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Source: *Journal of Labor Economics*, Apr., 1997, Vol. 15, No. 2 (Apr., 1997), pp. 206-233

Published by: The University of Chicago Press on behalf of the Society of Labor Economists and the NORC at the University of Chicago

Stable URL: https://www.jstor.org/stable/2535388

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Two key attributes of a job are its wage and its duration. Much has been made of changes in the wage distribution in the 1980s, but little attention has been given to job durations since Hall. We fill this void by examining the temporal evolution of job retention rates in U.S. labor markets, using data assembled from the sequence of Current Population Survey job tenure supplements. There have been relative declines in job stability for some of the groups that experienced the sharpest declines in relative wages. However, we find that aggregate job retention rates have remained stable.

I. Introduction

How stable are jobs in the U.S. economy? Has stability changed over time, and, if so, what is the nature of the change and which groups

Craig Hakkio provided helpful comments as did seminar participants at the University of Pennsylvania, the National Center on the Educational Quality of the Workforce, and the U.S. Department of Education, but we bear full responsibility for the outcome. We gratefully acknowledge support from the National Center on the Educational Quality of the Workforce, the National Science Foundation, the Sloan Foundation, and the University of Pennsylvania Research Foundation. Neumark's research was partly supported by National Institute of Aging grant 1-K01-AG00589. We thank Daniel Hansen for outstanding research assistance, Kenneth Swinnerton, Howard Wial, and Stephen Rose for sharing their programs, and Bob McIntire for helpful discussions.

[*Journal of Labor Economics*, 1997, vol. 15, no. 2]

0734-306X/97/1502-0006$02.50

have been most affected? Very little is known about these fundamental questions because little previous research exists.[1] This lack of research is particularly surprising considering the fact that the wage and stability of a job are certainly two of its key attributes. Moreover, the distribution across workers of one of those attributes (the wage) changed dramatically in the 1980s and has been subject to intense scrutiny.

The plausibility of declining stability is suggested by the dramatic changes in the distribution of wages that occurred in the 1980s. The salient changes are the deterioration of the relative wages of young and less-educated workers, an end to the convergence of black and white wages, and a closing of the gender gap in wages (e.g., Burtless 1990; Bound and Freeman 1992; Murphy and Welch 1992; Wellington 1993). Much research suggests that the bulk of these changes can be explained by changes in the relative demand for skilled workers (Blackburn, Bloom, and Freeman 1990; Katz and Murphy, 1992; Berman, Bound, and Griliches 1994). But there is also some work suggesting increased bifurcation of the U.S. labor market into "good jobs" and "bad jobs," with an associated disappearance of stable, relatively high-pay employment for the middle and lower-middle classes (Bluestone and Harrison 1986, 1988). This view is also reinforced by innumerable media reports of increased job turbulence (for a review, see Marcotte 1996).

In this article, we directly examine the temporal evolution of job stability in U.S. labor markets. We use data assembled from the sequence of Current Population Survey (CPS) tenure supplements, issued periodically by the Bureau of the Census, that ask workers how long they have been with their current employer or at their current job. In addition to the information collected from the supplements, we use demographic characteristics and other variables available from the general CPS administered to the same individuals.

Our empirical procedures for examining changes in job retention rates build on Hall's (1972, 1982) seminal work on estimating the distribution of eventual job tenure and Ureta's (1992) extensions of Hall's work. However, if the survival function is not stable, estimating the distribution

[1] Exceptions—many of which have appeared since the initial version of our study—include Farber (1993), Marcotte (1996), Boisjoly, Duncan, and Smeeding (1994), and Gottschalk and Moffitt (1994). Marcotte examines PSID data for the 1970s and 1980s while Farber and Boisjoly et al. examine layoffs and plant closings. Gottschalk and Moffitt study changes in stability of weeks of work. There is also evidence consistent with some firms or industries relying increasingly on part-time or nonpermanent workers (Belous 1989; Abraham 1992), although this evidence does not address trends among random samples of workers. The two studies closest to ours are Swinnerton and Wial (1995), which was done contemporaneously and independently, and Rose (1995), which was done subsequently. We discuss both of these studies in greater detail below.

of eventual job tenure from the CPS is problematic. Therefore, our research questions the assumptions and findings of their earlier work.

In Section II, we discuss the data used, our procedure for estimating retention rates, and the adjustments made in order to remove the effects of rounding, heaping, and business cycles. We present our estimates of retention rates in Section III, both in total and disaggregated by age, race, sex, education, occupation, and current tenure. We compare our results with others' in Section IV, and we conclude in Section V.

II. Data and Methods

Estimating Retention Rates

A concept central to our approach is the t-year *retention rate,* $R(t)$, which gives 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 demographic groups or workers with particular initial tenure levels. Denoting current tenure by c, and other characteristics by x, we write the t-year retention rate as $R_{xc}(t)$. We refer to a sequence of retention rates, $R_{xc}(t), t = 1, 2, \ldots$, as a *survival function.* The survival function provides a complete characterization of the probability distribution of eventual tenure.[2] Because the survival function and the probability distribution of eventual tenure contain precisely the same information, analysis may be based on either. From this point on, we work exclusively with the survival function.

Hall (1982) uses the 1978 CPS tenure supplement and concludes that expected U.S. job tenure is long, insignificantly different for blacks and whites, and significantly shorter for women. The estimation of tenure distributions is complicated because the CPS data capture incomplete spells (as would any nonretrospective data), that is, completed tenure-to-date for people's current jobs. Because of this, the distribution of eventual tenure cannot be observed, but only inferred by estimating the survival function for employment.

Hall's estimation of the survival function from a single tenure supplement (i.e., from a cross section) requires two assumptions. The first assumption, which is particularly germane to our research, is that the employment survival function is stable over time. This assumption allows one to infer, for example, the probability that an employed 35-year-old with 0 years of tenure will accumulate at least 10 more years of tenure (the 10-year retention rate for 35-year-olds) from data on 45-year-olds

[2] The probability distribution of eventual tenure is obtained from the survival function as follows: For a given level of current tenure, let $P_{xc}(t)$ denote the probability that additional tenure is greater than or equal to t years but less than $t + 1$ years. Then $P_{xc}(t) = R_{xc}(t) - R_{xc}(t + 1)$.

with 10 years of tenure at the same point in time. In particular, this probability is estimated as the ratio of the number of 45-year-olds with 10 years of tenure to the number of 35-year-olds with 0 years of tenure.

As Ureta (1992) points out, estimation of the survival function from cross-sectional data also requires a second assumption: that the overall arrival rate (the number of workers beginning new jobs) is constant. Hall corrects only for changes in arrivals due to cohort size variation, but Ureta emphasizes that this is insufficient because the period for which Hall was estimating the distribution of eventual job tenure witnessed large changes in labor force participation rates of women and older men, *changes that also affected the arrival rate. To see the nature of the problem,* note that if current 35-year-olds have a higher participation rate than current 45-year-olds, then the ratio of employed 45-year-olds with 10 years of tenure to employed 35-year-olds with 0 years of tenure will understate the 10-year retention rate for current 35-year-olds with 0 years of tenure.

Ureta develops a method of estimating the survival function that does not require stable participation rates. This method requires the use of multiple CPS tenure supplements to calculate historical retention rates.[3] By linking a few supplements, Ureta can estimate *historical* 1-year retention rates as the ratio of the number of individuals with $c + 1$ years of tenure in year $j + 1$ to the number of individuals with c years of tenure in year j. Historical retention rates are independent of changes in arrival rates because they are calculated from data on the same cohort at two points of time. To return to our previous example, the ratio of employed 45-year-olds with 10 years of tenure in, say, 1983 to employed 35-year-olds with 0 years of tenure in 1973 will not understate the 10-year retention rate for the latter group, even if participation rates are rising for this cohort, because none of the new labor market entrants (arrivals) between 1973 and 1983 can accumulate 10 years of tenure by 1983.

However, like Hall, Ureta must assume a stable survival function for two reasons. First, she estimates 1-year retention rates using tenure supplements that are more than 1 year apart. Second, she uses the estimated 1-year retention rates to estimate eventual tenure distributions.

[3] Ureta (1992) uses CPS tenure supplements for 1978, 1981, and 1983, ignoring (although acknowledging) the change in the tenure question after 1981 that we discuss below. Hall (1982) notes that retention rates can also be calculated historically by stringing together tenure supplements. In fact, he compares some crude calculations of retention rates from cross-sectional and historical data. Because he is interested only in a qualitative characterization of the long-term nature of jobs in the U.S. economy, he does not focus on the differences that emerge from the two procedures as both indicate the same qualitative characteristic of relatively long-term job attachment.

Here we dispense with the assumption of a stable survival function. In fact, our goal is to assess whether the survival function *is* stable. As Ureta's work makes clear, we cannot simultaneously avoid this assumption while providing a complete characterization of eventual tenure distributions. But we can avoid the assumption by linking together a longer sequence of CPS tenure supplements and using them to characterize tenure distributions based only on observed historical retention rates. The CPS tenure supplements are available for 1973, 1978, 1981, 1983, 1987, and 1991.[4] Conditional on any characteristic, we can estimate the t-year retention rate for the span of years, t, between any two supplements.

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, $N_x^{0+t}(t + c)$, to the number of workers with c years of tenure in the current tenure supplement, $N_x^0(c)$, where the superscripted 0 refers to the current year's supplement. Formally,

$$\hat{R}_{xc}(t) = \frac{N_x^{0+t}(t + c)}{N_x^0(c)} .$$

Retention rates can be estimated for any subgroup consistently represented across surveys. We shall classify by age, sex, education, race, industry, occupation, and current tenure.

Comparing Retention Rates

The central issue of this article is whether job stability has remained stable. To investigate this issue we simply compare retention rates. For reliable comparisons, retention rates should have the same meaning and span length. Unfortunately, the wording of the question on job tenure from the CPS tenure supplements has changed over time. In 1973, 1978, and 1981, the question referred to time working at the present job or business, while in 1983, 1987, and 1991, the question referred to time working (continuously) for the present employer (see table 1A). There is no decisive way to detect whether respondents interpreted the questions differently. Thus, to be safe, we are careful to focus on changes in retention rates that are not influenced by changes in the wording of the tenure question.

In table 1B, we show the set of spans for which we can directly estimate retention rates, distinguishing the question type used at the beginning

[4] Tenure supplements were also carried out in 1963, 1967, and 1969, but the microdata from these supplements are apparently not available in machine-readable form.

Table 1
Job Tenure Information on the Current Population Survey Tenure Supplements
A. Job Tenure Questions

Question type I (January 1973, January 1978, and January 1981): "When did . . . start working at his present job or business?"
Question type II (January 1983, January 1987, and January 1991): "How long has . . . been working continuously for his present employer (or as self-employed)?"

B. Spans between CPS Tenure Supplements by Question Type

	Year Span Begins				
	Question Type I			Question Type II	
Year Span Ends	1973	1978	1981	1983	1987
Question type I					
1978	5	. . .	. . .	. . .	. . .
1981	8	3	. . .	. . .	. . .
Question type II					
1983	10	5	2	. . .	. . .
1987	14	9	6	4	. . .
1991	18	13	10	8	4

C. Span Matches by Length and Question Type

	Question Types in Span		
Length of Span	Question Type: I to I	Question Type: II to II	Question Type: I to II
4 years	. . .	83–87, 87–91	. . .
5 years	73–78	. . .	78–83
8 years	73–81	83–91	. . .
10 years	. . .	. . .	73–83, 81–91

NOTE.—In part B, units represent the span length in years. In part C, units represent the span, identified by its first and last year.

and end of each span. The entries indicate the span, *t*, for which the retention rate can be estimated, and the year at the top of each column indicates the year from which all rates can be estimated. The span measures the number of years between the year at the top of each column to some future year listed on the left for which a supplement is available. Thus, for example, the 5 in the upper-left-hand corner indicates that we can directly estimate the 5-year retention rate for 1973.

In table 1C, we display the retention rates that can be compared over time by length of span and question type. However, changes in the tenure question limit the usefulness of some comparisons. The most consistent comparison is that of 4-year retention rates for 1983 and 1987, because it relies on a consistent tenure question. Comparisons of the 5- and 8-year retention rates (indicated in table 1B) are problematic because we cannot identify true changes in isolation from changes due to different question types. However, despite changes in the tenure question, a comparison of 10-year retention rates for 1973 and 1981 is possible. For each of these years, retention rates are based on the early tenure question in the initial year and the later tenure question in the final year. While estimates of retention rates for either 1973 or 1981 may be biased because of the change in the question, we can obtain an unbiased estimate of the change in the retention rate from 1973 to 1981 if the estimated retention rate for each year is equally biased. Because of these potential problems with the estimated 10-year retention rates, however, we focus more on the 4-year rates.

We study nonagricultural workers, currently working or with a job but not currently at work, aged 16 or older.[5] Because the retention rate estimate is based on comparing the number of workers between different CPS samples, we need the sample of workers from each CPS to be representative of the population in order to insure unbiased comparisons. The CPS is ideal for this type of comparison because it is a random sample representative of the U.S. population, with weights already provided for each sampled individual. We find, however, that nonresponse to the tenure supplement can vary across years (and in fact does so quite substantially) and can vary differently based on demographic or other characteristics. Thus, we adjust the standard CPS sample weight 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.[6]

Rounding and Heaping

The empirical probability distributions of reported tenure for each of the six supplements are shown by the bars in figure 1. The rough shape of these distributions is the same in each of the 6 supplement years; the highest proportion reporting tenure in the range of 0–1 year and the proportion declining nearly monotonically in subsequent years.[7] However, the empirical distributions reveal some other features.

[5] Exclusion of agricultural workers is routinely done, e.g., in the wage distribution literature that this article seeks to complement. In the final section of the article we show that our main result is insensitive to the inclusion of agricultural workers.

[6] The CPS sample weight is the reciprocal of the probability of being sampled, adjusted for noninterview and variation in the sampling of race, age, sex, and residence subgroups.

[7] Our convention is that when a tenure interval is specified, the first value is included in the interval and the second excluded. For example, 3–6 means $3 \leq \text{tenure} < 6$.

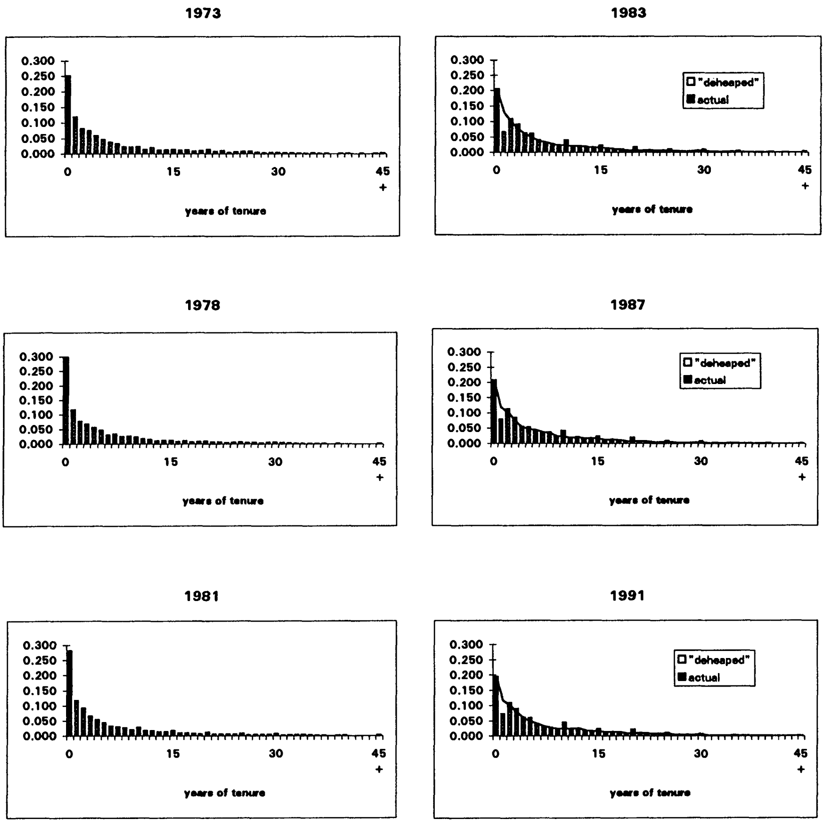

FIG. 1.—Frequency distributions of job tenure. "Deheaped" refers to the data adjusted for heaping and half-year rounding.

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 the 1983 and subsequent surveys, the tenure question asks how long a person has worked for his 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 years of tenure rather than 1. In contrast, before 1983, the survey asks for the year in which the spell of tenure began, so this problem does not arise.[8] The empirical probability distributions are consistent with this rounding problem as the proportion reporting tenure of 1–2 years in 1983, 1987, and 1991 is roughly one-half of the proportion reporting tenure of 1–2 years in 1973, 1978, and 1981.[9]

A second feature of the empirical tenure distributions is that the 1983, 1987, and 1991 distributions have spikes at 5, 10, 15, 20, 25, and 30 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. In contrast, for the three earlier supplements, in which respondents reported the year they began the tenure spell, any such heaping is much less evident.

We adjust the data for rounding and heaping by estimating a mixture model for reported tenure that 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 five with probability $1 - p$.[10] The corresponding Weibull survival function is $\exp[-(\alpha t')^{\beta}]$, where t' denotes true tenure. 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 $\delta < 0$.[11]

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 θ. First, we divide the possible values of reported tenure into J cells and then we find the values of the parameters that minimize $\Sigma_{j=1}^{J}[(O_j - E_j)^2/E_j]$, where O_j is the actual number of

[8] For example, if a respondent to the January 1981 supplement began working 1.5 years ago, he responds with 1979, and tenure will be coded as 1 year (i.e., 1–2 years).

[9] Another apparent difference between the first 3 and the last 3 years is that for the former, the spike at 0–1 year is larger. This may be attributable to the wording of the tenure question. Individuals may regard themselves as having changed jobs but not employers, which would put more mass at low levels of tenure defined for the job instead of the employer.

[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 increasing and decreasing hazard functions as well as a flat hazard function in the nested exponential case. Second, the Weibull mixture model is used purely as a sophisticated smoothing device, and a comparison of the original and adjusted tenure distributions in fig. 1 (discussed below) reveals that apart from the valleys and peaks corresponding to rounding and heaping, which of course do not appear in the adjusted distributions, the adjusted and unadjusted distributions agree closely.

[11] The estimates of γ and δ imply that $0 \leq p \leq 1$.

observations in cell j and E_j is the expected number of observations given the parameters.[12]

We use the estimates of the parameters of the mixture model to adjust the data for 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.[13]

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 $1 - \theta$. We then shift reported tenure down by 1 year for the proportion estimated to have rounded up by 1 year.[14]

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.[15] To correct for this

[12] The cells used are 0, 1–2, 3–4, 5, 6–9, 10, 11–14, 15, 16–19, 20, and 21 or more.

[13] Because the tendency to heap appears to be approximately three times more likely at multiples of five ending in zero (i.e., multiples of 10) than multiples ending in five, we use the following adjacent values: for multiples ending in five, we define the adjacent values as 1 year less or 1 year more of tenure (e.g., for 5 years of tenure, we use four and six); for multiples ending in 10, we define the adjacent values as 1–3 years less and 1–3 years more.

[14] We estimated the mixture model separately for each year for which we have data. For the 1983, 1987, and 1991 tenure supplements, the estimates of α ranged from .22 to .23 while those of β ranged from .73 to .80. 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 .975 to .999 and the estimates of δ were $-.009$. Finally, the estimates of θ ranged from .35 to .44, consistent with our conjecture that roughly one-half of respondents round reported tenure upward. As we would expect based on fig. 1, corrections for heaping and rounding were of little consequence for the three earlier tenure supplements, and we therefore use the original data.

[15] 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.

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.[16] 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 retention rate via

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

Clearly, if unemployment flows were always on trend (i.e., $U_x(m) = 0$ 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 aver*age deviation of changes in the unemployment rate from trend over* the span.

III. Results

Four-Year Retention Rates: All Workers

In table 2, we report estimates of 4-year retention rates for 1983 and 1987 for workers classified by initial tenure. These estimates are reported for each stage of adjustment made to the tenure data: unadjusted, adjusted for heaping, and adjusted for business-cycle effects.[17] We also display the change in retention rates between the 1983 and the 1987 spans and a test of the significance of the change.[18] Aggregate retention rates and retention rate changes are reported in the last row of each panel in table 2. The unadjusted economywide retention rate, reported in the first two columns of table 2A, decreased from .576 to .557 between the 1983 and the 1987 spans.

We also report retention rate estimates in table 2A using data adjusted

[16] The variable m indexes time periods (months), as will become clear.

[17] From this point onward, we use "heaping" to refer to both heaping and rounding.

[18] We obtain z-tests by exploiting the asymptotic normality of the estimated retention rates, which are simply relative frequencies, as discussed in Hogg and Craig (1978, pp. 215–22). In particular,

$$\hat{R}_{xc}(t) \overset{a}{\sim} N\left\{ R_{xc}(t), \frac{R_{xc}(t)[1 - R_{xc}(t)]}{N_x^0(c)} \right\}.$$

Table 2
Estimated Retention Rates for 4-Year Spans: 1983–87 and 1987–91
A. With and Without Heaping Adjustment, by Years of Current Tenure

Years of Current Tenure	Unadjusted Retention Rates 1983–87	Unadjusted Retention Rates 1987–91	Unadjusted Retention Rates Change	With Heaping Adjustment Retention Rates 1983–87	With Heaping Adjustment Retention Rates 1987–91	With Heaping Adjustment Retention Rates Change
0 to <1	.294	.303	.009	.294	.302	.008
1 to <2	.863	.792	−.071*	.438	.429	−.009
2 to <3	.426	.382	−.044*	.472	.417	−.055*
3 to <4	.427	.379	−.048*	.541	.451	−.090*
4 to <5	.655	.542	−.113*	.619	.523	−.096*
5 to <6	.406	.375	−.031	.565	.542	−.023
6 to <7	1.136	1.062	−.074*	.666	.606	−.060*
7 to <8	.552	.623	.071*	.684	.649	−.035*
8 to <9	.839	.678	−.161*	.871	.758	−.113*
9 to <10	.806	.812	.006	.783	.795	.012
10 to <12	.741	.710	−.031*	.878	.860	−.018
12 to <14	.724	.677	−.047*	.814	.785	−.029*
14 to <16	.589	.598	.009	.719	.694	−.025
16 to <20	.812	.830	.018	.768	.801	.033*
21+	.725	.770	.045*	.661	.672	.011
All	.576	.557	−.019*	.554	.530	−.024*

B. With and Without Business-Cycle Adjustment, by Current Tenure Group (All Retention Rates Adjusted for Heaping)

Current Tenure Group	Without Business-Cycle Adjustment Retention Rates 1983–87	Without Business-Cycle Adjustment Retention Rates 1987–91	Without Business-Cycle Adjustment Retention Rates Change	With Business-Cycle Adjustment Retention Rates 1983–87	With Business-Cycle Adjustment Retention Rates 1987–91	With Business-Cycle Adjustment Retention Rates Change
0 to <2	.346	.348	.002	.335	.346	.011*
2 to <9	.586	.525	−.061*	.568	.522	−.046*
9 to <15	.832	.810	−.022*	.806	.805	−.001
15+	.692	.717	.025*	.671	.712	.041*
All	.554	.530	−.024*	.537	.527	−.010*

* Difference in retention rates is statistically significant at the 5% level.

for heaping. The results for retention rate changes between spans are qualitatively similar, with an estimated decline of .024. The effect of the heaping adjustment is that it smooths retention rates between years of current tenure. For example, the 1983 unadjusted retention rate for 6 to <7 years of current tenure is 1.136, which is theoretically impossible, while the adjusted retention rate is .666, which is consistent with estimated retention rates for workers with similar years of current tenure.

In table 2B, we report retention rates (all adjusted for heaping) by four broader tenure groups. The first three columns report results unadjusted for business cycles. The tenure groups (0 to <2, 2 to <9, 9 to <15, and 15+) are chosen to simplify the presentation by combining workers experiencing similar changes between spans. For example, the retention rate decrease of −.061 in the 2 to <9 tenure group is similar to the retention rate change for each tenure level in that group shown in the last column of table 2A. The differences in changes in retention rates between tenure groups are notable. The large decline for the 2 to <9 group stands in sharp contrast to the increase for the 15+ group.[19]

In the fourth and fifth columns of table 2B, we report retention rates estimated from deheaped data that have also been adjusted for business-cycle effects. The results by tenure group are qualitatively similar. The business-cycle adjustment results in a decline in the 1983–1987 economywide retention rate, which reflects the correction for the expansionary period of that span. The absolute change in the aggregate retention rate is smaller, −.010 as opposed to −.024.[20]

In order to provide additional perspective, as a back-of-the-envelope calculation, we transform our estimates of retention rates into estimates of expected job tenure, under the assumption that the survival function may shift across time spans but is otherwise stable (e.g., across different workers). Using exponential survival functions, the drop in the average retention rate from the 1983–87 span to the 1987–91 span corresponds only to a small change in average job tenure from 6.8 years to 6.3 years.[21] For the estimates corrected for business cycles, the drop is from 6.4 years to 6.2 years.

Four-Year Retention Rates: Demographic and Job Characteristics

In table 3 we provide additional evidence on changes in job retention rates, reporting estimated 4-year retention rates for subgroups of workers, for some of whom previous research has documented changes in relative wages. The total retention rate for each subgroup is presented for the heaping-adjusted estimates with and without the business cycle correction. For each subgroup, retention rates are shown by current tenure group or age group adjusted for heaping but not business cycles.

The first estimates in table 3A are by age group. The younger the worker the greater the decline in retention rates between the 1983 span and the 1987 span. For the 16–24 age group the retention rate fell by .043 while the 55+ age group experienced a small (and insignificant) decline of .013. The totals adjusted for business cycles show a similar

[19] While we can only speculate, it may be the declines in job stability for the 2 to <9 tenure group that underlie the apparent public perception of declining job stability. Nonetheless, our estimates indicate that this perception is inaccurate with respect to aggregate changes and with respect to very long-term job holders.

[20] If we instead detrend using linear and quadratic time variables, we obtain virtually identical results. For example, the overall retention rate is estimated to decline by −.009.

[21] The exponential survivor function is $e^{-\lambda t}$, for which the expected duration is $1/\lambda$. Our 4-year retention rate is an estimate of $e^{-4\lambda}$, which we solve for λ.

pattern, but only the younger workers experience a significant decline. The retention rate drop for the 2 to <9 tenure group is present across all age groups while the rise for the 15+ tenure group is significant only for workers aged 40–54. Although the differences in the changes in retention rates by age group are not dramatic, it is nonetheless interesting that the decline is greatest for young workers, who also experienced the largest relative wage decreases.

Workers with a high school degree at most had a more difficult time retaining their jobs over the 4-year span that started in 1987 than over the 4-year span that started in 1983; our estimated retention rate for these workers drops by .034. The experience of college graduates during the same period was quite different. Their retention rates remain the same and when adjusted for the business cycle, their retention rates rise slightly, by .015. Again, these results parallel changes in the wage structure.

Differences also arise when comparing retention rates between blacks and whites. The decline for blacks is over three times that for whites (.067 vs. .019). The decline for blacks occurs across all tenure groups with a notable exception of the 15+ tenure group, which shows a rise of .060. Retention rate changes differ by gender as well. Males experience a drop of .040 while the change for females is negligible. Although declines for males occur in all tenure groups, the only significant decline occurs in the 2 to <9 group, by .08. The retention rate for females in the highest tenure group rose by .098, from .634 to .732. Adjusting for business-cycle effects leaves the qualitative results by race and gender unchanged. Since relative wages of blacks fell and relative wages of women rose, we once again find changes in job stability corresponding to changes in wages.

Between the 1983 and 1987 4-year spans, the most pervasive change occurred in the 2 to <9 tenure group. Overall, this group's retention rate declined by .061, and across all demographic subgroups in table 3A, the decline remained statistically significant and substantial, ranging from .040 for females to .104 for blacks. On the other hand, for many of the demographic subgroups, retention rates rose for the highest tenure workers (9 to <15 and 15+) as well as for the lowest tenure workers (0 to <2).

In table 3B we show subgroup retention rates by age group instead of tenure. By education group, the youngest workers with a high school education or less experienced the sharpest decline in the retention rate (.062) while the youngest college graduates experienced the sharpest increase (.051), both of which are significant.[22] These results parallel wage

[22] The estimated retention rate levels by education for the younger age groups are somewhat suspect because there are individuals, mostly under 25, whose education status could change over the 4-year span.

Table 3
Estimated Retention Rates of Selected Subgroups for 4-Year Spans: 1983–87 and 1987–91
A. Education, Race, Gender, and Age, by Current Tenure Group

Demographic Group	Current Tenure Group: 0 to <2	2 to <9	9 to <15	15+	Total	Business-Cycle Adjusted Total
Age:						
16–24:						
1983–87	.237	.416	[a]	[a]	.299	.290
1987–91	.208	.352	[a]	[a]	.256	.254
Change	−.029*	−.064*	[a]	[a]	−.043*	−.036*
25–39:						
1983–87	.396	.620	.936	.854	.596	.577
1987–91	.412	.547	.893	.836	.561	.558
Change	.016*	−.073*	−.043*	−.018	−.035*	−.019*
40–54:						
1983–87	.482	.654	.867	.813	.707	.685
1987–91	.485	.607	.784	.837	.679	.675
Change	.003	−.047*	−.083*	.024*	−.028*	−.010
55+:						
1983–87	.390	.532	.537	.501	.502	.487
1987–91	.416	.440	.595	.505	.489	.486
Change	.026	−.092*	.058*	.004	−.013	−.001
Education:						
High school graduate or dropout:						
1983–87	.306	.559	.788	.646	.518	.502
1987–91	.294	.490	.783	.668	.484	.480
Change	−.012*	−.069*	−.005	.022*	−.034*	−.022*
College graduate:						
1983–87	.457	.649	.872	.778	.644	.625
1987–91	.510	.597	.864	.800	.644	.640
Change	.053*	−.052*	−.008	.022	.000	.015*
Race:						
White:						
1983–87	.339	.581	.826	.693	.549	.534
1987–91	.347	.526	.811	.710	.530	.527
Change	.008	−.055*	−.015*	.017*	−.019*	−.007*
Black:						
1983–87	.401	.633	.884	.705	.611	.576
1987–91	.357	.529	.797	.765	.544	.533
Change	−.044*	−.104*	−.087*	.060*	−.067*	−.043*
Gender:						
Male:						
1983–87	.375	.623	.852	.714	.598	.578
1987–91	.361	.543	.844	.710	.558	.554
Change	−.014	−.080*	−.008	−.004	−.040*	−.024*
Female:						
1983–87	.317	.546	.804	.634	.502	.487
1987–91	.335	.506	.766	.732	.496	.494
Change	.018*	−.040*	−.038*	.098*	−.006	.007

Table 3 (*Continued*)

B. Education, Race, and Gender, by Age Group

Demographic Group	Age Group 16–24	25–39	40–54	55+	Total	Business-Cycle Adjusted Total
Education:						
High school graduate or dropout:						
1983–87	.273	.575	.679	.466	.518	.502
1987–91	.211	.529	.650	.456	.484	.480
Change	−.062*	−.046*	−.029*	−.010	−.034*	−.022*
College graduate:						
1983–87	.494	.612	.775	.597	.644	.625
1987–91	.545	.617	.738	.572	.644	.640
Change	.051*	.005	−.037*	−.025	.000	.015*
Race:						
White:						
1983–87	.298	.588	.704	.503	.549	.534
1987–91	.255	.563	.678	.490	.530	.527
Change	−.043*	−.025*	−.026*	−.013*	−.019*	−.007*
Black:						
1983–87	.335	.658	.770	.482	.611	.576
1987–91	.274	.569	.698	.487	.544	.533
Change	−.061*	−.089*	−.072*	.005	−.067*	−.043*
Gender:						
Male:						
1983–87	.348	.643	.736	.517	.598	.578
1987–91	.276	.595	.707	.481	.558	.554
Change	−.072*	−.048*	−.029*	−.036*	−.040*	−.024*
Female:						
1983–87	.249	.537	.670	.481	.502	.487
1987–91	.235	.520	.646	.499	.496	.494
Change	−.014	−.017*	−.024*	.018	−.006	.007

C. Industry and Occupation, by Current Tenure Group

Job Type Group	Current Tenure Group 0 to <2	2 to <9	9 to <15	15+	Total	Business-Cycle Adjusted Total
Industry:						
Goods-producing sector:						
1983–87	.423	.626	.913	.675	.625	.605
1987–91	.410	.569	.832	.686	.587	.583
Change	−.013	−.057*	−.081*	.011	−.038*	−.022*
Service-producing sector:						
1983–87	.318	.565	.781	.709	.517	.501
1987–91	.325	.504	.796	.745	.501	.498
Change	.007	−.061	.015	.036*	−.016*	−.003

Table 3 (*Continued*)

Job Type Group	Current Tenure Group 0 to <2	2 to <9	9 to <15	15+	Total	Business-Cycle Adjusted Total
Occupation:						
Blue-collar workers:						
1983–87	.340	.603	.870	.675	.571	.552
1987–91	.332	.491	.798	.640	.507	.503
Change	−.008	−.112*	−.072*	−.035*	−.064*	−.049*
White-collar and service workers:						
1983–87	.228	.505	.877	.830	.484	.469
1987–91	.212	.487	.880	.899	.477	.473
Change	−.016	−.018	.003	.069*	−.007	.004

NOTE.—All retention rates adjusted for heaping.
[a] Less than 100 observations in the sample.
* Difference in retention rates is statistically significant at the 5% level.

structure changes in which the returns to schooling increased most sharply for young workers (e.g., Blackburn et al. 1990). The changes in each age group by race and gender are, in general, similar to the overall change in the subgroup.

In table 3C, we report estimates of retention rates classified by characteristics of jobs rather than characteristics of workers. The relative decline of blue-collar manufacturing jobs is frequently bemoaned as heralding the disappearance of stable, high-paying jobs for less-educated workers (Bluestone and Harrison 1986, 1988). This classification by industry and occupation provides evidence on the relative stability of alternative jobs and is useful for assessing whether changes in the industrial and occupational composition of the workforce can explain the decreases in retention rates (however small) that have been documented in the previous discussion.

The overall retention rate decreased by .038 in the goods-producing sector and fell by .016 in the service-producing sector. In addition, retention rates were, and still are, higher in goods-producing industries than in service-producing industries, suggesting that shifts away from goods-producing jobs could lead to less stable jobs. Next, the retention rates classified by blue-collar and white-collar/service workers dropped substantially for blue-collar workers and stayed the same for white-collar/service workers. The decline for blue-collar workers is .064, with the greatest drops occurring in the 2 to <9 and the 9 to <15 tenure groups, while for white-collar/service workers the only significant change occurred in the 15+ tenure group where retention rates rose by .069. On the one hand, the lower retention rates for white-collar/service workers with fewer than 9 years of current tenure suggest that workers who moved from blue-collar to service jobs may have entered less stable jobs. On the other hand, long-lasting white-collar/service jobs offer more stability than long-lasting blue-collar jobs, a difference that increased over the 1980s.

A Longer-Term View: 10-Year Retention Rates

To this point, all our results refer to 4-year retention rates for the spans 1983–87 and 1987–91. We devote most of our attention to retention rates for these spans because they can be estimated and compared using a consistent job tenure question, as explained in Section II. However, we also argued that 10-year retention rates for 1973–83 and 1981–91 could be compared meaningfully because for each of these spans we begin with the first form of the tenure question and end with the second. Thus, at a minimum, we should be able to meaningfully compare the changes in 10-year retention rates. We report such changes in table 4.

In table 4A, we report changes in 10-year retention rates by tenure group and overall. Tables 4B and 4C report more disaggregated results. The 10-year retention rates (adjusted for heaping) increased for all but the highest tenure group; the average increase over all workers is .018. Thus, in contrast to the point estimates of 4-year retention rates for the 1980s, which suggest slightly declining job stability, the point estimates of 10-year retention rates for the 1970s and 1980s suggest slightly rising job stability. The differences across 4- and 10-year spans may occur either because 4- and 10-year retention rates changed differently or because the 1970s were different from the 1980s.

The disaggregation of the 10-year spans by demographic and job characteristics also reveals some reversals when compared to the 4-year spans. The 2 to <9 tenure group shows increased stability—although the change is small—and the 15+ group shows decreased stability. The college graduates have slightly reduced retention rates between spans while the dropouts and high school graduates stay the same. Blacks show some increase as opposed to the large decrease they experienced in the 4-year spans over the 1980s, and younger workers fared better than older workers.

One difference between the 2 periods can be traced to business cycles. When the 10-year estimates are adjusted for business-cycle effects the overall rise in retention rates is eliminated and many of the changes for demographic subgroups are considerably reduced.[23] Thus, in both the 4-

[23] In this case, the estimates are slightly different if we detrend nonlinearly, in which case the aggregate 10-year retention rate is estimated to rise by .017 rather than decrease by −.001. Because the unemployment rate was very high in 1982 and 1983, job loss should have contributed to a downward bias in the 1973–83 estimate of the retention rate and, hence, an upward bias in the estimated change in the retention rate from 1973–83 to 1981–91. Consequently, the linear detrending

Table 4
Estimated Retention Rate and Changes between 10-Year Spans: 1973–83 and 1981–91
A. All Workers, by Current Tenure Group

All Workers	Current Tenure Group: 0 to <2	2 to <9	9 to <15	15+	Total
Unadjusted	.007	.029*	.103*	−.010	.019*
Adjusted for heaping	.005*	.037*	.093*	−.016*	.018*
Adjusted for business cycle	−.004	.012*	.063*	−.043*	−.001

B. Education, Race, Gender, Age, Industry, and Occupation, by Current Tenure Group

Demographic Group	Current Tenure Group: 0 to <2	2 to <9	9 to <15	15+	Total	Business-Cycle Adjusted Total
Education:						
High school graduate or dropout	.004	.037*	.052*	−.024*	.011*	−.007*
College graduate	−.038*	−.008	.102*	−.061*	−.015*	−.045*
Race:						
White	.008*	.030*	.080*	−.022*	.014*	−.002
Black	−.030*	.120*	.164*	.047	.062*	.017
Gender:						
Male	.006	.043*	.087*	−.036*	.017*	−.006
Female	.007	.034*	.116*	.046*	.028*	.013*
Age:						
16–24	.013*	.144*	[a]	[a]	.048*	.040*
25–39	−.001	.031*	.086*	.044*	.021*	−.005
40–54	−.019*	−.035*	.077*	−.033*	−.017*	−.044*
55+	−.015	.022*	−.006	.002	.003	−.005
Industry:						
Goods-producing sector	.011	.040*	.084*	−.027*	.019*	−.003
Service-producing sector	.004	.039*	.101*	.007	.022*	.003
Occupation:						
Blue-collar workers	.006	.040*	.076*	−.025*	.020*	.001
White-collar and service workers	−.012	.054*	.176*	.065*	.019*	−.003

C. Education, Race, and Gender by Age Group

Demographic Group	Age Group: 16–24	25–39	40–54	55+	Total	Business-Cycle Adjusted Total
Education:						
High school graduate or dropout	.048*	.016*	−.012	.000	.011*	−.007*
College graduate	.075*	−.003	−.080*	−.020	−.015*	−.045*
Race:						
White	.048*	.015*	−.024*	.006	.014*	−.002
Black	.051*	.091*	.041*	−.013	.062*	.017
Gender:						
Male	.061*	.015*	−.012	.004	.017*	.006
Female	.038*	.047*	−.015	.002	.028*	.013*

NOTE.—All retention rates adjusted for heaping. Retention rate levels are not reported since they are based on job tenure at the beginning of each span and employer tenure at the end.
[a] Fewer than 100 observations in the sample.
* Difference in retention rates is statistically significant at the 5% level.

Table 5
Alternative Estimates of Overall 4-Year Retention Rates

Alternative Estimates	4-Year Retention Rates 1983–87	1987–91
Swinnerton and Wial's published estimates	.55	.49
Our findings	.54	.53
Our unadjusted findings	.58	.56
Our estimates using Swinnerton and Wial's methodology*	.55	.53
Our estimates using Swinnerton and Wial's methodology plus errors detected in their coding†	.55	.49

* Swinnerton and Wial's methodology differs from our unadjusted findings by using a sample which includes agriculture, omits some initial tenure values, and omits unincorporated self-employed.
† Errors detected are omitting the incorporated self-employed in 1991 and failing to adjust for nonresponse to tenure questions in 1991.

and 10-year spans, many of the changes in retention rates are reduced or eliminated after accounting for business-cycle effects.

IV. Comparison of Our Results to Others'

Our results contrast with two recent studies that report strong evidence of declines in job stability. This section reconciles the conflicting evidence.

Swinnerton and Wial

In a contemporaneous and independent study, Swinnerton and Wial (1995) claim to find empirical evidence of declining job stability in the U.S. economy over the 1980s that is based on representative samples from the tenure supplements to the Current Population Survey. Averaging over all workers, they report that the 4-year retention rate fell from .55 to .49 from 1983 to 1987 (see row 1 of table 5) and they conclude that "if the pattern of the late 1980s persists, workers who have stable, long-term jobs will make up an increasingly exclusive club" (p. 304). Although we use the same data sources and address many of the same statistical issues, our estimated 4-year retention rate for all workers declines by only .01, from .54 to .53, just one-sixth the size of the Swinnerton-Wial decline (see row 2 of table 5).

method produces results more consistent with our prior expectations. The estimated change in retention rates for blacks is particularly sensitive to the business-cycle adjustment. The adjusted estimate of the change in the 10-year retention rate is much lower (.017) than the unadjusted estimate (.062). This presumably reflects the adverse consequences of the 1982–83 recession for blacks. This sensitivity suggests that such adjustments may be important for black-white comparisons of job stability, such as those in Ureta (1992) and Hall (1982).

In table 5 we display the sensitivity of our findings to the methods we use.[24] First, of course, we implement corrections for business cycles and heaping, but even our unadjusted results show only a modest decline in retention rates, from .58 to .56 (see row 3). The methods of Swinnerton and Wial differ from our unadjusted results in three other obvious ways; all are related to differences in the samples. First, Swinnerton and Wial include agricultural workers in their calculations of overall retention rates. Second, Swinnerton and Wial omit observations with 8, 13, 23, 28, or 33 years of initial tenure; this is intended to deal with the heaping problem, as explained in their paper. Third, Swinnerton and Wial omit self-employed workers. In row 4 of table 5 we report our results when we include agricultural workers, omit observations with the initial tenure levels listed above, and exclude the unincorporated self-employed. We now obtain the same retention rate (.55) for 1983–87, but the retention rate for 1987–91 is .53, which contrasts with their estimate of .49. Thus, even when we mimic the sample of workers considered by Swinnerton and Wial, we do not reproduce their finding of sharp declines in job stability.

However, two errors in using the CPS data do account for their finding. First, we discovered that Swinnerton and Wial did not use the same definition of self-employment in all years. They used the narrower unincorporated self-employment definition for 1983 and 1987 but the broader classification (i.e., unincorporated and incorporated self-employed) for 1991. As a result, they exclude a class of workers in 1991 that they include in 1983 and 1987. This results in 2.9% too few workers in 1991.

An additional discrepancy arises from treatment of nonresponse to the tenure questions. In 1991, 2.8% of respondents to the tenure supplement did not respond to the tenure questions. In contrast, in 1983 and 1987 there was negligible nonresponse to the tenure questions among those who participated in the supplement. Swinnerton and Wial use the CPS supplement weights, which do not account for nonresponse to the tenure questions within the supplements, and account only for nonresponse to the supplements. Our weighting procedure explicitly accounts for nonresponse to the tenure questions by attempting to make the sample responding to the tenure questions representative of the working population.

When we mimic these two errors, we reproduce the estimated retention rates in Swinnerton and Wial, with a decline from .55 for 1983–87 to .49 for 1987–91. Thus, the inconsistent classification of the self-employed, coupled with the failure to account for nonresponse to the tenure ques-

[24] This section draws heavily from Diebold, Neumark, and Polsky (1996), to which the reader is referred for additional details.

tions, explains the larger decline in job stability that Swinnerton and Wial report.

Rose

In a study done subsequent to ours, Rose (1995) computes a measure of job stability using Panel Study of Income Dynamics (PSID) data and reports sharp declines from the 1970s to the 1980s. Rose's measure of job stability is the number of times over a 10-year period that an individual changed employers. Rose computes this measure using data on a cohort of 24–48-year-olds beginning in 1970 and on another cohort of 24–48-year-olds beginning in 1980. He classifies as "strong stayers" those workers who had at most one employer change, as "medium stayers" those who had two or three changes, and as "weak stayers" those who had more than three changes. His estimates indicate "a pronounced decline in the numbers of strong stayers" (p. 9). In particular, he estimates that the proportion of strong stayers fell from 67% in the 1970s to 52% in the 1980s. The estimated proportion of medium stayers changed little, while the estimated proportion of weak stayers rose from 12% to 24%.

Rose explains the difference between his results and ours by claiming that recall bias makes the CPS tenure data unreliable. The CPS tenure questions require respondents to recall how long they have been with their current employer. Rose claims to form his estimate from data that require no more than 12 months of recall. Such data, Rose reasonably asserts, are less affected by recall bias. Because of this, Rose argues, his estimates of changes in job stability are preferable.

Although reported job tenures certainly are subject to recall bias (see Brown and Light 1992) that leads to heaping and perhaps other phenomena, the quality of our retention rates estimated from the reported job tenures is not necessarily compromised. First, of course, we explicitly adjust for heaping and rounding. Second, and equally important, the holes that Rose attempts to poke in our approach require the recall bias to change over time. We know of no convincing anecdotes along these lines, let alone serious evidence, with one exception (and one that supports our approach): our table 2A indicates that although heaping is substantial, its effects have been stable over time because the retention rate changes are similar for the unadjusted and adjusted data.

Moreover, we have reservations about the way Rose uses the PSID data to construct a measure of job stability over the 1970s and 1980s. Rose claims to construct his measure from the PSID question: "Did you have another main employer during the previous 12 months?" (p. 6). The PSID never asks precisely this question and only in a few years asks a question close to this one. In fact, Rose's measure is constructed by combining information from two questions, one regarding employment status and the other indicating job or employer changes for the continuously employed.[25]

The first question asks respondents their current employment status. Rose uses this question to detect employer changes by determining whether a worker was employed in one year and not employed in the next. The second question, which we refer to as the "previous job" question, detects employer changes among those who are continuously employed according to the employment status question. In particular, individuals are asked what happened to the job they previously held. Possible responses include quits, layoffs, firings, promotions, etc. Individuals may also respond that they did not experience a job change in the recent past (the precise specification of which is discussed below). Rose attempts to count as employer changers only those individuals who provide a response indicating a job change, excluding those who report a promotion (because it may be interpreted as a job change but is not an employer change).

However, the previous job question changes over time in two significant ways.[26] The most important change concerns the period over which a job change is defined. From 1970 through 1983, respondents are coded as having no job change if their current job began 12 or more months ago. But from 1984 through 1987, respondents are coded as having no job change if their current job began before the previous calendar year (e.g., in 1984 they are coded as having no job change if their present position began before 1983). Because interviews are conducted over the course of the year, the 1984–87 responses will pick up more employer changers because fewer individuals will have been on their job for, say, 1.5 years (if the interview is in June) than for 1 year. Finally, for the 1988–90 period, the structure of the question changes again. Respondents are asked if they had another main-job employer only during the previous calendar year (not since the beginning of the previous calendar year). If

[25] The following discussion is based, in part, on copies of programs that Rose generously supplied.

[26] The employment status question also changes over the years that Rose (1995) studies. From 1970 to 1975, the code 1 refers to those working now or temporarily laid off, while the other codes refer to those not working. But from 1976 on, the code 1 refers to those working now, while the code 2 refers to those temporarily laid off (and from 1985 on, also includes those on sick leave). However, Rose's programs indicate that he used the codes 1 and 2 to identify employed workers in *all* years. We also found other coding errors in his programs. For example, in calculating the cumulative number of employer changes, Rose adds each year that an individual was nonemployed; that is, if an individual was employed in one year and nonemployed in the next two, this is considered as two employer changes. However, Rose's results regarding changes in job stability are robust to fixing these coding errors for employment status.

they respond positively, they are then coded (in the previous job question) as having no job change. Because this question will miss employer changes that occurred between the survey date and the beginning of the survey year, fewer employer changes should be picked up than in the 1984–87 period.

In addition, promotions are not identified on a consistent basis over the sample period. For 1975–90, job changes due to promotions can be distinguished from other job changes; for 1975–87, the previous job question has a specific response for promotions; and for 1988–90, the question actually refers to employer rather than job changes. In contrast, for 1970–74, promotions are grouped with quits, resignations, retirements, and other reasons and, therefore, cannot be separately identified.[27]

The changes in the previous job question affect the classification of employer changers. To document this, the second column of table 6A reports the proportion of workers in the PSID whose responses to this question indicate a job change. In this column we do not exclude promotions so that we can use the data beginning in 1971. The proportion reporting job changes is much higher in the 1984–87 surveys than in the earlier surveys. From this information alone we cannot tell whether true job changes increased beginning in 1984 or whether the 1984–87 surveys are overstating the number of job changes as we would expect, given the longer period over which such changes are defined. As additional evidence, the third column reports the proportion reporting employer changes; defining employer (rather than job) changes requires that we exclude promotions, which we can do for 1988–90 but not for 1971–74. This column also shows that the number of changes in the 1984–87 surveys is high relative to the earlier surveys. In addition, it is high relative to the 1988–90 surveys, as the structure of the survey question suggests would occur. It thus seems most likely that the higher incidence of job change in the 1984–87 surveys is a spurious result of changes in the surveys.

Can much of the shift in job stability that Rose detects between the 1970s and the 1980s be explained by variation in the question that identifies job changes? We address this question by comparing changes in his job stability measures between spans of years for which the PSID does and does not provide a consistent classification of job changes. The years for which we can simultaneously avoid all changes in the time period over which job changes are defined and distinguish promotions from other job changes are 1975–83. Thus, using Rose's

[27] Despite these changes in the question, Rose (1995) appears to use the code 6 to identify promotions in all years. In the 1970–74 period this actually identifies those who report that they were previously self-employed.

Table 6
Estimating Changes in Job Stability with the PSID
A. Sensitivity of Job Change Response to Questionnaire Changes

Years	Proportion Reporting Job Change	Proportion Reporting Employer Change
1971–74	.14	...
1975–83	.15	.13
1984–87	.20	.18
1988–90	...	.13

B. Estimates of Job Stability Using PSID Data, Comparing the 1970s to the 1980s

1970s			1980s		
	0–1 Change	Ages		0–1 Change	Ages
1975–77	.95	29–51	1981–83	.92	29–51
1975–80	.83	29–54	1981–86	.75*	29–54
			1984–86	.88*	32–54
1978–80	.95	32–54			
			1988–90	.92	32–54

NOTE.—The estimates in panel A are averages over the corresponding years from tabulations provided in the PSID codebook. In the second column of panel A, promotions are not excluded; this cannot be done for 1988–90 because the question refers to employer rather than job changes. In the third column of panel A, promotions are excluded; this cannot be done for 1971–74 because promotions are not separately identified. The estimates in panel B are sample weighted and adjusted to hold the age composition of the two cohorts fixed. The ages of the subsets of Rose's cohorts that are used in each calculation are indicated.
* Entries are likely to be downward biased because the previous job question for 1984–87 inflates the number of job changes.

cohorts, the most direct comparison we can make is for the 1975–77 and the 1981–83 periods. In the first row of table 6B, we compute the proportions for comparably aged respondents with zero or one employer changes (Rose's strong stayers) in these 2 periods.[28] The comparison indicates a decline, from .95 to .92, in the proportion of strong stayers.[29]

On the other hand, once we extend the analysis to 1984 and beyond, we begin to use the data in which the PSID (at least as used by Rose)

[28] For example, we use the cohort of 28–48-year-olds beginning in 1974 (since Rose's (1995) cohort is 4 years older than in 1970) and a similar-aged cohort beginning in 1980; the ages of members of these cohorts are in the range 29–51 in the 1975–77 and 1981–83 periods. In these calculations, we also correct the coding errors in employment status and use fixed age weights to remove the influence of differences in the age composition of the cohorts.

[29] Because we define these over a shorter period than Rose (1995) did, the proportions of strong stayers are higher than he reports.

overstates job changes. The effect of doing this is to generate a sharper estimated decline in the proportion of strong stayers. In the second row of table 6B, we report the estimated proportion of strong stayers over the 6-year periods of 1975–80 and 1981–86, again for comparable ages. These figures suggest a more dramatic decline, from .83 to .75, in the proportion of strong stayers, a decline nearly three times as large as that in the first row. Similarly, the third row of table 6B (using the top entry for the 1980s) reports the estimated proportion of strong stayers for the last 3 years of these 6-year periods (1978–80 and 1984–86). Here, too, the estimates indicate a sharp decline, from .95 to .88, in the proportion of strong stayers, a decline more than twice as large as that for the earlier 3-year periods reported in the first row of the table.[30]

Finally, we address the question of whether job stability truly began to fall in 1984 or whether the preceding evidence from table 6B stems from changes in the survey. To examine this, we estimate the proportion of strong stayers over the 1988–90 period. This estimate relies on a previous job question that differs slightly from the 1975–83 question but, according to the evidence in table 6A, appears not to overstate employer changes. In the last row of table 6B (this time using the bottom entry for the 1980s), the figures reveal that the proportion of strong stayers reverts to the proportion estimated for the 1981–83 period and, like the estimate for that period, is .03 lower than the estimate for the corresponding 3-year period in the 1970s.

Thus, the dramatic decline in job stability that Rose estimates in the 1980s is most likely an artifact of changes in the PSID survey. When we restrict attention to the years in which the data are consistent we find that, in contrast to Rose's conclusions, the PSID data—like the CPS data—suggest only small declines in job stability.[31]

V. Summary and Concluding Remarks

In our view, the most important conclusion to emerge from our study is the approximate stability of aggregate job retention rates over the 1980s and early 1990s, which contrasts with pronounced shifts in the wage

[30] Because of the way the years line up, we do not use the data for 1987, which also inflate employer changes and would therefore presumably result in a larger drop in the estimated proportion of strong stayers.

[31] This conclusion hinges on interpreting the .03 decline in the proportion of strong stayers as small. There is no way to directly translate this proportion into a retention rate. However, we can use the PSID to estimate retention rates of up to 3 years for jobs held in 1974 and 1980, while restricting ourselves to the consistent data. The estimated 3-year retention rate (holding age composition fixed, as in table 6B) fell from .73 in 1974 to .72 in 1980, very much in line with the magnitudes of retention rate declines that we estimate from the CPS.

distribution. Taken as a whole, the evidence from 4-year job retention rates estimated over this period does not point toward a secular decline in job stability. Analysis of 10-year retention rates confirms the impression of little change in job stability; these estimates show little evidence of change from the 1970s to the 1980s, whether from an aggregate or disaggregate perspective.

Disaggregation of 4-year retention rates by current tenure, age, race, sex, education, industry, and occupation does reveal some changes in 4-year retention rates by demographic group that correspond loosely to changes in the wage structure. In particular, retention rates have declined for high school dropouts and high school graduates relative to college graduates (and more so for the youngest workers) and for blacks relative to whites. Thus, there is some evidence of relative declines in job stability for those groups that experienced the sharpest relative wage declines over the sample period, although aggregate job stability has been largely unchanged.

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