HAS JOB STABILITY DECLINED YET? NEW EVIDENCE FOR THE 1990'S

David Neumark
Daniel Polsky
Daniel Hansen

Working Paper **6330**

NBER WORKING PAPER SERIES

HAS JOB STABILITY DECLINED YET? NEW EVIDENCE FOR THE 1990'S

David Neumark
Daniel Polsky
Daniel Hansen

Working Paper 6330
http://www.nber.org/papers/w6330

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 1997

We thank Robert Mare, Peter Schmidt, Mark Schweitzer, and Jeff Wooldridge for helpful comments. Neumark gratefully acknowledges support from NIA grant number K01-AG00589 to the National Bureau of Economic Research. This paper was prepared for the Russell Sage Foundation's "Conference on Changes in Job Stability and Job Security," February 1998. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

Has Job Stability Declined Yet? New Evidence for the 1990's

David Neumark, Daniel Polsky and Daniel Hansen

NBER Working Paper No. 6330
December 1997
JEL Nos. C80, J21, J63

ABSTRACT

In earlier work we examined the temporal evolution of job stability in U.S. labor markets through the 1980's, using data assembled from a sequence of Current Population Survey tenure supplements. We found little or no change in aggregate job stability in the U.S. economy. In addition, older and more-tenured workers experienced increases in job stability in the latter part of the 1980's.

In this paper we update the evidence on changes in job stability through the mid-1990's, using recently-released CPS data for 1995 that parallel the earlier job tenure supplements. Updating the evidence from systematic random samples of the population and workforce through this period is especially important because the media have painted a particularly stark picture of declining job stability in the 1990's.

In the aggregate, there is some evidence that job stability declined modestly in the first half of the 1990's. Moreover, the relatively small aggregate changes mask rather sharp declines in stability for workers with more than a few years of tenure. Nonetheless, the data available to this point do not support the conclusion that the downward shift in job stability for more-tenured workers, and the more modest decline in aggregate job stability, reflect long-term trends.

David Neumark
Department of Economics
Michigan State University
East Lansing, MI 48824
and NBER
dneumark@msu.edu

Daniel Polsky
Health Services Research
University of Pennsylvania
3615 Chestnut Street
Room 303
Philadelphia, PA 19104

Daniel Hansen
Laurits R. Christensen Associates
4610 University Avenue
Suite 700
Madison, WI 53705

I. Introduction

Until recently, little was known about how the stability of jobs has changed over time in the U.S. economy, the nature of the changes (if any), and which groups have been most affected. In the past few years, however, researchers have begun to assemble evidence on these questions. This research was spurred in part by a wave of corporate downsizings and accompanying media stories suggesting that workers "forget any idea of career-long employment with a big company" (Time, Nov. 22, 1993), and that "the notion of lifetime employment has come to seem as dated as soda jerks, or tail fins" (New York Times, March 8, 1996). However, a spate of widely-publicized downsizings does not necessarily imply that the nature of the employment relationship has really changed. The goal of much of the research on job stability is to ask whether, in fact, such changes are evident in labor market surveys based on nationally representative samples.

In earlier work (Diebold, et al., 1996 and 1997), we examined the temporal evolution of job stability in U.S. labor markets through the 1980's, using data assembled from a sequence of Current Population Survey (CPS) tenure supplements. In contrast to evidence reported by some other researchers, we found little or no change in aggregate job stability in the U.S. economy. Some groups of workers--in particular, the same lower-skill groups that suffered relative wage declines in the 1980's--experienced decreases in job stability in this same period. On the other hand, in contrast to perceptions reflected in the media, white, older, more-tenured, more-educated, white-collar workers experienced increases in job stability in the latter part of the 1980's.

While resolving the disagreements regarding changes in job stability through the 1980's is important from the perspective of measuring long-term trends, the goal of this paper is to update evidence on job stability through the mid-1990's, using recently-released CPS data for 1995 that parallel the earlier job tenure supplements. Updating the evidence from systematic random

samples of the population and workforce through this period is especially important because the media have painted a particularly stark picture of declining job stability in the 1990's, based largely on anecdotal evidence and surveys of subjective assessments regarding job security (see Neumark and Polsky, 1997).

In measuring job stability using the CPS, it is critical to ensure comparability of the data over time. Because the CPS survey eliciting information on tenure changed in 1995, in this paper we pay particular attention to the adjustments to the data to needed for comparability, and analyze the sensitivity of the results to alternative adjustment procedures in order to obtain a robust impression of the empirical evidence. Despite some relatively minor ambiguities, we believe that a relatively consistent picture emerges in the data for the 1990's, one that contrasts in some ways with our earlier findings for the 1980's.

In the aggregate, there is some evidence that job stability declined modestly in the first half of the 1990's. Moreover, the relatively small aggregate changes mask rather sharp declines in stability for workers with more than a few years of tenure. These sharp declines are partially offset in the aggregate by gains in job stability for low-tenure workers at the beginning stages of attachment to an employer. The changes by tenure group contrast with the 1980's, and are more consonant with the increase in job loss among "career workers" noted in the popular press. The pattern by tenure group is roughly similar for blacks and whites and for males and females. However, these data do not permit us to infer how stable the jobs of currently younger, low-tenure workers will be after they reach higher levels of tenure. This factor, coupled with the contradictory results for the 1980's, implies that the data on job stability do not support the conclusion that the downward shift in job stability for more-tenured workers, and the more modest decline in aggregate job stability, reflect long-term trends.

II. Related Literature

In earlier work (Diebold, et al., 1996 and 1997) we studied data assembled from a sequence of Current Population Survey (CPS) tenure supplements, issued periodically by the Bureau of the Census, which ask workers how long they have been with their current employer or at their current job. By stringing these supplements together, we study the evolution of tenure for cohorts, summarized in job retention rates over periods of 4 and 10 years. Some of the details regarding our earlier evidence of little or no change in aggregate job stability in the U.S. economy are discussed below.

This earlier conclusion is by no means uncontested, as there has been substantial disagreement among researchers studying this question. Using the PSID, Boisjoly, et al. (1994), Rose (1995), and Marcotte (1996), report evidence of declines in job stability through sample periods extending into the late 1980's or early 1990's. However, Polsky (1996) and Diebold, et al. (1997) examine the evidence from the PSID and conclude that the evidence of decreased job stability reported in the first two of these papers, at least, is largely attributable to changes over time in the variables used to measure job attachment. Swinnerton and Wial (1995) report evidence of declining job stability based on essentially the same CPS tenure supplements that we have used, but the differences in the findings appear to be due in large part to errors they made in using the CPS data (Diebold, et al., 1996; Swinnerton and Wial, 1996). Comparing findings in the National Longitudinal Surveys of Young Men and of Youth, Monks and Pizer (1995) and Bernhardt, et al. (1997) report a large increase in the probability of job turnover in the 1971-1990 period. It is not entirely clear that one can consistently compare these probabilities across the surveys, however, because the NLSY collects information on more jobs per year, which may result in a greater number of spurious job changes (relative to the earlier NLS), and more generally because of

differences between the early experiences of these two cohorts (e.g., the Vietnam War). In contrast, the results in Farber (1995)--although based on a different analysis--are similar to ours, indicating overall stability of cross-section tenure distributions in the CPS tenure supplements.

A second issue raised in recent literature concerns what might be termed "job security" as opposed to job stability. Most of the studies discussed above--as well as the research presented in this paper--look at the employment relationship from the perspective of job stability, asking, in one fashion or another, whether the length of time people remain on their jobs has declined. However, job security could be declining even if job stability remains unchanged. For example, Polsky (1996) suggests that even though overall separations have remained constant, a greater proportion of recent job separations may now be involuntary (layoffs, plant closings, etc.) rather than voluntary (quits). Because we think that workers quit to improve their well-being, whereas involuntary separations are more likely to make an individual worse off, a rising proportion of separations that is involuntary could make workers feel less secure and more anxious about their jobs. Using PSID data to compare the periods 1976-1981 and 1986-1991 (periods chosen because of similar cyclical behavior), Polsky reports that there were modest increases in the rate of involuntary job separation, and that these increases were more marked for older and more-tenured workers. Parallel findings are also reported in Boisjoly, et al. (1994), Valletta (1996), and Farber (1996). In addition, Polsky finds that in the 1980's the consequences of job loss became more severe relative to the 1970's. In particular, the probability of a large real wage loss following an involuntary separation rose significantly, which could also have reduced perceived job security. While we think the security vs. stability question is of interest, in this paper we restrict our attention to evidence on overall job stability.

III. Methods

General Approach

The magnitudes we estimate are retention rates for current jobs, which are the probabilities of retaining a current job over future periods. Specifically, we define the *t-year retention rate*, R(t), as the probability that a worker will have an additional t years of tenure t years hence. The t-year retention rate may be defined for any subgroup of the population, such as workers with particular initial tenure levels. Denoting current tenure by c, and other characteristics by x, we write the t-year retention rate for workers with initial tenure c and characteristics x in the base sample year 0 as $R_{xc}^{0}(t)$.[1] The sequence of retention rates, $R_{xc}^{0}(t)$, t = 1, 2, ... , is the survival function, which provides a complete characterization of the probability distribution of eventual tenure.

The estimation of the complete sequence of retention rates (i.e., the survival function), requires either longitudinal data covering workers' entire careers, or rather strong assumptions. For obvious reasons, researchers have adopted the latter approach. For example, Hall's (1982) estimation of the survival function requires the assumption that the employment survival function is stable over time, and that the overall arrival rate (the number of workers beginning new jobs) is constant (Ureta, 1992); the latter assumption would be violated, for example, by increased labor force participation rates of women. These assumptions enable Hall to estimate the survival function using the 1978 CPS tenure supplement, because these assumptions imply that one can estimate the survival function from synthetic cohorts constructed from a single cross-section; specifically, the t-year retention rate for workers with current tenure c can be computed as the ratio of workers with t+c years of tenure to workers with c years of tenure. However, given that we are investigating *changes* in job stability, we cannot assume a stable survival function. Instead, we

link together a sequence of CPS tenure supplements, and use them to characterize tenure distributions based only on observed historical retention rates across spans of years covered by different tenure supplements.

Tracking changes in job stability using retention rates is potentially advantageous compared with looking at tenure distributions at a point in time. Static tenure distributions are not informative about the stability of currently-held jobs. Only by examining whether these jobs continue to be held some time in the future can their stability be assessed. An obvious response to this problem is to look at changes in static tenure distributions over time, as in Farber (1995). But this can potentially be misleading because mean or median tenure may fall owing to new labor market entrants--the same arrival rate problem discussed above. Thus, while it is of interest to look at overall tenure distributions, we think that retention rates are probably more informative about changes in job stability.

Estimating Retention Rates

The basic t-year retention rate for workers with c years of tenure is estimated as the ratio of the number of workers with t+c years of tenure in the tenure supplement t years hence ($N_{x,t+c}^{0+t}$), to the number of workers with c years of tenure in the base year tenure supplement (N_{xc}^{0}). Formally,

$$(1) \qquad \hat{R}_{xc}^{0}(t) = \frac{N_{x,t+c}^{0+t}}{N_{xc}^{0}}.$$

Retention rates can be estimated for any subgroup consistently represented across surveys. Ideally, we would like to estimate retention rates from longitudinal data in which we can observe whether individuals remain on their jobs in the future. However, as equation (1) indicates, we instead use cross-sectional data sets, studying cohort experiences with respect to job stability by

stringing these data sets together.[2]

Because the retention rate estimate is based on ratios of numbers of workers in different CPS samples, we need the sample of workers from each CPS to be representative of the population to obtain unbiased estimates. The CPS is ideal for this type of comparison, because it provides a representative sample of the United States population when weights provided for each sampled individual are applied. We find, however, that non-response to the tenure supplement varies substantially across years, and varies with demographic characteristics. Therefore in constructing the N's in equation (1), we adjust the standard CPS sample weights by multiplying by the reciprocal of the response rate to the tenure question for each race-age-sex subgroup, with age grouped into 5-year intervals.[3] We always use the adjusted weights in forming the N's, which are then rescaled to reproduce the actual sample sizes.

We are also interested in the variance of this estimated retention rate. If we had true longitudinal data, our estimated retention rate would come from the following calculation. We initially draw a sample of size N_{xc}^{0} (a number on which we condition), and t years later $N_{x,t+c}^{0+t}$ observations have an additional t years of tenure. Conditioning on N_{xc}^{0}, $N_{x,t+c}^{0+t}$ can be modeled as a binomial random variable (with $N_{x,t+c}^{0+t}$ the number of "successes" in N_{xc}^{0} trials). Then the estimate of $R_{xc}^{0}(t)$ in equation (1) would just be the estimate of the proportion of "successes" (defined as remaining in the job for t years). The estimated retention rate would then be asymptotically normally distributed, with

$$(2) \qquad \hat{R}_{xc}^{0}(t) \overset{a}{\sim} N\left(R_{xc}^{0}(t), \frac{R_{xc}^{0}(t)(1 - R_{xc}^{0}(t))}{N_{xc}^{0}} \right).$$

Of course, we do not have true longitudinal data, but rather estimate $N_{x,t+c}^{0+t}$ from an

independent sample. What we do not know, then, is how many observations from the year 0 sample would have been observed with t+c years of tenure t years later (we denote this quantity $N_{x,t+c}^{0,t}$). That is, in contrast to equation (1), the retention rate estimate we would like to use (from true longitudinal data) is

$$(3) \qquad \hat{Q}_{xc}^{0}(t) = \frac{N_{x,t+c}^{0,t}}{N_{xc}^{0}}.$$

We can rewrite the retention rate estimate we actually use as

$$(4) \qquad \hat{R}_{xc}^{0}(t) = \frac{N_{x,t+c}^{0,t}}{N_{xc}^{0}} + \frac{N_{x,t+c}^{0+t} - N_{x,t+c}^{0,t}}{N_{xc}^{0}},$$

which makes clear that our estimated retention rate can be interpreted as a true retention rate in longitudinal data, plus an error (the second term) that comes from the fact that we estimate the number of individuals with t+c years of tenure from an independent sample, rather than a true longitudinal sample. This sampling error generally adds variance to the estimated retention rate.[4] To calculate the variance of the retention rate we can estimate, $N_{x,t+c}^{0+t}$ can be modeled as a multinomial random variable for the number of observations with characteristics x and tenure t+c, out of a sample of N^{0+t}. Using $p_{x,t+c}^{0+t}$ to denote the true proportion with x and t+c in the year 0+t supplement, the variance of the estimated retention rate is

$$(5) \qquad \mathrm{Var}(\hat{R}_{xc}^{0}(t)) = \frac{N^{0+t} p_{x,t+c}^{0+t} (1 - p_{x,t+c}^{0+t})}{(N_{xc}^{0})^2}.$$

We can estimate this variance consistently using the corresponding sample moments.

Because

$$(6) \qquad \frac{N^{0+t}p_{x,t+c}^{0+t}}{(N_{xc}^{0})} \cong R_{xc}^{0}(t),$$

we can write

$$(7) \qquad Var(\hat{R}_{xc}^{0}(t)) \cong \frac{R_{xc}^{0}(t)(1-p_{x,t+c}^{0+t})}{N_{xc}^{0}} = \frac{R_{xc}^{0}(t)(1-R_{xc}^{0}(t))}{N_{xc}^{0}} \cdot \frac{(1-p_{x,t+c}^{0+t})}{(1-R_{xc}^{0}(t))}.$$

When we are estimating the retention rate for the whole sample, $p_{x,t+c}^{0+t}$ and $R_{xc}^{0}(t)$ are both estimates of the retention rate, although they have different expectations if the survival function is not stable (otherwise we could just use cross-sectional data to estimate survival functions). But to a first approximation, in this case the variance of the estimated retention rate using repeated cross sections is no higher. However, when we are looking at a subsample with particular characteristics x, the variance is higher than it would be with true longitudinal data, since $p_{x,t+c}^{0+t}$, which is the cross-sectional estimate of the retention rate multiplied by the proportion of the sample with characteristics x, is less than $R_{xc}^{0}(t)$, as equation (6) shows. Intuitively, this occurs because in the year 0+t sample we also resample on the characteristics x. The smaller is the proportion of observations with characteristics x (and hence the smaller is $p_{x,t+c}^{0+t}$ relative to $R_{xc}^{0}(t)$), the more the variance is increased because we may get variation in $N_{x,t+c}^{0+t}$ owing to sampling variation in the proportion of observations with characteristics x, and not solely variation owing to the uncertainty of retaining a job.

We also study changes in retention rates over time. In computing these changes, in some cases we use the same supplement in the numerator of one retention rate and the denominator of

the earlier retention rate. For example, using the surveys in years 0, 0+t, and 0+2t to estimate changes in t-year retention rates from year 0 to year 0+t, the change in retention rates is

$$(8) \qquad \Delta\hat{R}_{xc}(t) = \frac{N_{x,t+c}^{0+2t}}{N_{xc}^{0+t}} - \frac{N_{x,t+c}^{0+t}}{N_{xc}^{0}}.$$

The variance of this expression involves the covariance of the two estimated retention rates. Depending on the tenure groups we specify, there may be some overlap between the observations counted in N_{xc}^{0+t} and those counted in $N_{x,t+c}^{0+t}$.[5] However, as explained above, in computing each retention rate we condition on the number in the denominator of each rate (the number of individuals with characteristics x and tenure c in the "base" year for calculating the retention rate). Under this assumption, the overlapping observations between the denominator of the first expression and the numerator of the second does not generate a covariance between the two expressions. Obviously, though, the existence of this overlap suggests that this conditioning assumption is not entirely satisfactory, and that the standard errors for the changes in retention rates that we compute are likely to be somewhat understated if we ignore the negative covariance between the estimated retention rates that is generated by the overlapping observations. On the other hand, retention rates may be positively correlated over time as, for example, increases in retention rates affect employers' expectations regarding worker turnover, leading to changes in behavior (such as increased human capital investment) that in turn reduce turnover further. This positive covariance would tend to reduce the variance of the difference in equation (8). For these reasons, one must regard statistical inferences regarding some of our estimated changes in retention rates quite cautiously. However, when we compute changes in 8-year retention rates, we end up using different surveys for the end of the first span over which we compute these rates

(1991), and the beginning of the second span (1987), so the problem of negative covariance from overlapping observations does not arise. Similarly, this problem does not arise when we compute changes in 4-year retention rates from the period 1983-1987 to 1991-1995.

Aside from these factors, there are numerous corrections and adjustments to the data that we have to make to obtain reliable comparisons of job stability over time. These adjustments probably contribute further variance to our estimates of levels of and changes in retention rates. On balance, we suspect that we are understating these variances, which strengthens the evidence when we find no significant changes in estimated retention rates, and weakens the evidence when we find significant changes. Given the uncertainty inherent in the numerous corrections and adjustments we have to make, we suggest that the reader focus more on the point estimates than statistical inferences regarding these estimates.

IV. Data

The empirical analysis in this paper first updates estimates of changes in 4-year retention rates, using 1983, 1987, 1991, and 1995 CPS data. Thus, we estimate and compare retention rates for 1983-1987, 1987-1991, and 1991-1995; estimates for the first two spans were reported in our earlier work. In addition, the 1995 tenure data enable us to estimate and compare 8-year retention rates for 1983-1991 and 1987-1995, on which we also report evidence. For 1983, 1987, and 1991, we use the January CPS tenure supplements. For 1995, we use tenure data from the February Contingent Work Supplement. As in our earlier work, there are some problems with tenure data reported in all years in the CPS. We first discuss how we handle these. We then go on to discuss specific additional problems that arise in the new 1995 data that we use.[6] For all years we study non-agricultural, non-self-employed workers,[7] currently working or with a job but not currently at work, aged 16 or older.

Rounding and Heaping

The empirical probability distributions of reported tenure for each of the 4 years are shown by the dark bars in Figure 1. The rough shape of these distributions is the same in each of the 4 years, with the highest proportion reporting tenure in the range of 0-1 years, and the proportion declining nearly monotonically in subsequent years.[8] However, the empirical distributions reveal some other features.

First, for 1983, 1987, and 1991, the proportion reporting tenure of 1-2 years is lower than the proportion reporting tenure of 2-3 years. This almost surely arises because of the wording of the tenure question, which leads to a phenomenon we call rounding. In each of these surveys, the tenure question asks how long a person has worked for the present employer. If the answer is less than 1 year, the respondent is queried as to length of tenure in months; otherwise the answer is recorded in years. This suggests that if a person has worked more than 1.5 years, he is likely to respond that he has been working for 2 years. So we might expect that approximately one-half of the respondents who have 12-24 months of tenure are coded as having 2 (i.e., 2 to <3) years of tenure, rather than 1 (i.e., 1 to <2). On the other hand, this problem is much less apparent for 1995, because individuals could choose to respond in terms of weeks, months, or years.[9]

A second feature of the empirical tenure distributions is that they have spikes at multiples of 5 years, which we call heaping. The problem was originally identified by Ureta (1992), and presumably arises because of rounding with regard to the number of years for which a respondent has worked for the present employer.

Following procedures developed in Diebold, et al. (1997), we adjust the data for rounding and heaping by estimating a mixture model for reported tenure, the estimates of which we then use to reallocate the rounded and heaped data. For this purpose we model true tenure with a Weibull

distribution, and we assume that individuals report true tenure with probability p and report a nearby multiple of 5 with probability (1-p).[10] The corresponding Weibull survival function is $\exp[-(\alpha t')^{\beta}]$, where t' denotes true tenure in years. We expect heaping to be more severe the longer the true length of the tenure spell, so we allow p to depend linearly on reported tenure t, $p = \gamma + \delta t$, where we expect to find $\delta<0$. We treat the problem of half-year rounding, discussed above, in a similar fashion, by assuming that independently of reported tenure (as long as it exceeds 12 months), individuals report true tenure with probability θ and report 1 year more than true tenure with probability $1-\theta$.

Under these assumptions, the reported tenure distribution differs from the true probability distribution for three reasons--rounding, heaping, and sampling variation. We use the minimum chi-square method to estimate the parameters α, β, γ, and δ for each of the 4 years. First, we divide the possible values of reported tenure into J cells, and then we find the values of the parameters that minimize

$$(9) \qquad \sum_{j=1}^{J} (O_j - E_j)^2/E_j \quad ,$$

where O_j is the actual number of observations in cell j, and E_j is the expected number of observations given the parameters. We estimate the parameters using a grid search.[11]

We use the estimates of the parameters of the mixture model to adjust the data for heaping and rounding. With respect to heaping, for each multiple of 5 years for reported tenure, we calculate the probability that respondents have reported the truth, using the estimates of γ and δ. We then redistribute to adjacent values of tenure the number of respondents estimated to have rounded. The redistribution is in proportion to the percentage shortfall between the expected

number of observations at each of the adjacent values based on the estimated Weibull distribution, and the expected number of observations based on the mixture of the Weibull distribution and the heaping mechanism.[12] To handle rounding, we shift reported tenure down by 1 year for the proportion estimated to have rounded up by 1 year. The graphed lines in Figure 1 show the adjusted distributions for each year.

Business Cycles

Business cycles may influence estimates of retention rates as fluctuations in unemployment affect the probability of termination, independently of underlying changes in job retention rates.[13] To correct for this potential bias, we adjust retention rates for cyclical fluctuations by, in effect, adding back cyclical job terminations.

Our proxy for the cyclical position of unemployment, $U_x(m)$, is simply the residual from a regression of the monthly civilian unemployment rate on a linear time trend, where m indexes time periods measured in months. We do this separately for demographic subgroups, classifying by sex, race, and age (16-20 and 20+). Then we form $E_x(m) = 1 - (U_x(m)-U_x(m-1))$ and estimate the adjusted retention rate as

$$(10) \qquad \hat{R}_{xc}^{*}(t) = \frac{N_{x,t+c}^{0+t}}{N_{xc}^{0}} \left[\frac{1}{[E_x(1)E_x(2)...E_x(12t-1)]} \right].$$

Clearly, if unemployment flows were always on trend (that is, $U_x(m)=0$ and $E_x(m)=1$ for all m), the adjustment factor would be unity, so no adjustment would be made. Otherwise, the adjustment lowers retention rates estimated over expansions and raises retention rates estimated over contractions, with the size of the adjustment depending on the deviations of changes in the unemployment rate from trend over the span.

Problems with 1995 Tenure Data

The 1983, 1987, and 1991 tenure supplements used a uniform question, in particular "How long has ... been working continuously for his present employer ... ?" Unfortunately, in 1995 the Census Bureau did not continue to elicit information on tenure in the same way for the same 4-year interval between earlier supplements. Because the primary results of our analysis involve comparisons over time, ensuring comparability of the data over time is critical to being able to attribute changes in retention rates to changes in job stability, rather than changes in the data instrument. In particular, there were two potentially quite important changes in the tenure questions in the 1995 supplement.[14]

The first important change is that the tenure questions takes different forms for individuals classified as contingent workers. Table 1 lists the various ways the question is asked, depending on the classification of the worker; Appendix Table 1 provides the questions and definitions used to classify alternative types of contingent workers. As a general matter, the questions try to clear up some ambiguities that might arise in using the general tenure question from earlier years for contingent workers. For example, if an individual is a temporary worker in an agency (type 4 in the table), defining how long he has been working continuously for his present employer is ambiguous, since this could refer to the agency itself, or the present placement. Thus, the Contingent Work Supplement first asks about the amount of time worked at the current place of work. It then asks how long the worker has been accepting assignments from a temporary help agency, although it might be preferable to ask how long the worker has been accepting assignments from the temporary help agency from which it now accepts assignments, were there only one. Similarly, contractors are asked about tenure at the place to which they are currently assigned, as well as at the company that contracts out their services.[15]

While these more detailed questions provide less ambiguous information on tenure for contingent workers, it is not immediately obvious which types of answers are most comparable to the tenure responses such workers would have provided on the earlier tenure supplements. Our guess is that the responses that refer to the current place of work are more comparable. Furthermore, using these lower-bound responses makes us least likely to erroneously reject the widely-held view that job stability has declined, which seems the appropriate strategy given that our previous work is at odds with this view. The changes for contingent workers may also have a minor overall impact on the estimates because these workers represent less than 10 percent of the workforce, although this of course depends on how much the measurement of tenure is affected. Nonetheless, we proceed by defining low and high values of tenure in the 1995 survey, based on the workplace specific and more general tenure responses, respectively, and report some results both ways.

The second and most important difference in the 1995 tenure data is that in the earlier tenure supplements the question referred to "continuous" work for the same employer, while the question in 1995 drops the word "continuous." Because some employer separations are temporary, this introduces a comparability problem between reported tenure in 1995 and reported tenure in the previous supplements. For example, if we are calculating a 4-year retention rate from 1991 to 1995, because people will appear to have higher tenure in 1995 than they actually do (based on the earlier definition), it will appear that more people have stayed on their job for 4 years. This upward bias would of course tend to obscure any decrease in job stability through 1995.

We correct for this bias by using alternative available data sources to adjust the 1995 CPS data. First, in February 1996 the Displaced Worker, Job Tenure, and Occupational Mobility

Supplement to the CPS was administered, which included both continuous and total tenure questions. In particular, in the 1996 CPS the question pertaining to total tenure asks how long the respondent has been working for his present employer. The question pertaining to continuous tenure asks how long he has been working continuously for that employer. (The latter is the same question asked in the 1983, 1987, and 1991 supplements.) Our basic strategy is to adjust the 1995 CPS total tenure distribution to represent continuous tenure based on adjustments computed from the 1996 CPS.

These adjustments include three steps. We first calculate the fraction of individuals whose total tenure exceeds continuous tenure. We then calculate the amount of the difference for this subset of individuals. We carry out this latter calculation by disaggregated levels of total tenure, since it is likely that the two measures diverge more for more-tenured workers, and take an additional step to try to more closely approximate the distribution of continuous tenure. Specifically, using the 1996 CPS data for specific tenure groups (encompassing sufficiently large sample sizes), we first estimate the proportion for whom the two measures differ. We then calculate the difference between total and continuous tenure at four quartiles of the distribution of this difference (always weighting the data).[16] Appendix Table 2A reports these results for the 1996 CPS data. In the final step of the adjustment we randomly choose this same proportion of respondents in the corresponding total tenure group in the 1995 CPS, divide this chosen group into four equally-sized groups, and then adjust tenure by the estimated ratio for one of the quartiles.[17]

The top left-hand panel of Figure 2A displays the distribution of total and continuous tenure in the 1996 CPS tenure data, as well as the estimated distribution of continuous tenure that results if we apply this procedure to the distribution of total tenure in the 1996 CPS. The top right-hand panel summarizes this information in terms of the implied differences in the

distribution of total and continuous tenure. The bottom panels display similar information for the 1995 CPS data (having already applied the deheaping procedure for the 1995 CPS explained in the previous subsection), although in this case there is no actual continuous tenure variable. As we would expect, the adjustment based on the 1996 CPS data moves some individuals from higher tenure to lower tenure; this is reflected in the right-hand panels as a negative difference between the proportion with total tenure of 1-2 years, for example, and the proportion with continuous tenure in this range, and positive differences at higher tenure levels. Obviously, then, this adjustment will lead to lower retention rates through 1995 than we obtain in the absence of this adjustment.

As a check on the robustness of our results to this adjustment to the 1995 CPS data, we carry out a similar exercise using information from the Panel Study of Income Dynamics (PSID), which, for the waves covering 1988-1992, asks workers to report current tenure in two ways: total time with employer, and total time since most recent hire. For these years, the question pertaining to total tenure is "How many years' experience do you have altogether with your present employer?" The question pertaining to continuous tenure is "In what month and year did you start working for your present employer? ... [G]ive us your most recent start date if you have gone to work for them more than once." Although the first (total tenure) question is not identical to the 1995 tenure questions in the CPS, and the second (continuous tenure) question is not identical to the tenure questions in the 1983, 1987, and 1991 CPS tenure supplements, the two PSID questions seem sufficiently similar to the corresponding CPS questions that they can provide a useful set of alternative adjustments to the 1995 CPS data.

As in the previous case, the adjustment is a three-step procedure. In the two steps we estimate from the PSID the fraction of individuals whose total tenure exceeds continuous tenure,

and the amount of the difference for this subset of individuals. We then go to the 1995 CPS data, randomly pick out this fraction of workers, and adjust their total tenure downward by this amount; again we use four quartiles of the distribution of the difference between continuous and total tenure. The final outcome is an approximate distribution of continuous tenure in the 1995 CPS data. An appendix describes this adjustment in detail, and Appendix Table 2B reports information on the distribution of total and continuous tenure in the PSID. A comparison with Appendix Table 2A indicates that in the PSID a higher proportion of respondents have different continuous and total tenure, although the differences tend to be smaller than in the 1996 CPS. The larger number of smaller differences presumably stems from the differences between the two tenure measures in the PSID, as explained in the appendix. As the appendix makes clear, the PSID adjustment is more complicated, leading us to prefer the CPS adjustment, while reporting the PSID adjustment as a check on the robustness of our findings. Figure 2B summarizes the effects of the adjustment, with the top panels showing the effects of the adjustment procedure within the PSID data, and the bottom panels showing the effects on the 1995 CPS data. As for the adjustment using the 1996 CPS data, the adjustment based on the PSID data moves some individuals in the CPS from higher tenure to lower tenure, again leading to lower retention rates through 1995. The precise adjustments at each tenure level are different, however, from those using the 1996 CPS data.

Finally, as a last check on the robustness of the results to the tenure correction, we report limited findings in which we simply pretend that the 1996 CPS supplement was administered one year earlier, and substitute it for the 1995 supplement. Strictly speaking, this requires that the 1996 sample comes from a stable population with a stable survival function for job tenure and a constant arrival rate. While we do not believe that either of these assumptions holds, we believe that violations of this assumption over such a short period may be sufficiently minor that

substituting the 1996 data for the 1995 data provides at least an informative check on whether the more complicated adjustments using the 1996 CPS data or the 1988-1992 PSID data are providing misleading estimates of job retention rates.

A natural question that arises given the preceding discussion is whether we can learn anything reliable from extending the analysis to use the 1995 data, and more significantly, whether there are alternative data sources that might not be plagued by changes in the survey instrument, which might provide enhanced comparability of job stability over time. As noted above, some of the other data sources that may address this issue are also affected by changes in the survey questions. Problems with the PSID were noted earlier, although Polsky (1996) has demonstrated that it is possible to use the PSID to look at changes in job stability on a consistent basis over time; in addition, the PSID sample is much smaller, which prevents much disaggregated analysis. Farber (1996) has used the Displaced Worker Surveys (DWS) to study job loss--in contrast to overall job stability. It turns out, though, that this data source also underwent changes that make comparisons of the 1994 DWS with earlier surveys problematic. Specifically, the recall period covered through the 1992 DWS was 5 years, compared with 3 years in the 1994 DWS. Farber uses information from the PSID to adjust the data. Thus, researchers interested in measuring changes in job stability or job security over time appear to have little choice but to use data sources that are not entirely consistent over time, and to attempt to develop procedures for making these sources consistent. In our view, it is important to try to update the evidence on changes in job stability, while being cautious regarding strong conclusions drawn from somewhat different data sources. Finally, we would express the hope that uniform methods of tracking job stability over time will be pursued.

V. Results

We first report, in Table 2A, estimates of 4-year retention rates for 1983-1987 and 1987-1991, for all workers in our sample, classified by initial tenure; this provides a brief overview of the results reported in Diebold, et al. (1997). Table 2A reveals that taking account of heaping and the business cycle, the estimated 4-year retention rate scarcely changed, falling from .539 to .536 from 1983 to 1987.[18] Without accounting for heaping or the cycle, the data suggest a modest decline in job stability, with the 4-year retention rate falling by .021. Note that the main effect of the business cycle adjustment is to lower the estimated 1983-1987 retention rate, which follows from the fact that the unemployment rate fell over this period. The qualitative pattern by tenure group is similar with and without the adjustments. In particular, job stability fell for those with 2-9 years of tenure, but rose for those with 15 or more years of tenure. Thus, in what follows using the 1995 data, we focus on results with adjustments for heaping and the business cycle.

As discussed earlier, the tenure questions in the 1995 supplement, on which estimated retention rates through 1995 are based, are not entirely comparable with earlier tenure questions. In Table 2B we report results of several alternative adjustment strategies to gauge the degree of uncertainty in the estimates through 1995 stemming from the changes in the tenure questions. Specifically, in Table 2B we report estimates of 4-year retention rates for 1991-1995 based on the alternative 1995 tenure questions, using first the raw 1995 total tenure data, and then the alternative strategies for adjusting the total tenure distribution to the continuous tenure distribution. However, our preferred estimates are based on the lower-bound tenure question in the 1995 CPS supplement, using the 1996 CPS data to adjust the 1995 data; these are reported in the second column. We also report the estimated changes in retention rates from 1983 and 1987.

The first column of Table 2B reports results using total tenure in 1995 (i.e., unadjusted

tenure), based on the lower-bound variables for 1995 tenure; these latter variables correspond to the tenure questions labeled "low" in Table 1, when there are two questions. As the table shows, the estimated 1991-1995 retention rate (.551) is slightly higher than for either 1987-1991 (.536) or 1983-1987 (.539). Although this suggests an increase in aggregate job stability, the total tenure variable in 1995 overstates tenure as defined in earlier years, and hence biases the estimated 1991-1995 retention rates upward.

The second column therefore adjusts the 1995 data using information on total and continuous tenure in the 1996 CPS supplement. As expected, this lowers estimated 1991-1995 retention rates. Nonetheless, aggregate job stability still appears not to have fallen, with an estimated 4-year retention rate of .544, a shade higher than for 1983-1987 or 1987-1991.[19] However, when we disaggregate by tenure some rather sharp differences emerge. Workers with less than 2 years of tenure experienced significant estimated increases in 4-year retention rates (.061 relative to 1983-1987, and .045 relative to 1987-1991). On the other hand, workers with 9-15 years of tenure experienced significant and somewhat larger declines, and for the comparison with 1987-1991, the same is true of workers with 15 or more years of tenure. In contrast, in comparison with 1983-1987, workers with 2-9 years of tenure experienced smaller but still significant declines in job stability.[20] The qualitative difference between less-tenured and more-tenured workers is striking. Some of these qualitative findings show a reversal of the evidence through 1991. Specifically, the 4-year retention rate for workers with 15 or more years of tenure rose from 1983 to 1987, and the 4-year retention rate for workers with 9-15 years of tenure was flat over this period. The evidence of recent declines in job stability for those with 9-15 and 15+ years of tenure indicates a shift toward less job stability among long-term, career workers in the first half of the 1990's.

The third column reports parallel results using the PSID adjustment to the total tenure data in the 1995 CPS supplement. With this adjustment there is a slight (.006 or .008) but significant decrease in aggregate job stability, and the changes by tenure group are very similar to those based on the adjustment using the 1996 CPS. Thus, the evidence appears generally robust to adjustments based on these two data sources.

In the next three columns, we show the differences that result from using the upper-bound tenure variables in the 1995 Contingent Work Supplement; these latter variables are based on the tenure questions labeled "high" in Table 1, when there are two questions. As we would expect, estimated retention rates for 1991-1995 are higher using these alternative tenure variables, in the aggregate and for each tenure group, and hence any declines in retention rates are moderated. However, the differences in the results are small. As a consequence of the small differences, and because the lower-bound estimates correspond more closely to tenure with an employer, we will focus on the lower-bound estimates in the remainder of the paper.

Finally, the last column instead substitutes the 1996 tenure data for 1995, our third (admittedly crude) method of accounting for problems with the 1995 Contingent Work Supplement. By tenure group, the results are qualitatively similar to those based on adjustments to the 1995 data, with increased stability for less-tenured workers, and decreased stability for more-tenured workers. The only difference is that using the data this way indicates a more substantial but still modest (.020) increase in aggregate job stability; this difference hints at an improvement in job stability between 1995 and 1996, since the 1996 tenure data are now used directly rather than to adjust the 1995 data. We have more confidence in the estimates based on explicit adjustments to the 1995 CPS data. But regardless, we have what we regard as robust evidence of no decline in aggregate job stability based on 4-year retention rates, moderate increases in job

stability for less-tenured workers, and sizable decreases in job stability for more-tenured workers. Furthermore, the combined evidence in Table 2B indicates that our preferred retention rate adjustment is a cautious choice, producing an estimated change in the aggregate retention rate that is bounded below by the PSID adjustment and bounded above by the "high" CPS adjustment and the 1996 CPS data.

With the tenure data for 1983, 1987, 1991, and 1995, we can also estimate 8-year retention rates for 1983-1991 and 1987-1995. Table 3 reports estimates of these rates, in all cases adjusting for heaping and the business cycle and using the lower-bound tenure variable for 1995. We first report estimates using 1995 total tenure, and then estimates using the adjustment to continuous tenure, based on the 1996 CPS supplement. As expected, this adjustment lowers estimated retention rates for 1987-1995. The evidence suggests a modest decline in aggregate job stability, with the estimated retention rate falling by .021 (based on the adjusted data).[21] Note that a given decline in retention rates is more important the longer the span over which the rate is computed, because the retention rate over a longer span is lower in the first place; for example, the 8-year retention rate from 1983 to 1991 is .368, whereas the 4-year retention rate from 1983 to 1987 is .539. The pattern of changes in 8-year retention rates by tenure group partly mirrors that obtained from 4-year retention rates, with job stability increasing for workers with 0-2 years of tenure and falling for those with 9-15 or 15+ years of tenure. However, here stability also falls for those with 2-9 years of tenure, providing a relatively consistent picture of decreases in job stability for more-tenured workers. Although not reported in the table, the adjustment using the PSID gives very similar results, with if anything sharper declines in aggregate 8-year retention rates. Using the 1996 CPS file directly gives qualitatively similar (but weaker) results by tenure group, and no evidence of an aggregate change. Thus, although the results for 8-year retention rates are

somewhat less robust across the alternative adjustments, the preferred adjustment based on using the 1996 CPS to correct the 1995 CPS data appears to be bounded by the other two adjustments, and all of the alternative adjustments point to declines in job stability for more-tenured workers.

Finally, we turn to evidence disaggregated by race, sex, and age. In the disaggregated analysis, we report results using the same adjustments and tenure questions as in Table 3. Table 4 first reports results for 4-year retention rates broken down by race. The patterns of changes in retention rates between 1987 and 1991 are similar for whites and blacks; the sharper decline for blacks for the 1983 vs. 1991 comparison stems solely from the change between 1983 and 1987. For both groups, there is little evidence of an aggregate decline in job stability. For both races more-tenured workers suffered sizable declines in job stability, although for blacks the decline in retention rates is concentrated among those with 15 or more years of tenure. Less-tenured white workers experienced increases in job stability, while for less-tenured black workers this increase is smaller and is not statistically significant.

Panel B reports results by sex. Convergence in aggregate 4-year retention rates has continued, with women experiencing some increase, and men some decrease (since 1983). Among both sexes, though, workers with 9-15 years of tenure experienced declines in job stability. Panel C reports results disaggregated by age group instead of by tenure. The estimates reveal modest and generally insignificant declines in all age groups.

Table 5 reports similar disaggregated evidence for the 8-year retention rates. The most striking results in this table are the race and sex differences. In particular, blacks experienced considerably sharper declines in aggregate job stability than did whites (.058 vs. .016)--although the 4-year retention rates suggest that these changes occurred in the late 1980's--and men experienced considerably sharper declines than women (.033 vs. .007); all of these estimated

declines except that for women are statistically significant. For males and especially for blacks, sharp declines are evident for those with 9-15 years of tenure.

The results disaggregated by age indicate statistically significant declines for all age groups except those aged 55 and over, although the estimated declines are not sharper for the older, "career employees" about whom much has been written. Curiously, estimated retention rates for each subgroup under age 55 declined by more than the aggregate retention rate (unlike the case when we disaggregated by tenure). This can be explained by the combination of two facts. First, the workforce is aging, with the decline in the proportion of workers aged 16-24 particularly sharp. Second, through the 40-54 age group, retention rates increase with age; for example, in 1987 the estimated 8-year retention rates for age groups 16-24, 25-39, and 40-54 are .151, .397, and .475, respectively. Thus, age-specific retention rates have declined, but this is masked to some extent by population shifts toward older workers whose jobs are more stable. This implies that aggregate changes in job stability tell us what is happening to jobs overall, while the age-specific changes are more informative as to what is happening to particular types of workers. As Panel C of Table 5 suggests, this distinction can be of some importance. In particular, the evidence of declines in job stability is a bit stronger when we ask how job stability has changed relative to what workers at similar points in their careers used to experience.

VI. Conclusions

We update evidence on job stability in the U.S. economy through 1995, by combining information from the 1995 CPS Contingent Work Supplement with earlier CPS tenure supplements. Using these somewhat different data sources requires a variety of adjustment procedures. But the general robustness of results across alternative adjustment procedures suggests that we have been successful in transforming these supplements into a set of cross-section

data sets with comparable tenure data, which we can string together to provide a reasonably accurate empirical description of changes in job retention rates. These retention rates provide measures of job stability that are immune to both changes in the underlying distribution (or survival function) for job tenure, and changes in new arrivals in the labor market.

The aggregate evidence points to modest declines in job stability in the first half of the 1990's, differing somewhat from the 1980's during which aggregate job stability remained stable. However, the drop in aggregate job stability might appear somewhat worse if the workforce was not shifting towards ages in which jobs are typically more stable. More significantly, in the first half of the 1990's more-tenured workers experienced significant and perhaps rather large declines in job stability, although less-tenured workers experienced gains in job stability. This contrasts with the 1980's, in which the declines in job stability that did appear were concentrated among younger, less-skilled, less-tenured workers.

Other research has claimed evidence of substantial declines in job stability during the 1980's, although we have disputed many of those findings. Thus, while we have found evidence of more recent declines in job stability--especially for older, more-tenured workers--we caution strongly against the conclusion from the accumulating body of research on this topic that there is evidence of a long-term decline in job stability. Rather, at this point we view the evidence as pointing mainly to a drop in job stability for some groups of workers in the first half of the 1990's. We do not see this as part of a longer-term trend, and can only speculate as to whether it is a temporary or permanent change in U.S. labor markets.

Appendix: The PSID Adjustment

The PSID adjustment to the 1995 CPS data is carried out in the same steps as the adjustment based on the 1996 CPS data. The first two steps use the PSID to estimate the fraction of individuals whose total tenure exceeds continuous tenure and the amount of the difference for this subset of individuals. Because of differences between the PSID and CPS questions, this is more complicated than the adjustment based on the 1996 CPS supplement described in the main text. This appendix reviews how these adjustment factors are computed and how we deal with heaping of data in the PSID. In the third step we apply these adjustment factors to the 1995 CPS to arrive at an approximate distribution of continuous tenure for the 1995 CPS data. This step was described in detail in the main text for the adjustment based on the 1996 CPS data.

The PSID question pertaining to total tenure is "How many years' experience do you have altogether with your present employer?" Respondents could answer in years or months, although most answered in years, in which case the corresponding multiple of 12 months was recorded. The question pertaining to continuous tenure is "In what month and year did you start working for your present employer? ... [G]ive us your most recent start date if you have gone to work for them more than once."

As in the CPS data, the total tenure question--which is effectively asked in terms of number of years--exhibits substantial heaping of the data at multiples of 5 years. On the other hand, the continuous tenure question, which is asked in terms of start dates, exhibits little heaping. Because we want to compare the total and continuous tenure measures in the CPS, and heaping occurs only in the former, this is problematic, and requires that we eliminate or at least substantially reduce the heaping in the PSID total tenure data. As described in the paper, for the CPS data we have only one tenure measure for each person, and implement a deheaping procedure

that is essentially based on a model with random heaping, the parameters of which are estimated using a goodness-of-fit criterion. In the PSID data, however, we can use a more judicious method of deheaping, because we can spot inconsistencies between the total and continuous tenure variables that are most likely to arise from heaping.

In particular, we look at individuals whose reported tenure is a multiple of 5 years. We identify heapers (who have rounded down) as any of these individuals whose continuous tenure is greater than total tenure, since (conditional on accepting continuous tenure as accurate) there is an inconsistency for such individuals. We then adjust total tenure for these individuals to equal continuous tenure. This gives us a number of individuals (for a given multiple of 5) who rounded down because of heaping. We then assume that there was symmetric heaping from the other side of that multiple of 5, in the sense that we assume the same number at each multiple of 5 rounded up because of heaping. Furthermore, because for most individuals continuous and total tenure are the same, we assume that those whose reported continuous tenure is closest to the multiple of 5 are most likely to have rounded up. For example, we assume that it is more likely that someone with 8 or 9 years of continuous tenure would have rounded total tenure up to 10 years than would someone with 2 or 3 years of continuous tenure. Thus, we distribute the assumed heapers first to those with 9 years of continuous tenure, then 8, etc., until the total number is exhausted.[22] The outcome of this procedure is "corrected" data on years of total tenure.

In order to use the data on continuous and (now corrected) total tenure to obtain adjustments to apply to the CPS data, we estimate the fraction of individuals whose total tenure exceeds continuous tenure, and the amount of the difference for this subset of individuals. As explained in the text, we actually carry out this procedure by disaggregated levels of total tenure, since it is likely that the two measures diverge more for more-tenured workers, and we try to more

closely approximate the distribution of continuous tenure.

However, there is an additional complication with the PSID data that must be addressed to obtain comparable continuous and total tenure measures. Specifically, because of how the questions are asked, the total tenure measure in the PSID is rounded to multiples of 12 months, while continuous tenure is not. In the procedure used to deheap years of tenure, we leave months unchanged (other than adjusting by multiples of 12 months), so this heaping by month remains. Because the density for job tenure is monotonically decreasing, this rounding in the total tenure variable would lead us to overstate the discrepancy between years of total and continuous tenure. For example, suppose that reported total tenure equals 24 months, and continuous tenure equals 13 months. If we divide each of these by 12 and round down to get years, the discrepancy is 1 year. But for those individuals who rounded up to 24 months (2 years), this overstates the discrepancy, possibly considerably, given that we follow the convention of calling 0-11 months of tenure 0 years, 12-23 months 1 year, etc. Of course, others rounded down, but given the shape of the density, there should be fewer of these. We therefore implement a simple procedure for removing this bias from rounding, distributing those reporting a multiple of 12 months to 1-6 months more or less, in proportion to the number of people reporting 1 year more or less. For example, suppose we are looking at the people heaped at 60 months. We distribute these individuals to 54-61 months (it does not matter to which exact month we assign them) and to 61-66 months. Because the density for tenure is monotonically decreasing, more should get distributed to below 60 than to above 60. We use as proportions the number at 48 months and the number at 72 months, divided by the sum of these numbers. (For those reporting 12 months or 24 months, there are more observations reporting months that are not multiples of 12, so we use the numbers reporting 6 months more or less to construct these proportions.)[23]

Once we have the final continuous and total tenure measures in the PSID, we define specific tenure groups (encompassing sufficiently large sample sizes), and then estimate the proportion for whom the two measures differ. We then calculate the difference between total and continuous tenure at four quartiles of the distribution of this difference (always weighting the PSID data). Appendix Table 2A reports these results for the PSID data.

Endnotes

1. c can refer to a range of tenure levels.

2. When x refers to age, it is increased by t years in each subsequent survey to follow the right cohort over time.

3. The CPS sample weight is the reciprocal of the probability of being sampled, adjusted for non-interview and variation in the sampling of race-age-sex and residence subgroups.

4. In Diebold, et al. (1997) we ignored this additional variation. However, accounting for it would only have reinforced our statistical inference that aggregate job stability had not declined.

5. For example, if we calculate retention rates for those with 1-10 years of tenure in the base year, and t equals 4, then some observations counted in N_{xc}^{0+t} also are counted in $N_{x,t+c}^{0+t}$, where t+c will be the range 5-14 years of tenure.

6. The February 1996 tenure supplement is more comparable to the earlier surveys, but because it comes 5 years after the 1991 supplement, it cannot be used directly to estimate 4-year retention rates that are comparable to those we estimate for earlier years. Nonetheless, as explained below we do consider some evidence from this supplement.

7. Specifically, the unincorporated self-employed are excluded.

8. Our convention is that when a tenure interval is specified, the first value is included in the interval and the second excluded. For example, 0-1 means $0 \leq \text{tenure} < 1$.

9. In fact, the BLS recognized this problem, and changed the subsequent February 1996 tenure supplement in a similar way to reduce rounding.

10. For at least two reasons, it is unlikely that the rounding and heaping adjustments depend on the Weibull assumption in any important way. First, the Weibull is actually a fairly rich functional form, allowing for both an increasing and decreasing hazard, as well as a flat hazard in the nested exponential case. Second, the Weibull mixture model is used purely as a smoothing device, and a comparison of the original and adjusted tenure distributions in Figure 1 (discussed below) reveals that apart from the valleys and peaks corresponding to rounding and heaping, which of course hardly appear in the adjusted distributions, the adjusted and unadjusted distributions agree closely.

11. The cells used are 0, 1-2, 3-4, 5, 6-9, 10, 11-14, 15, 16-19, 20, and 21 or more. For the four supplements, the estimates of α ranged from .21 to .23, while those of β ranged from .79 to .81. The estimates of γ and δ were consistent with a very low probability of rounding at low levels of tenure, but a rising probability with tenure; the estimates of γ ranged from .94 to .99, and the estimates of δ ranged from -.005 to -.008, implying that $0 < p < 1$ for all observed values of tenure. Finally, the estimates of θ ranged from .33 to .41 for the 1983-1991 tenure supplements, consistent with our conjecture that roughly one-half of respondents round reported tenure upward in these supplements, and the estimate was .15 for the 1995 survey, consistent with less evidence of rounding in the 1995 data apparent in Figure 1.

12. Because the tendency to heap appears to be approximately 3 times more likely at multiples of 5 ending in 0 (i.e., multiples of 10) than multiples ending in 5, we use the following adjacent values. For multiples ending in 5, we define the adjacent values as 1 year less or 1 year more of tenure (e.g., for 5 years of tenure, we use 4 and 6). For multiples ending in 10, we define the adjacent values as 1 to 3 years less, and 1 to 3 years more.

13. In addition, variation in the unemployment rate could influence the retention rate of workers employed at any point in time, if unemployment is correlated with the quality of job matches. We do not explore this source of bias.

14. A third change, while most apparent, is inconsequential. Whereas tenure information for 1983, 1987, and 1991 comes from January, the information for 1995 comes from the February 1995 Contingent Work Supplement. Since there is always some risk of losing one's job in any month, defining workers' tenure as of February will lead to downward bias in estimates of retention rates through 1995. However, it is easy to show that this bias is trivial. If we assume a constant risk of losing one's job each month, this biases the estimated 4-year retention rate through 1995 downward by 1/48 = .02 (because there are 49 months). Of course, the actual risk is much higher in the early months on a job, and much lower in later months, suggesting that the bias from using data from February is much smaller. For example, if we use estimates of the Weibull parameters of $\alpha = .22$ and $\beta = .80$, based on the estimates obtained in the deheaping procedure, the downward bias is .006.

15. We experimented with trying to use the 1995 data to identify a large number of industry and occupation cells with few or no contingent workers, to use the tenure data from all of the years to study these cells only. But it turned out that contingent workers were distributed across a wide array of industry and occupation cells.

16. These calculations are made on the deheaped data in order to preserve the relationship between continuous and total tenure at the individual level.

17. The total tenure question in the 1996 CPS is not asked of the self-employed, but this group is not included in the adjustment since the distinction between total and continuous tenure is not applicable to it.

18. There are slight differences in the sample and estimation method compared with Diebold, et al. (1997), in which the corresponding estimated 4-year retention rate fell from .537 to .527.

19. Without the business cycle adjustment, the increase in the 4-year retention rate for 1991-1995 is slightly larger.

20. These tenure groups were chosen in our earlier work by combining more disaggregated tenure groups with similar experiences over the 1983-1991 period.

21. Without the business cycle adjustment, the decrease in the 8-year retention rate is a bit sharper.

22. For example, suppose that among 50 individuals reporting 10 years of total tenure, 5 reported 11 years of continuous tenure, and 2 reported 12 years of continuous tenure. Then we assume that these 7 individuals heaped total tenure downward, and adjust their total tenure to equal continuous tenure. We then assume that 7 individuals also heaped upward to 10. Suppose that

of those reporting 10 years of total tenure, 4 report 9 years of continuous tenure, and 5 report 8 years. Then we adjust total tenure to 9 for the 4 who report 9 years of continuous tenure, and adjust total tenure to 8 for 3 of the 5 who report 8 years of continuous tenure, selecting the subset of 3 randomly. Thus, for those initially reporting 10 years of total tenure, the distributions are as follows:

Tenure:	≤7	8	9	10	11	12	≥13
Number reporting total tenure=	0	0	0	50	0	0	0
Number reporting continuous tenure=	c	5	4	34-c	5	2	0
Number with deheaped total tenure=	0	3	4	34	5	2	0

23. The total tenure question in the PSID is asked of the non-self-employed. As noted earlier, we leave the incorporated self-employed in the CPS data. We therefore may introduce a slight discrepancy by applying the adjustment based on the PSID data to the CPS data.

References

Bernhardt, Annette, Martina Morris, Mark Handcock, and Marc Scott. 1997. "Job Instability and Wage Inequality: Preliminary Results from Two NLS Cohorts." Mimeograph.

Boisjoly, Johanne, Greg J. Duncan, and Timothy Smeeding. 1994. "Have Highly-Skilled Workers Fallen from Grace? The Shifting Burdens of Involuntary Job Losses from 1968 to 1992." Mimeograph.

Diebold, Francis X., David Neumark, and Daniel Polsky. "Job Stability in the United States." *Journal of Labor Economics*, Vol. 15, No. 2, pp. 206-33.

_____. 1996. "Comment on Kenneth A. Swinnerton and Howard Wial, "Is Job Stability Declining in the U.S. Economy?"" *Industrial and Labor Relations Review*, Vol. 49, No. 2, pp. 348-52.

Farber, Henry S. 1995. "Are Lifetime Jobs Disappearing? Job Duration in the United States: 1973-1993." NBER Working Paper No. 5014.

_____. 1996. "The Changing Face of Job Loss in the United States, 1981-1993." Working Paper #360, Industrial Relations Section, Princeton University.

Hall, Robert E. 1982. "The Importance of Lifetime Jobs in the U.S. Economy." *American Economic Review*, Vol. 72, pp. 716-24.

Marcotte, Dave E. 1996. "Has Job Stability Declined? Evidence from the Panel Study of Income Dynamics." Mimeograph.

Monks, James, and Steven Pizer. 1995. "Trends in Voluntary and Involuntary Job Turnover." Mimeograph.

Neumark, David, and Daniel Polsky. 1997. "Changes in the Employment Relationship: Anecdotes and Evidence." Mimeograph.

Polsky, Daniel. 1996. "Changes in the Consequences of Job Separations in the United States." Mimeograph.

Rose, Stephen J. 1995. "Declining Job Security and the Professionalization of Opportunity." Research Report 95-04, National Commission for Employment Policy, Washington, D.C.

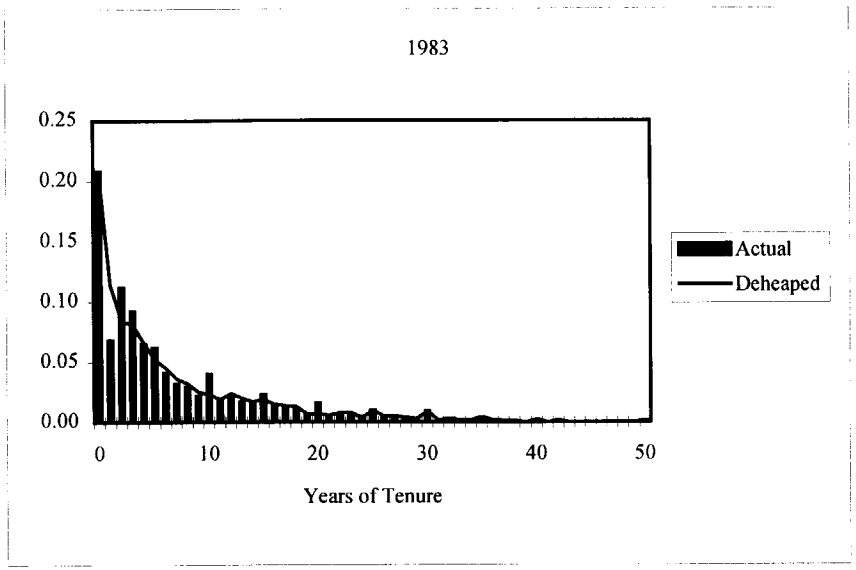
Swinnerton, Kenneth, and Howard Wial. 1995. "Is Job Stability Declining in the U.S. Economy?" *Industrial and Labor Relations Review,* 48, pp. 293-304.

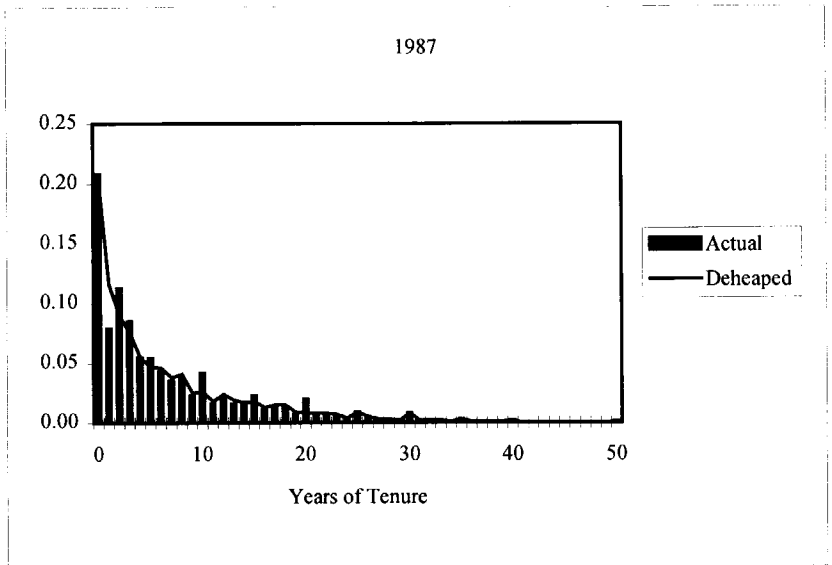
_____. 1996. "Is Job Stability Declining in the U.S. Economy? Reply to Diebold, Neumark, and Polsky." *Industrial and Labor Relations Review*, 49, pp. 352-55.

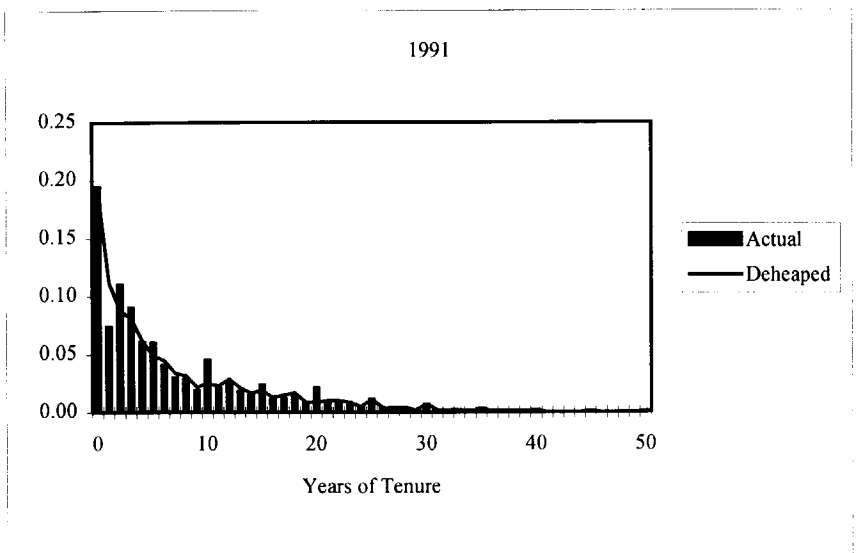
Ureta, Manuelita. 1992. "The Importance of Lifetime Jobs in the U.S. Economy, Revisited." *American Economic Review*, 82, March, pp. 322-35.

Valletta, Robert G. 1996. "Has Job Security in the U.S. Declined?" Federal Reserve Bank of San Francisco Weekly Letter, Number 96-07, February 16, 1996.

Figure 1
Frequency Distributions of Job Tenure in CPS Surveys







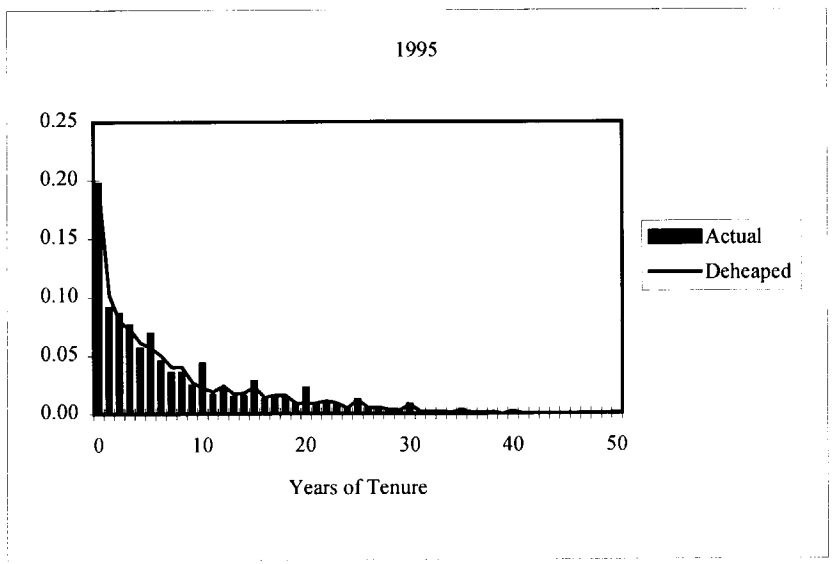
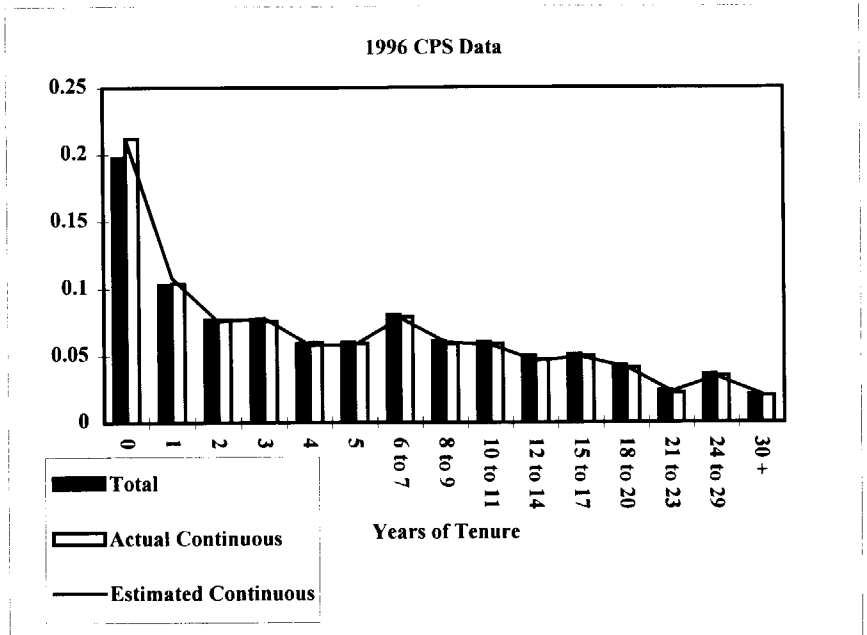
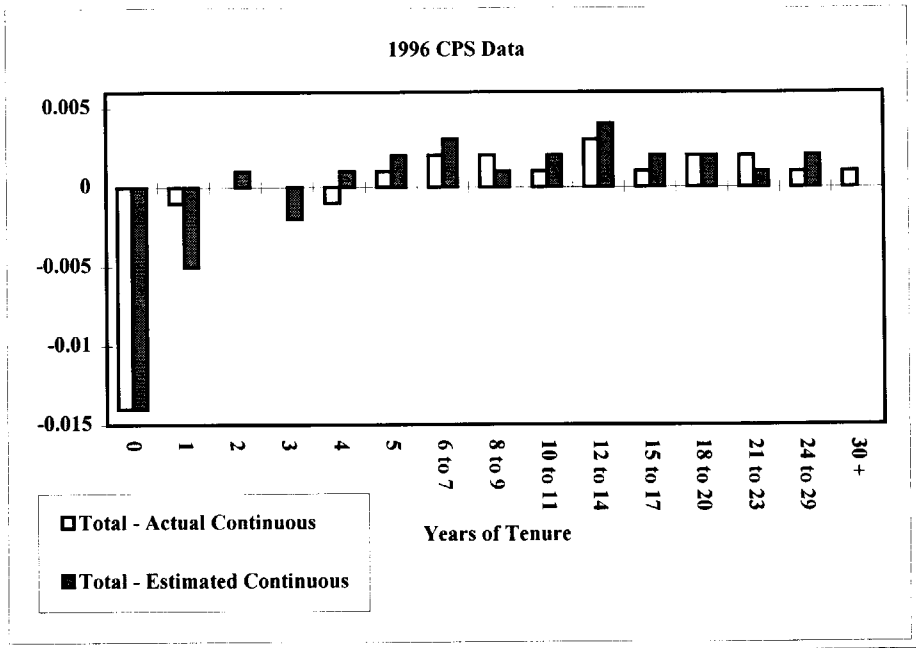
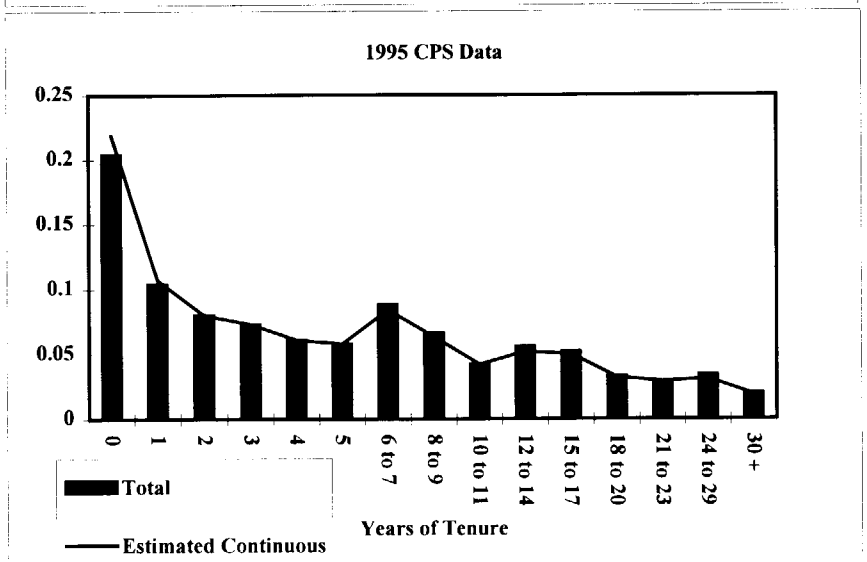


Figure 2A
Frequency Distribution of Total Job Tenure and Estimated Continuous Job Tenure Based on the CPS Adjustments







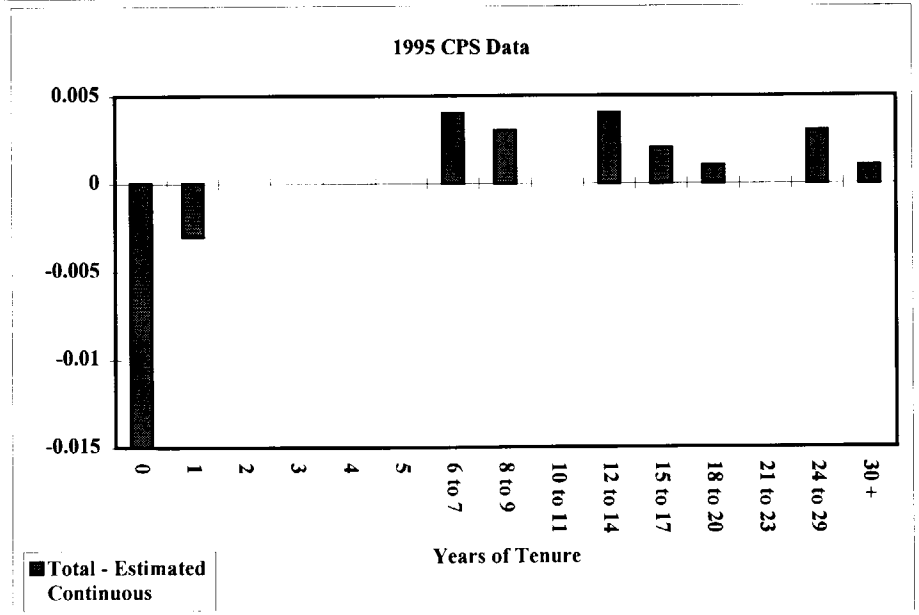
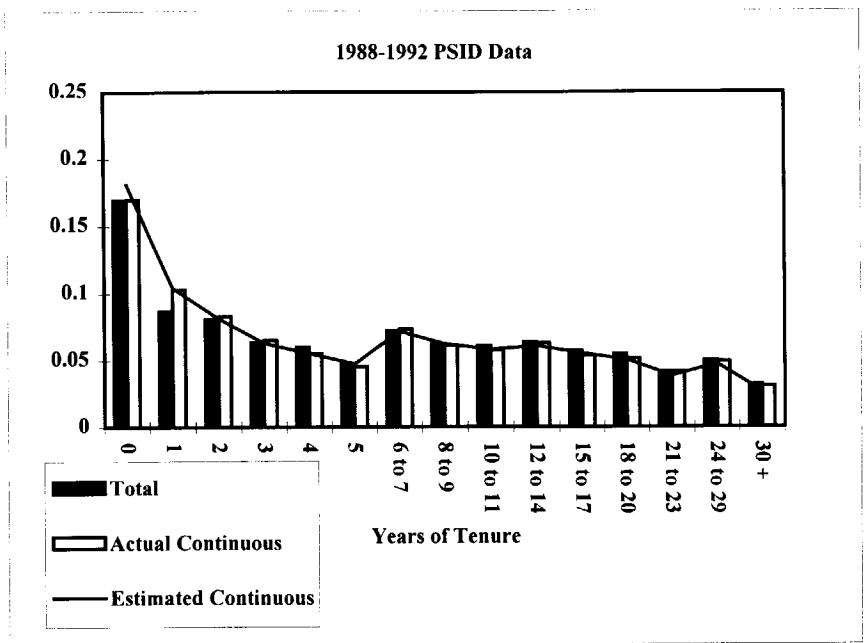
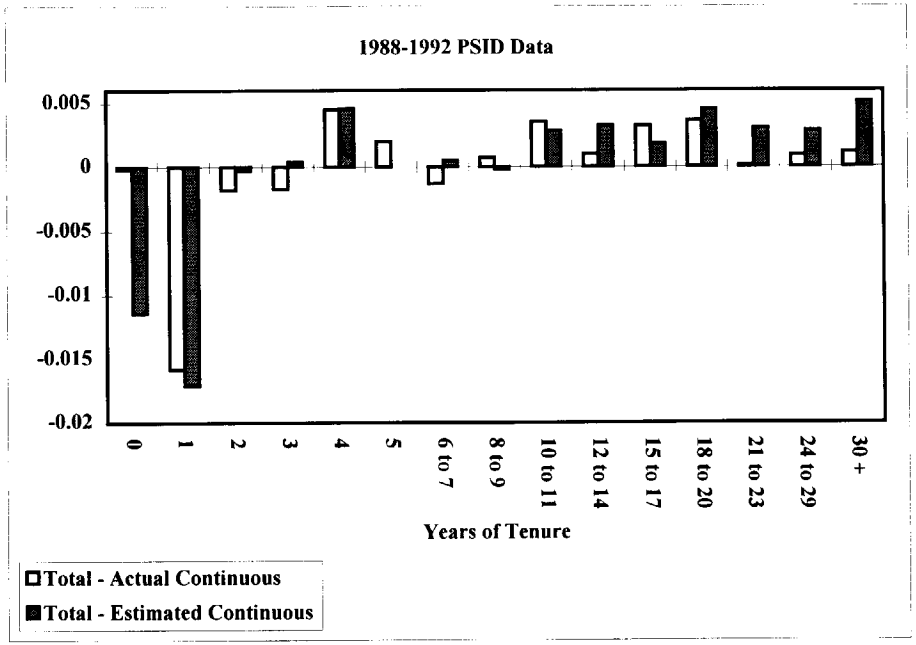
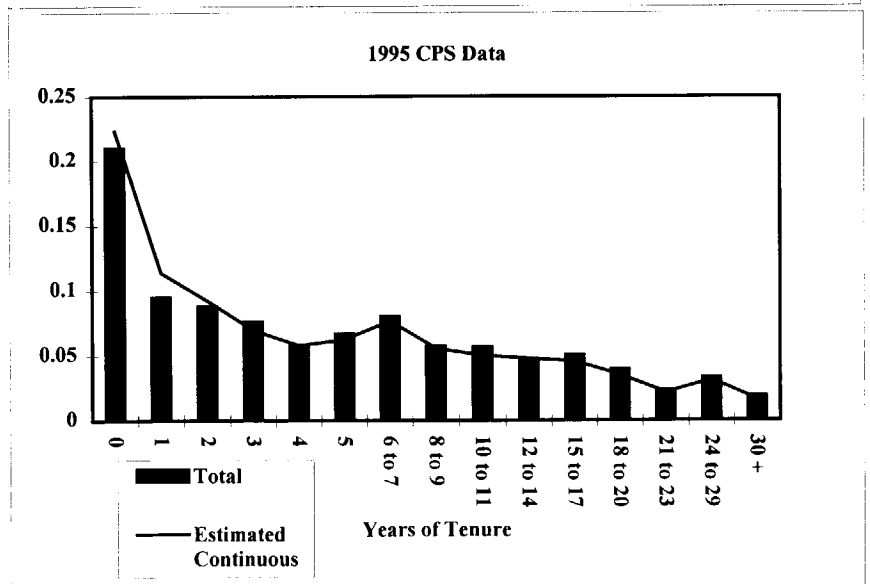


Figure 2B
Frequency Distribution of Total Job Tenure and
Estimated Continuous Job Tenure Based on PSID Adjustments







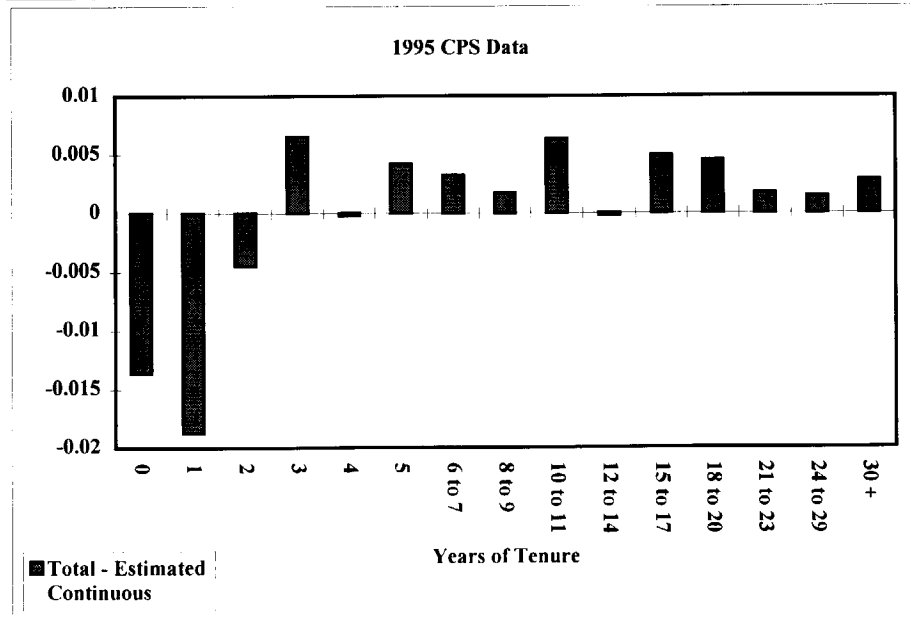


Table 1
Tenure Questions in the February 1995 Contingent Worker Supplement

Type 1: Incorporated Self-Employed (3.87%)

"How long have you been self-employed?"

Type 2: Independent Contractor (.98%)

"How long have you worked for the employer where you worked last week?" (low), or

"How long have you been an independent contractor? (high)

Type 3: Temporary Worker, No Agency (3.29%)

"How long have you worked for (fill: employer's name from the basic CPS)?"

Type 4: Temporary Worker in an Agency (.94%)

"How long have you worked (where you are currently working / fill: employer's name from the basic CPS)?" (low), or

"How long have you been accepting assignments from a temporary help agency? If there have been long periods when you have been turning down assignments for reasons such as attending school, only include the time since the last interruption." (high)

Type 5: Contractor with Employer (0%)

"How long have you worked (fill: for employer's name from basic CPS / the place where you were assigned)? (low), or

"How long have you worked for the company that contracts out your services?" (high)

Type 6: On-Call Worker (1.40%)

"How long have you worked for the employer where you were working last week? (low), or

"How long have you been an on-call worker? (high)

Type 7: Day Laborer (0%)

"How long have you been a day laborer?"

Type 8: Non-Contingent Worker (89.51%)

"How long have you worked for (fill: employer's name from the basic CPS)?", or

"Excluding your time as a temporary worker, contractor, consultant, free lancer, or on-call worker, how long have you worked for (fill: employer's name from the basic CPS)?"

Table 2A
Estimated Retention Rates for All Workers by Tenure Group
Four-Year Spans: 1983-1987 and 1987-1991[1]

Time Span	Current Tenure Group	Raw Data	Deheaped	Deheaped and Business Cycle Adjustment
83-87	0 to <2	.425	.344	.329
	2 to <9	.554	.611	.586
	9 to <15	.777	.861	.827
	15 plus	.682	.653	.630
	Total	.566	.561	.539
87-91	0 to <2	.422	.348	.346
	2 to <9	.508	.552	.548
	9 to <15	.747	.821	.816
	15 plus	.724	.706	.702
	Total	.545	.539	.536
Δ 83-87 to 87-91	0 to <2	-.002	.004	.016*
	2 to <9	-.046*	-.059*	-.038*
	9 to <15	-.031*	-.040*	-.010
	15 plus	.042*	.053*	.072*
	Total	-.021*	-.021*	-.002

[1] * denotes that the estimated difference in the retention rates is significant at the 5 percent level.

Table 2B
Estimated 1991-1995 Continuous Four-Year Retention Rates
A Comparison of Adjustment Methods[1]

Time Span	Current Tenure Group	Lower Bound			Upper Bound			
		1995 total tenure	1995 tenure adjusted to continuous using 1996 CPS	1995 tenure adjusted to continuous using PSID	1995 total tenure	1995 tenure adjusted to continuous using 1996 CPS	1995 tenure adjusted to continuous using PSID	1996 CPS substituted for 1995
91-95	0 to <2	.396	.391	.382	.396	.391	.384	.387
	2 to <9	.572	.564	.548	.574	.566	.553	.593
	9 to <15	.758	.748	.746	.768	.758	.745	.757
	15 plus	.641	.633	.605	.654	.646	.624	.656
	Total	.551	.544	.531	.555	.548	.536	.559
Δ 87-91 to 91-95	0 to <2	.048*	.045*	.037*	.048*	.045*	.038*	.041*
	2 to <9	.021*	.016*	.000	.022*	.018*	.005	.045*
	9 to <15	-.064*	-.069*	-.070*	-.053*	-.058*	-.071*	-.060*
	15 plus	-.065*	-.070*	-.097*	-.052*	-.056*	-.079*	-.046*
	Total	.012*	.008*	-.006*	.016*	.012*	-.001	.022*
Δ 83-87 to 91-95	0 to <2	.053*	.061*	.053*	.053*	.061*	.055*	.058*
	2 to <9	-.039*	-.021*	-.037*	-.037*	-.020*	-.032*	.007
	9 to <15	-.103*	-.079*	-.081*	-.093*	-.069*	-.082*	-.070*
	15 plus	-.012	.003	-.025*	.001	.016	-.006	.026*
	Total	-.010*	.005	-.008*	-.006	.009*	-.003	.020*

[1] All retention rates are adjusted for heaping and business cycle effects. * denotes that the estimated difference in the retention rates is significant at the 5 percent level.

Table 3
Estimated Retention Rates for All Workers by Tenure Group
Eight-Year Spans: 1983-1991 and 1987-1995[1]

Time Span	Current Tenure Group	Retention Rate (with 1995 total tenure)	Retention Rate (continuous tenure)[2]
1983 - 1991	0 to <2	.174	.174
	2 to <9	.413	.413
	9 to <15	.622	.622
	15 plus	.466	.466
	Total	.368	.368
1987 - 1995	0 to <2	.213	.205
	2 to <9	.379	.361
	9 to <15	.609	.574
	15 plus	.472	.446
	Total	.365	.347
Δ 83-91 to 87-95	0 to <2	.039*	.031*
	2 to <9	-.034*	-.052*
	9 to <15	-.013	-.047*
	15 plus	.006	-.020*
	Total	-.003	-.021*

[1] All retention rates are adjusted for heaping and business cycle effects. * denotes that the estimated difference in the retention rates is significant at the 5 percent level.

[2] 1995 total tenure is adjusted to continuous tenure using the 1996 CPS; the lower-bound tenure variable from the 1995 CPS is used.

Table 4
Estimated Retention Rates for Selected Subgroups
Four-Year Spans: 1983-1987, 1987-1991 and 1991-1995[1]

		Time Span			Difference Between Spans		
Specification	**Current Tenure Group**	**83-87**	**87-91**	**91-95**	**Δ 83-87 to 87-91**	**Δ 87-91 to 91-95**	**Δ 83-87 to 91-95**
Panel A Race							
White	0 to <2	.322	.347	.384	.025*	.037*	.062*
	2 to <9	.579	.546	.564	-.034*	.019*	-.015*
	9 to <15	.818	.819	.740	.001	-.079*	-.078*
	15 plus	.628	.695	.640	.067*	-.055*	.012
	Total	.532	.535	.542	.004	.007*	.011*
Black	0 to <2	.383	.342	.418	-.040*	.075*	.035
	2 to <9	.644	.569	.549	-.074*	-.021	-.095*
	9 to <15	.892	.799	.852	-.093*	.053	-.040
	15 plus	.661	.757	.594	.096*	-.163*	-.067*
	Total	.605	.554	.553	-.050*	-.002	-.052*
Panel B Sex							
Male	0 to <2	.353	.361	.394	.007	.033*	.040*
	2 to <9	.637	.566	.584	-.071*	.018*	-.053*
	9 to <15	.861	.846	.786	-.015	-.060*	-.075*
	15 plus	.660	.705	.633	.045*	-.072*	-.028*
	Total	.585	.565	.564	-.020*	-.001	-.021*
Female	0 to <2	.305	.331	.388	.026*	.056*	.082*
	2 to <9	.534	.529	.544	-.005	.015	.009
	9 to <15	.782	.780	.705	-.002	-.075*	-.077*
	15 plus	.557	.697	.633	.140*	-.064*	.076*
	Total	.484	.503	.521	.020*	.018*	.037*
Panel C	**Current Age Group**						
All Workers	16 to 24	.305	.281	.292	-.025*	.011	-.014
	25 to 39	.585	.577	.573	-.009	-.004	-.012*
	40 to 54	.686	.683	.674	-.004	-.009	-.012
	55 plus	.469	.468	.451	-.000	-.018	-.018
	Total	.539	.536	.544	-.002	.008*	.005

[1] All retention rates are adjusted for heaping and business cycle effects. * denotes that the estimated difference in the retention rates is significant at the 5 percent level. 1995 total tenure is adjusted to continuous tenure using the 1996 CPS; the lower-bound tenure variable from the 1995 CPS is used.

Table 5
Estimated Retention Rates for Selected Subgroups
Eight-Year Spans: 1983-1991 and 1987-1995[1]

Specification	Current Tenure Group	83-91	87-95	Δ 83-91 to 87-95
Panel A				
Race				
White	0 to <2	.172	.205	.032*
	2 to <9	.409	.357	-.051*
	9 to <15	.605	.581	-.025
	15 plus	.464	.447	-.017
	Total	.363	.347	-.016*
Black	0 to <2	.177	.194	.016
	2 to <9	.453	.407	-.046*
	9 to <15	.739	.536	-.202*
	15 plus	.496	.449	-.046
	Total	.418	.361	-.058*
Panel B				
Sex				
Male	0 to <2	.190	.228	.039*
	2 to <9	.455	.380	-.075*
	9 to <15	.679	.614	-.065*
	15 plus	.475	.444	-.031*
	Total	.408	.375	-.033*
Female	0 to <2	.158	.182	.024*
	2 to <9	.370	.340	-.030*
	9 to <15	.545	.525	-.020
	15 plus	.444	.451	.006
	Total	.322	.315	-.007
Panel C	**Current Age Group**			
All Workers	16 to 24	.181	.151	-.030*
	25 to 39	.434	.397	-.037*
	40 to 54	.503	.475	-.029*
	55 plus	.179	.184	.005
	Total	.368	.347	-.021*

[1] All retention rates are adjusted for heaping and business cycle effects. * denotes that the estimated difference in the retention rates is significant at the 5 percent level. 1995 total tenure is adjusted to continuous tenure using the 1996 CPS; the lower-bound tenure variable from the 1995 CPS is used.

Appendix Table 1
Classification of Workers in the February 1995 Contingent Worker Supplement

Type 1: Incorporated Self-Employed (3.87%)

Identified by: S8IC="Something Else"

Type 2: Independent Contractor (.98%)

Identified by: S1=Y, S7=Y or S8IC=Independent contractor/Independent consultant/Free-lance worker

Type 3: Temporary Worker, No Agency (3.29%)

Identified by: S1=Y, S2=N, S4=N, S5=N, S6=N or S7EL=Y

Type 4: Temporary Worker in an Agency (.94%)

Identified by: S1=N, S2=Y or S1=Y, S2INS=Y

Type 5: Contractor with Employer (0%)

Identified by: S1=Y, S6=Y

Type 6: On-Call Worker (1.40%)

Identified by: S1=Y, S4=Y

Type 7: Day Laborer (0%)

Identified by: S1=Y, S5=Y

Type 8: Non-Contingent Worker (89.51%)

Identified by: S1=N, S1SCR=Y, S2=N, S4=N, S6=N, S7EL=N, S7=N, S28=N (95.4%) or, if S28=Y (4.6%)

S1: Some people are in temporary jobs that last only for a limited time or until the completion of a project. Is your job temporary?

S1SCR: Provided that the economy does not change and your job performance is adequate, can you continue to work for your current employer as long as you wish?

S2INS: Are you paid by a temporary help agency?

S2: Even though you told me your job is not temporary, are you paid by a temporary help agency?

S4: Some people are in a pool of workers who are ONLY called to work as needed, although they can be scheduled to work for several days or weeks in a row, for example substitute teachers, and construction workers supplied by a union hiring hall. These people are sometimes referred to as ON-CALL workers. Were you an ON-CALL worker last week?

S5: Some people get to work by waiting at a place where employers pick up people to work for a day. These people are sometimes called DAY LABORERS. Were you a DAY LABORER last week?

S6: Some companies provide employees or their services to others under contract. A few examples of services that can be contracted out include security, landscaping, or computer programming. Did you work for a company that contracts out you or your services last week?

S7EL: Are you paid by an employee leasing company?

S7: Last week, were you working as an independent contractor, an independent consultant, or a free-lance worker? That is, someone who obtains customers on their own to provide a product or service.

S8IC: Are you self employed as an independent contractor, independent consultant, free-lance worker, or something else? (1 = independent contractor / independent consultant / free-lance worker, 2 = something else)

S28: Did you EVER work as a temporary worker, contractor, consultant, free-lancer, or on-call worker for (fill: employer's name from basic CPS)?

Appendix Table 2A
The Difference in Years Between Total and Current Tenure by Tenure Group in the 1996 CPS

Years of Total Tenure	Proportion (continuous tenure < total tenure)	Distribution of the difference in years, conditional on total tenure greater than continuous tenure			
		12.5 Centile	37.5 Centile	62.5 Centile	87.5 Centile
0-11 months	.000	1	1	1	1
1	.049	1	1	2	1
2	.065	1	2	3	2
3	.069	1	2	3	3
4	.065	1	2	4	4
5	.082	1	2	4	5
6-7	.083	1	2	5	6
8-9	.096	1	3	5	7
10-11	.087	1	3	7	9
12-14	.123	1	4	9	11
15-17	.085	1	4	8	14
18-20	.096	1	3	8	15
21-23	.092	1	3	8	18
24-29	.096	1	4	5	23
30+	.076	2	7	22	29

Appendix Table 2B
The Difference in Years Between Total and Current Tenure by Tenure Group in the PSID 1988-1992[1]

Years of Total Tenure	Proportion with total greater than current tenure	Distribution of the difference in years, conditional on total tenure greater than current tenure			
		12.5 Centile	37.5 Centile	62.5 Centile	87.5 Centile
0-11 months	0	---	---	---	---
1	.089	1	1	1	1
2	.196	1	1	1	1
3	.243	1	1	1	2
4	.226	1	1	1	3
5	.250	1	1	1	5
6-7	.258	1	1	1	5
8-9	.261	1	1	1	6
10-11	.291	1	1	1	8
12-14	.357	1	1	1	7
15-17	.283	1	1	1	10
18-20	.291	1	1	1	5
21-23	.254	1	1	1	11
24-29	.260	1	1	1	13
30+	.274	1	1	5	26

[1] The differences are calculated from deheaped total tenure and actual continuous tenure in 1988-1992 PSID data.