\)).

Altonji and Shakotko present an instrumental variables solution to this problem. They use data from the Panel Study of Income Dynamics (PSID) on earnings over time, which allows them to rely on within-job variation in tenure to estimate the rate of growth of wages within jobs. They find a return to tenure that are substantially smaller than those derived using OLS on the usual sort of cross-sectional data (close to 0 for the IV estimates versus 0.020 for the OLS estimates).

Abraham and Farber (1987) note the same problem with standard OLS estimates of the

\(^{35}\) Borjas (1981) and Mincer and Jovanovic (1981) are examples of studies with OLS estimates of the return to tenure.

\(^{36}\) Hutchens (1989) presents an overview of the early literature in this area.
return to tenure, but they cast it in slightly different terms and propose a different, though related, solution. They argue that the individual and job-specific error components in the earnings function are correlated with completed job duration and tenure is correlated with these error components only indirectly because tenure is correlated with completed job duration. Clearly, workers with high levels of tenure must be in long jobs while workers with low levels of tenure can be in either short jobs or long jobs. Abraham and Farber go on to argue that if completed job duration is included in the earnings function along with tenure then this new variable will eliminate the correlation of tenure with the error components and the result will be unbiased estimates of the return to tenure. They use data from the PSID to estimate the augmented earnings function,

\[ \ln W_{ij} = \beta_0 + \beta_1 ED_{ij} + \beta_2 EXP_{ij} + \beta_3 TEN_{ij} + \beta_4 DUR_{ij} + \epsilon_{ij}, \]

where \( DUR_{ij} \) represents completed job duration for worker \( i \) on job \( j \).\(^{37}\) Estimation of Eq. (20) yield much smaller estimates of the return to tenure than does OLS estimation of the same equation without the completed job duration variable (0.005 for the model with completed job duration versus 0.01–0.015 for the model without completed job duration).

Abraham and Farber also present an instrumental variable approach to this problem. They note that, in a cross-section, tenure is on average half of completed job duration. On this basis, they use the residual from a regression of tenure on completed job duration as an instrument for tenure. Since, by construction, this residual is orthogonal to completed job duration, it will be orthogonal to the error components in Eq. (19) that are at the root of the bias in the OLS estimates. These IV estimates of the return to tenure are very close to those derived by including completed job duration as a regressor as in Eq. (20).\(^{38}\)

While the Abraham and Farber approach to this problem is not a full structural solution since completed job duration is surely jointly determined with wages, the results are useful nonetheless. Perhaps more interesting than the reduction in the estimated return to tenure in Eq. (20) is the substantial and significant positive estimate of the coefficient of completed job duration (\( \beta_4 \)). Their estimate for \( \beta_4 \) of approximately 0.02 implies that each 10 years that a job will last implies approximately 20% higher earnings throughout the job. Abraham and Farber investigate this further by modifying the specification to allow completed job duration to have different effects on earnings at different tenure levels. The results of this less restrictive specification verify that completed job duration is correlated with higher earnings at all tenure levels (including very early in jobs).

Topel (1991) takes a different approach to the problem and argues that there is, in fact, a substantial return to tenure. He rightly worries about the endogeneity of tenure and presents results from a two-stage estimation procedure that yields a lower bound estimate of the return to tenure. Topel recasts the earnings function slightly as

\(^{37}\) Completed job durations are censored for many jobs in the sample. Abraham and Farber’s solution to this problem is to use a parametric job duration model to estimate expected completed job duration for the censored jobs.

\(^{38}\) Note that this IV approach and the resulting estimates of the return to tenure are very close to those of Altonji and Shakotko (1987).
\[ \ln W_{ijt} = \alpha_0 + \alpha_1 ED_{ijt} + \alpha_2 EXP_{ijt}^0 + \alpha_3 TEN_{ijt} + \epsilon_{ijt}, \]  

where \( \alpha \) represents the parameters of the model. The key difference between Topel’s specification and the earlier specification is that labor market experience (\( EXP_{ijt} \)) is replaced by labor market experience at the start of the job (\( EXP_{ijt}^0 \)). Since \( EXP_{ijt} = EXP_{ijt}^0 + TEN_{ijt} \), the return to tenure controlling for current labor market experience is \( \alpha_3 - \alpha_2 \). The return to initial experience is subtracted because \( \alpha_3 \) is an estimate of total wage growth within jobs and reflects wage growth due to the accumulation of experience as well as of tenure. Topel proceeds by deriving what he argues is an unbiased estimate of \( \alpha_3 \) and a potentially upward biased estimate of \( \alpha_2 \). The resulting difference is then a lower bound estimate of the return to tenure.

Topel uses average within-job wage growth of workers who do not change jobs as an unbiased estimate of \( \alpha_3 \). The underlying assumption is that the wage growth of stayers is an unbiased estimate of wage growth for all workers had they not changed jobs. This does not allow the possibility that workers change jobs for reasons related to wage growth (or the lack thereof). Topel’s justification is that earnings move with a random walk after removal of trend growth so that, after allowing for trend growth, the change in earnings this period is unrelated to the change in earnings last period. With this estimate of \( \alpha_3 \) in hand Topel runs the following regression:

\[ \ln W_{ijt} = \hat{\alpha}_3 TEN_{ijt} = \alpha_0 + \alpha_1 ED_{ijt} + \alpha_2 EXP_{ijt}^0 + \epsilon_{ijt}, \]  


to derive his estimate of the return to experience (\( \alpha_2 \)). Topel argues that \( \hat{\alpha}_2 \) is upward biased on the basis of a very simple search model of job change over the working life (Burdett, 1978; Topel, 1986; Topel and Ward, 1992; Manning, 1997). In this model, workers face a stable wage offer distribution and offers arrive exogenously at some rate. Workers change jobs if the wage offer exceeds their current wage. Thus, wages at the start of jobs grow with experience for reasons having nothing to do with the accumulation of general human capital. On this basis, the estimate of the return to experience in a standard earnings function is an upward-biased estimate of earnings growth due to human capital accumulation. Topel’s two-step procedure yields a lower bound estimate of the return to tenure that is about the same magnitude as the standard OLS estimates (about 0.025 for the two-step estimate versus 0.03 for the OLS estimates), and the conclusion is that there is a substantial return to tenure.

Altonji and Williams (1997) revisit the question of the return to tenure, investigating in detail the earlier work described above, particularly with regard to issues of timing, measurement, and specification. They attempt to reconcile the earlier Altonji-Shakotko

39 Topel presents evidence from the PSID that supports this assumption, but it is not clear how powerful his test or how much deviation from the random walk assumption would introduce substantial bias. Farber and Gibbons (1996) present evidence from the NLSY rejecting the hypothesis that wages evolve as a martingale (a generalization of the random walk).

40 Topel estimates a non-linear relationship between log-earnings and tenure, and the estimates I cite here are computed at 10 years of tenure.
results with Topel's results. Their finding is that Topel's treatment of secular trends in wages and his use of lagged wages with a current measure of tenure results in upward bias in the return to tenure in his sample. Altonji and Williams also consider the role of measurement error in tenure. They conclude from their analysis that the return to tenure is relatively small (about 1% per year) and close to that estimated by Altonji and Shakotko and by Abraham and Farber.

How are these estimates of the return to tenure to be interpreted, regardless of their precise magnitude? I argue that an appropriate estimate that accounts for the sorts of biases described above measures an average rate of wage growth within jobs. This is best understood as measuring the compensation structures used by employers to achieve appropriate performance from their workforce. An important aspect of this performance is discouraging turnover where this is substantial investment in specific capital. In a well-known paper, Lazear (1979) develops a model of the compensation structure where employers who value long-term employment relationships, presumably because they want to invest in specific capital in their workers, offer a back-loaded compensation structure so that workers will want to remain with the firm.\footnote{Lazear (1998) goes on to argue that firms then need a mechanism to end the employment relationship since workers who are highly paid at the end of the implicit long-term relationship will not want to leave voluntarily. Mandatory retirement rules can play this role, and the outlawing of mandatory retirement in the United States may have unintended negative consequences for economic efficiency.}

In other words, these firms offer steeply-sloped compensation profiles with a high return to tenure\footnote{Carmichael (1989) presents a survey of the theoretical literature on life-cycle incentive issues in labor markets. Felli and Harris (1996) present a dynamic theoretical model of specific capital that incorporates issues of information, matching, and turnover and which has implications for the slope of the tenure-earnings profile.}

What this discussion suggests is that the relationship between tenure and earnings is not a market-determined constant but is likely to vary across firms with different technologies and in different markets. Firms that invest heavily in workers and want stable long-term workers may use a steeply sloped earning profile as an incentive device. They may also pay higher wages throughout and offer substantial fringe benefits. Firms that are less concerned about having a stable workforce are likely to offer flatter earnings profiles. An important direction for future research will be to investigate variation in the tenure earnings profile and relate it to the underlying economic forces that cause firms to make different decisions regarding their compensation structure.\footnote{Abowd et al. (1994) report significant differences in slopes of tenure-earnings profiles across a large sample of French firms. Margolis (1995, 1996), using the same data, analyzes the return to tenure in the context of models that allow for heterogeneity in tenure slopes across firms. Margolis confirms that there is substantial heterogeneity in tenure slopes, and he concludes that accounting for self-selection of workers into firms on this basis is important in estimating the return to tenure.} But there is no sense in which evidence of this sort on the tenure-earnings profile can provide direct evidence on the importance of specific human capital and its relationship with turnover.
8. Testing the specific capital model: evidence from displaced workers

Another approach to testing the specific capital model relies on examining the incidence of job loss and wage dynamics of workers who change jobs for reasons exogenous to their own decisions or the decisions of employers with regard to their wages or performance. The idea is that jobs are lost exogenously due to shocks to demand that cause firms to shed workers through plant closings in the extreme case and through layoffs in the less extreme case. Technology shocks can have much the same effect. Of course, the likelihood that workers will be displaced in this way is surely related to wages and productivity on the margin, but the shocks themselves can reasonably be considered exogenous.

In order to make this approach work, tenure must be accepted as being monotonically positively related to accumulated specific capital. Note that this is weaker than the relationship required to interpret the coefficient on tenure in an earnings function as a (rescaled) return to specific human capital. A second requirement is that the firm and worker share in the return to any accumulated specific capital. This seems reasonable in light of the necessary incentives to reduce labor turnover where there is specific capital.

Given these two conditions and if specific capital is an important component of employment relationships then the probability of job loss will decline with tenure. This is closely related to the stylized fact presented earlier that the probability of job change declines with tenure. Essentially, the claim is that when a firm needs to reduce employment due to a demand or technology shock, the firm will choose to lay off less senior workers because they embody less specific capital (on which the firm is enjoying some return). Using data from the 1984–1992 Displaced Workers Surveys and other CPS supplements with data on job tenure over the same period, I found support for this view (Farber, 1993). The probability of job loss declines sharply with tenure. Quantitatively, my calculations yield the result that the probability of job loss in the 2 years prior to the DWS survey date declines from about 14% for workers with less than 1 year of tenure to less than 4% for workers with ten or more years of tenure.

A weakness of this approach to testing for the importance of specific capital is that a finding of a negative relationship between tenure and the probability of job loss is susceptible to an explanation based on heterogeneity. If there is persistent heterogeneity across firms in the volatility of labor demand, then firms where there is little volatility will be more likely to have high-tenure workers and less likely to have substantial lay-offs. This implies the same negative relationship between tenure and job loss without specific capital. A related argument is that firms with substantial volatility in labor demand may find investment in specific capital less attractive and, hence, organize production around a high-turnover workforce.

This is analysis is complicated by the fact that the DWSs do not contain information on tenure for workers who did not lose their jobs, and even if there was such information it will not accurately reflect job tenure during the period the worker was at risk to lose a job if the worker changed jobs for reasons other than job loss. See Farber (1993) for details.
A second test of the specific capital model is that displaced workers earn less on their post-displacement jobs than on their pre-displacement jobs. There is a substantial empirical literature that examines this issue. The bulk of the recent work on this question is based on the DWS. 45 However, there has also been research using the PSID, and administrative data from the records of state unemployment systems (Jacobson et al., 1993). There is strong consistency in this literature. Regardless of the data used, the finding is that job loss results in substantial and persistent earnings loss. This can be interpreted as evidence that specific capital is important in the labor market. It may be that specific capital represents actual skills that are useful only with the current employer or that specific capital represents the value of a good match that is sacrificed by job loss (Jovanovic, 1979a). However, these findings could be accounted for by heterogeneous job quality where the high-wage jobs are allocated in ways unrelated to ability (perhaps randomly). Agency considerations of the sort that underlay efficiency wage models could account for the finding that high-wage job losers are not able to find equivalent new jobs.

Perhaps a more focused test of the specific capital model is that the wage decline borne by displaced workers will be larger for those displaced workers with more tenure. This relies on the assumption that the workers are receiving at least some of the return to the specific capital. When a worker is displaced, the value of any specific capital is destroyed, and the worker's wage on next job will be commensurately lower by an amount equal to the worker's share of the return on the specific capital on the lost job. Since workers with more tenure are presumed to have more specific capital, the wage loss of workers with more tenure will be larger than the wage loss of workers with less tenure.

There is empirical support for this prediction. Addison and Portugal (1989) present an analysis of data from the January 1984 DWS which finds that earnings losses are larger for workers with more pre-displacement tenure. My calculations, based on the DWSs from 1984 to 1996, show that the proportional difference in weekly earnings (post-displacement minus pre-displacement) for more than 18,000 re-employed displaced workers is strongly negatively related to pre-displacement tenure. Regressing the change in log real earnings on pre-displacement tenure yields a coefficient on tenure of −0.017 (SE = 0.0008). Controlling additionally for survey year, education, age, sex, race, and part-time status on the old and new jobs reduces the coefficient on tenure only slightly to −0.012 (SE = 0.0008).

One potential criticism of this analysis is that workers who lose jobs may be selected by their employer on the basis of poor performance or relatively high pay. Gibbons and Katz (1991) argue that workers displaced due to “slack work” are subject to this sort of selection. Within the limits of human resource management policies that give preference in retention to high tenure workers, employers are likely to lay off less productive workers when demand declines. In contrast, they argue that workers who are displaced due to a

“plant closing” are not subject to such selection. Employers must lay off all workers in such situations. This suggests that workers who report a “plant closing” as the cause of their job loss are more likely to be exogenous job losers than are those who are displaced due to “slack work”. This suggests that only a subset of displaced workers, those displaced due to “plant closing,” are appropriate to use in testing models of mobility. While Gibbons and Katz present evidence from the 1984 and 1986 DWSs showing that the wage loss of job losers due to “slack work” is larger than the wage loss of job losers due to “plant closing,” the differences are small. And my own analysis of data from the DWS suggests that the relationships of the wage loss with pre-displacement tenure are similar across job-loss categories.

However, there is another potential heterogeneity-based alternative explanation for the positive relationship between tenure and the wage loss of displaced workers. If jobs are heterogeneous so that some jobs pay higher wages than do others for equivalent workers, it is likely to be the case that average tenure is higher on the jobs that pay the higher wages due to a reduced probability of quits. In the extreme case where the likelihood of job losers being hired into a high-wage new job is independent of what type of job they held earlier, there will be a strong negative relationship between the wage change of job losers and tenure on the lost job. Both high- and low-tenure will receive the average wage in expectation on their new job. This implies a large decline in wages for high-tenure job losers and a smaller decline (or even an increase) in wages for low-tenure job losers controlling for observable characteristics.

There is a related prediction with regard to the relationship between tenure on the lost job and the wage level on the new job for job losers that can help establish the relative importance of specific capital and heterogeneity in these models. Kletzer (1989) notes that evidence that tenure on the lost job is related to the level of wages on the new job is evidence that heterogeneity is important. Clearly, if the only role of tenure on the lost job was to proxy for accumulated specific human capital, then it would have no effect on wages on the new job. But if workers are heterogeneous and more stable workers are more productive generally, then high tenure job losers (whose stable employment history is verifiable) will command higher wages in their new jobs. Kletzer, using data from the 1984 DWS, finds a positive but not statistically significant relationship between earnings on the post-displacement job and pre-displacement tenure. Addison and Portugal (1989) do find a significant positive relationship. My own calculations, based on a sample of more than 20,000 re-employed displaced workers from the 1984–1996 DWSs show a significant positive relationship. Regressing post-displacement log real earnings on pre-displacement tenure, controlling additionally for survey year, education, age, sex, race, and part-time status yields a significant coefficient on tenure of 0.0064 (SE = 0.0009). These estimates imply that 10 years of tenure on the lost job is associated with about 6% higher earnings on a new job. This, suggests that heterogeneity does play some role in the relationship between tenure and wages.

We can also examine the relationship between tenure on the lost job and earnings on the pre-displacement job. If job loss is exogenous, this is roughly equivalent to estimating a
standard earnings function including tenure, and it is not surprising that we find a strong positive relationship. My own calculations using data on a sample of more than 30,000 displaced workers from the 1984–1996 DWSs verify this strong relationship. Regressing pre-displacement log real earnings on pre-displacement tenure, controlling additionally for survey year, education, age, sex, race, and part-time status yields a significant coefficient on tenure of 0.021 (SE = 0.0006). These estimates imply that 10 years of tenure on the lost job is associated with about 21% higher earnings on the lost job.

A comparison of the magnitudes of the coefficients on tenure in these pre- and post-displacement earnings functions may shed some light on the relative importance of heterogeneity and specific capital considerations. A strong caution is that this approach is subject to the important problems, noted earlier, of interpretation of the coefficient of tenure in earnings functions as a return to specific capital. Start by interpreting the coefficient on pre-displacement tenure in the post-displacement earnings function as reflecting heterogeneity and interpreting the coefficient on tenure in the pre-displacement earnings function as reflecting the sum of heterogeneity and specific capital. Then the difference in coefficients represents the contribution of specific capital. Note that this is roughly equivalent to measuring the relationship between the change in earnings and tenure on the lost job. To the extent that this is negative, it implies that specific capital considerations are important. My estimates for the coefficients on tenure are 0.006 in the post-displacement earnings function and 0.021 in the pre-displacement earnings function. Taken at face value, the difference between these estimates suggests that about 30% of the estimated return to tenure in a cross-section is due to heterogeneity with the remaining 70% being due to specific capital.

Neal (1995) presents an analysis that extends this work in an interesting way. He investigates how pre-displacement tenure is related to post-displacement earnings separately for workers who change industries and for workers who do not change industries following displacement. Using data from the DWSs from 1984 to 1990, he finds that the level of post-displacement earnings is positively related to pre-displacement tenure only for workers who are re-employed in their pre-displacement industry. There is no relationship between tenure and post-displacement earnings for industry switchers. Parent (1995), in related work using the NLSY and the PSID, finds that industry-specific experience is a more important determinant of earnings than either total labor market experience or firm-specific tenure. Perhaps most interesting is the finding that the coefficient on tenure is reduced substantially when within-industry labor market experience is included in a standard earnings function.

These findings suggest an important reinterpretation of the earlier results on the rela-

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46 Kletzer (1989) and Addison and Portugal (1989) carry out this analysis, and both find a strong significant positive relationship.

47 The difference between these coefficients of -0.015 is very close to my estimated coefficient on tenure in the regression of the change in log-earnings cited above of -0.017. Addison and Portugal (1989) note this as well.
tionship between tenure and earnings. They imply that the capital that accrues with tenure has a strong industry-specific rather than firm-specific component. To the extent that this is the case, it is harder to argue that the accrual of firm-specific capital is what drives the decline in the probability of job change with tenure.

9. Final remarks

A core set of facts about worker mobility are clear: (1) long-term employment relationships are common; (2) most new jobs end early; and (3) the probability of a job ending declines with tenure. However, evidence in support of particular models that can explain these facts is relatively weak. While the specific capital model is a parsimonious explanation for these facts, it also appears that worker heterogeneity can account for much of what we observe in the mobility data.

The task of testing the specific human capital model is very difficult because specific capital is not observed directly and the wage need not reflect productivity where there is specific capital. Tenure is, at best, an imperfect proxy for accrued firm-specific capital and, at worst, another outcome jointly determined with wages.

It seems clear that high-wage workers change jobs less frequently than do low-wage workers implying that they have higher tenure. Thus, it is not surprising that tenure shows a strong positive coefficient in standard earnings functions. While this does not measure a return to specific capital, it does, when appropriately estimated to take account of heterogeneity, measure how firms structure compensation over the course of jobs. And the compensation structure may imply something interesting about the importance of specific capital.

Why is it that firms pay higher wages to some workers who will stay with the firm a long time, either as a cause or an effect of the high wages? Why is it that firms structure compensation profiles so that wages increase with tenure? It is likely because firms value and want to encourage long-term employment relationships. Since it is expensive for firms to encourage such relationships, it must be the case that these relationships are a more efficient production technology for them. The unanswered question is what attributes of long-term employment relationships make them the efficient production technology in many settings. One important advantage may be that long-term employment relationships enable firms and workers to invest in firm-specific capital. But alternative explanations remain. For example, it may be that worker quality may be difficult to observe, ex ante, so that firms that need high-quality workers want to retain workers who they learn are high quality in order to avoid the risk of hiring a series of low-quality workers before finding another high-quality worker. Or it may be that many workers prefer long-term stable employment relationships and are more willing to supply effort in such situations.

In conclusion, while deriving convincing direct evidence for the specific capital model of mobility is difficult, it appears that specific capital is a useful construct for understanding wage dynamics and worker mobility. Future progress in understanding these issues
will require more and better data on mobility histories along with models that combine specific capital considerations with carefully specified models of heterogeneity and other alternatives.

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