Youth Labor Markets in the U.S.: Shopping Around vs. Staying Put

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Abstract

The need for school-to-work programs or other means of increasing early job market stability is predicated on the view that the "chaotic" nature of youth labor markets in the U.S. is costly because workers drift from one job to another without developing skills, behavior, or other characteristics that in turn lead to higher adult earnings. However, there is also ample evidence that workers receive positive returns to job shopping. This paper asks whether youths in unstable or dead-end jobs early in their careers suffer adverse labor market consequences as adults. In particular, it accounts for the endogenous determination of early job stability as a response to job match quality–which may also influence adult wages–using labor market conditions in the early years in the labor market as instrumental variables for the job stability experienced during those years. The instrumental variables estimates generally point to substantial positive effects of early job stability on adult wages.

Introduction

Early labor market experiences of youths in the U.S. are often characterized as "churning" or "milling about" in the form of initial periods of joblessness or a series of "dead-end" jobs (U.S. General Accounting Office, 1990), or as "floundering" from one job to another, representing a "waste of human resources" (Stern, et al., 1990). This characterization of U.S. labor markets has motivated policy initiatives to address the school-to-work transition by helping to transform the youth labor market from the current "chaotic" system in the U.S. to a more "orderly" system, like that of the German apprenticeship system or the informal contracts between Japanese schools and employers, in which youths leave school for further career training or stable employment.[1] The need for school-to-work programs or other means of increasing early job market stability is predicated on the view that the chaotic nature of youth labor markets in the U.S. is costly, presumably because workers drift from one job to another without developing skills, behavior, or other characteristics that in turn lead to higher adult earnings. Such problems may be particularly profound for less-advantaged workers, as recognized, for example, in the 1994 School-to-Work Opportunities Act.[2] Thus, policies that hasten the placement of workers into steady jobs soon after leaving school may help to offset the wage declines experienced by many workers in the U.S. in recent decades, especially young, less-skilled, or disadvantaged workers.

However, there is a potentially strong counter-argument to this negative view of the turbulent nature of youth labor markets in the U.S. Specifically, there is compelling evidence that workers receive positive returns to job shopping (e.g., Topel and Ward, 1992), presumably as workers (and employers) learn about their skills, aptitudes, and interests by trying different jobs, leading to increasingly better matches as young workers move through a series of jobs. Thus, it remains an open question whether school-to-work programs or other means of increasing early job market stability would result in higher earnings as adults—stemming from good matches as well as acquired skills, behavior, or other characteristics—compared with the current functioning of youth labor markets in the U.S.

Answering this question is extremely difficult, probably requiring carefully designed experiments. The compendium of research on school-to-work programs discussed in the NCRVE report by Stern, et al. (1994) suggests that researchers are a long way from a definitive answer to this question. A more limited goal is to ask whether youths who appear to be in unstable or dead-end jobs early in their careers suffer adverse labor market consequences as adults. It seems that, *minimally*, a case for attempting to replace current—perhaps chaotic—methods of job shopping with programs that induce earlier job stability requires evidence that those youths who experience unstable jobs or "floundering about" in their early years in the labor market suffer longer-term consequences.

Assessing such evidence poses econometric problems, however. As a leading example, job matching models suggest that the statistical association between adult wages and early job stability may not provide a good estimate of the increase in adult wages that might ensue from increased early job stability, because good matches are relatively more likely to have resulted in early job stability (Jovanovic, 1979; Mortensen, 1978). That is, these models predict non-random selection of individuals (based on their job matches) into early job market stability, in which case this statistical association will overstate the returns to increasing early job market stability for those who would otherwise have low stability, unless the induced increases in early job stability are accompanied by better matches.

The goal of this paper is to examine evidence on the causal effects of early job stability on adult labor market outcomes, by eliminating the bias in this estimated relationship that stems from omitted job match quality or other factors. Specifically, rather than simply estimating least squares regressions of adult wages on youth labor market experiences related to early job stability, along with adult characteristics, youth labor market conditions from the years in which workers entered the labor market are used as instrumental variables for the job stability experienced by workers as youths. The idea is that variation in youth labor market conditions is exogenous to the individual, and therefore generates variation in early job stability that is unrelated to job match quality. This empirical approach, in principle, yields estimates that come closer to measuring the potential effects of policies that would increase early job stability.

Past research that does not fully account for the endogeneity of early job market stability finds little or no evidence that early job stability improves adult labor market outcomes (Gardecki and Neumark, 1998). This suggests that the approach taken in this paper will reach an even more pessimistic conclusion; if unobserved job match quality is a source of positive correlation between adult wages and early job stability, the expectation is that the instrumental variables procedure will lead to even lower estimates of the positive effects of early job stability (or stronger estimates of negative effects). As will become clear, however, the evidence generally points in the opposite direction, with the instrumental variables estimation pointing to substantial benefits from early job stability. After presenting the evidence, the paper considers alternative interpretations of these unanticipated findings. The interpretation that appears to best fit the available facts is that while there are returns to search, there are also positive returns to early job stability. However, heterogeneity in the returns to search is a source of spurious *negative* correlation between early job stability and adult wages—as those with higher returns to search have lower early job stability but higher adult wages—which is eliminated by the instrumental variables estimation. Thus, the evidence in this paper suggests that exogenous increases in early job stability in youth labor markets in the U.S.—such as might be caused by school-to-work or other programs—would have beneficial effects on the incomes youths eventually earn as adults.

Previous Work

Earlier research considered evidence on the relationship between early job market stability and adult labor market outcomes by exploring the correlations between a wide range of individuals' youth labor market experiences and their labor market outcomes as more mature adults, in a multivariate framework that controlled for other adult characteristics (Gardecki and Neumark, 1998, hereafter GN). GN reported estimates of wage regressions for individuals in their late-20s to mid-30s, controlling for the usual ingredients of wage regressions—schooling, experience, etc.—at the time the wage was measured, but adding in measures of youth labor market experiences over the first five years in the labor market, including number of jobs, longest job held, labor market experience, industry and occupation changes, etc. The results suggested that adult labor market outcomes are for the most part unrelated to the stability of early labor market experiences, especially for men, although as many studies have found, training bestowed longer-term benefits.[3] This evidence was interpreted as undermining the case for policy initiatives to create more early job market stability in U.S. labor markets.

However, that evidence may be misleading if, for example, good job matches are likely to have resulted in early job stability. For the most part, GN documented that the estimated partial correlations between early job market stability and adult earnings were essentially zero, and argued that the positive bias in the estimates suggests that exogenous increases in early job market stability have if anything *adverse* effects on adult labor market outcomes.[4] This is consistent with the view that job shopping is a crucial source of earnings growth for young workers, as the process of searching for better matches generates low early job market stability but ultimately higher adult wages. Although policy innovations that lead to better job matches would, undeniably, be helpful, this evidence suggests that policies targeting early job stability *per se* may be harmful. The present paper moves beyond this earlier work by studying explicitly the bias in the estimated relationship between early job market stability and adult labor market outcomes that stems from omitted job match quality or other sources, leading—it is hoped—to estimates of the causal effects of changes in early job market stability, which are a necessary input for policy evaluation.

The Data

The Sample

The data set is very similar to that used in GN, since the goal of this paper is to expand the empirical analysis in that paper. This section provides an overview. The NLSY is used for the years 1979-1992, providing comprehensive labor market, schooling, and test score information on a large cohort near or at the beginning of their school-to-work transition, and later in their careers as more mature adults. The sample is first restricted to individuals who were neither in the military subsample nor reported any military duty through 1992. Next, a number of restrictions on the sample are imposed to focus on individuals' first years in the labor market. A window of five years is used, based on the presumption that this window is sufficiently long to observe many individuals' transitions from their earliest entrance into the labor market into somewhat steadier employment (Osterman, 1980). The tradeoff is that the longer the window used, the smaller the sample gets, as it becomes more likely that a non-interview is encountered, or that some of the questions change and become unusable. Because of data constraints, and because it was important to have a substantial amount of elapsed time between this early labor market period and adult labor market outcomes, attention was restricted to five-year windows ending in 1986. This implies that the windows ranged from 1979-1983 to 1982-1986.

Dating labor market entry is ambiguous, because some individuals acquire work experience during school and others go back to school after working. A natural procedure is to regard entry into the labor market as the first year in which individuals are observed "permanently" out of school—i.e., out of school for the remaining observations in the NLSY. However, because school-to-work transitions may involve course work at community or other two-year colleges, it seems unduly restrictive to limit the analysis to the period after which individuals report no additional schooling. Thus, the date of entrance into the labor market was defined as the first year in which individuals no longer report schooling other than at two-year colleges.

To obtain data on these early labor market periods, individuals with non-interviews in the relevant periods, individuals missing the enrollment data required to date their labor market entry, and individuals who did not have a first labor market entry (as defined above) in the 1979-1982 period had to be dropped; obviously, these restrictions generate large reductions in the available sample, because they tend to exclude both the oldest and youngest members of the NLSY cohort. GN present evidence suggesting that these sample selection rules hinge largely on the age of respondents—although obviously schooling decisions also play a role—and therefore may not generate substantial biases. Some observations are also lost because of missing or inconsistent data on early labor market experiences, or because respondents were not observed in a job during the survey week at any time during the five-year post-schooling period (which eliminates only about six percent of the sample). Other than this latter restriction, imposed so that some job information is available, there is no lower limit on the amount of time they had to have worked during that period. Finally, because the outcome of interest is adult wages, attention is restricted to those working for a wage on the CPS job in the NLSY in 1992, which refers to the main job held in the previous week.[5]

The Variables Used

Attention is focused on two specific characteristics of early labor market experiences that are most closely related to early job stability: the longest job held in the initial five-year post-schooling period; and the number of jobs held in this same period. The longest job held is defined as the highest tenure (in years) attained during the five-year post-schooling period. This is simply constructed from the reported job tenure measure.[6] The variable measuring number of jobs held is a cumulative measure of the number of jobs ever reported as of the end of the five-year post-schooling period.[7] This job count can include information on jobs held prior to the five-year post-schooling period, which is potentially problematic if individuals hold a number of short-term jobs while in school. However, for those who enter in 1980 or afterwards the number of jobs held as of 1979 can be subtracted out, thus focusing only on jobs in the five-year post-schooling period. Results are therefore reported for the subsample entering in 1980 or afterwards, correcting the number of jobs variable for jobs held before labor market entry.[8]

Information on local unemployment rates is used to measure youth labor market conditions that might have exerted an exogenous influence on youth labor market experiences, as explained more fully in the next section. These unemployment rates come from the NLSY Geocode data file, and are based on state and area labor force data from the May *Employment and Earnings* covering March of each year; for most states and areas, these rates are based on more than just the household data from the CPS. The Geocode file includes the rates for metropolitan areas for which unemployment rates are reported in *Employment and Earnings*, for individuals residing in those areas, and rates for the rest of the state for other individuals.[9]

Descriptive statistics for the samples of men and women are reported in Table 1. Because interest centers primarily on the regression estimates, these statistics are unweighted. The non-white proportions are high because of the NLSY oversample of blacks and Hispanics. The figures reveal many typical features. Men's wages are higher by 26 percent on average, and the average experience and tenure of men are higher, while women have higher average schooling. The average year of labor market entry, as defined here, is in the middle of 1980. The average annual unemployment rates experienced by individuals in the sample range from 7.85 to 9.86 percent over the five-year post- schooling period.[10] Finally, both the longest tenure attained in the five-year post-schooling period and the number of jobs held are slightly higher for men than for women, suggesting that men go through more jobs initially, but also settle into a somewhat longer job in this post-schooling period.[11]

Methods

The equation that is estimated is of the form

$$\textbf{(1)} \qquad \ln(w_{it}) = \alpha S_{it'} + X_{it}\beta + \varepsilon_{it}$$

where w is the adult wage, X is a vector of standard contemporaneous labor market characteristics included as controls, and S is a variable measuring early job stability. To control for simple individual heterogeneity that may be correlated with both wages and early job stability, the AFQT test score (standardized for age) and parents' education are also included in X. Although the estimated regression is cross-sectional, time subscripts are included to clarify the time at which the variables are measured. Those with a t subscript are measured as an adult, and those with a t′ subscript are measured during the initial five-year post-schooling period.

If early job stability enhances adult labor market outcomes conditional on the adult characteristics in X, then α in equation (1) should be positive when S is the longest tenure attained, and negative when S is the number of jobs held. In GN, OLS estimates generally indicated that the coefficients on longest tenure attained and number of jobs were small and insignificant, and often did not have the expected sign.

The concern with omitted job match quality suggests that the error term ε consists of two components,

$$\textbf{(2)} \qquad \varepsilon_{it} = \mu_{it} + \eta_{it}$$

The component η_{it} is the usual idiosyncratic error, assumed to be uncorrelated with S and X, and independently and identically distributed across observations. The component μ_{it} is an unobserved measure of the quality of the adult job match, and hence is assumed to be positively correlated with ln(w). In turn, μ_{it} is presumed to be positively related to the quality of the job match attained during the five-year post-schooling period, $\mu_{it'}$, for two reasons.[12] First, some individuals remain on the same job and hence retain their earlier match.[13] Second, all else the same individuals with good matches in the early years may be more likely to have good matches as adults even if they changed jobs, because voluntary job changes, at least, are likely to be in the direction of even better matches. Thus, because μ_{it} is likely to be positively correlated with early job stability, μ_{it} is also likely to be positively correlated with early job stability, suggesting that OLS estimation of equation (1) will result in upward biased estimates of the effects of early job stability.

The strategy for estimating equation (1) in the presence of an unobserved variable related to match quality is to use an instrumental variable for S, the measure of early job stability. What is required is a variable or set of variables that explains variation in S, but does not directly affect the current wage—specifically, it must be uncorrelated with μ_{it}. Various measures of the unemployment rate faced by the individual in the immediate post-schooling period are used.[14,15] Variation in unemployment rates faced by young people should be related to early job stability. For example, if a recession occurs soon after a person enters the labor market, the likelihood of holding a job for a long period would be reduced. Similarly, the number of jobs the person held in this period might increase, if the principal effect of the recession is to cause jobs to end. In this case, a weak youth labor market reduces early job stability. On the other hand, the relationship between early unemployment and early job stability could go in the opposite direction, as, for example, slack labor markets reduce wage offers from firms to employed workers and hence deter mobility. The direction of this relationship is an empirical question; what is required, however, is that youth labor market conditions help to predict early job stability.

The more difficult requirement for the instrument is that it is uncorrelated with the error term of equation (1), or that it not appear in the wage equation. Because unemployment rates in geographical areas may be persistent over time, and because many individuals may remain in the same geographical area, past unemployment rates may be correlated with contemporaneous unemployment rates, which may in turn affect current wages (see, e.g., Blanchflower and Oswald, 1990). To handle this potential problem, the contemporaneous unemployment rate is added as a control variable in equation (1).

However, a direct role for early unemployment rates cannot be decisively ruled out, even controlling for contemporaneous unemployment rates. There is, in fact, some existing work on the relationship between current wages, current unemployment rates, and past unemployment rates. In particular, Beaudry and DiNardo (1991) show that in a model with implicit labor market contracts and costly mobility, the unemployment rate at time of hire is related to the current wage, even taking account of the current unemployment rate. On the other hand, in a model with relatively costless mobility, the lowest unemployment rate since beginning the job influences the current wage, again independently of the current unemployment rate. Moreover, in this case earlier unemployment rates (other than the minimum) do not affect the contemporaneous wage. The evidence Beaudry and DiNardo present, using PSID and CPS data, suggests that the latter characterization better fits the data, as it is the lowest unemployment rate since beginning the job that has a significant, robust negative effect on the wage, and once this unemployment rate is included, neither the unemployment rate at the start of the job nor the current rate is significantly associated with the wage.

As a consequence of this earlier work, the minimum unemployment rate faced by the individual since beginning his or her current (adult) job is also constructed. This minimum rate is included in the wage equation, and then the earlier unemployment rates (those from the first five years in the labor market) are used as instruments.[16] Replicating the Beaudry and DiNardo result, the evidence generally shows that the minimum unemployment rate on the current job is strongly negatively related to contemporaneous wages, and that once this variable is included, neither the unemployment rate at the beginning of the job nor the current unemployment rate is significantly negatively related to contemporaneous wages.[17] More specifically related to the analysis of this paper, the evidence typically indicates that the early unemployment rates are valid instruments (in the sense of passing overidentification tests) only when the minimum unemployment rate on the current job is included, as the Beaudry and DiNardo results would lead us to expect; these results are reported in the tables below.

Of course, the unemployment rate that an individual faces in a geographic area is not necessarily exogenous, since there is always the possibility of mobility. Because of this, the sensitivity of the results to using the unemployment rate in different forms is explored. First, the actual unemployment rate for each individual for each year, U_{ijt}, is used, where i, j, and t index individuals, entry year, and year in the labor market (i.e., 1-5). Next, the cohort/year variation, which is more plausibly exogenous, is isolated by constructing

$$\textbf{(3)} \qquad U_{\cdot jt} = \frac{1}{N_{jt}} \sum_{i \epsilon jt} U_{ijt}$$

where N_{jt} is the number of individuals in entry cohort j in year t, and iϵjt indicates that the summation is taken over all members of a particular entry cohort j in year t in the labor market. $U_{\cdot jt}$ measures the average unemployment rate faced by a member of cohort j in their t-th year in the labor market. Although this average rate is still potentially endogenous because labor market conditions could affect the timing of labor market entry, this measure removes the cross-sectional variation within a year that is most prone to endogeneity bias from migration and residence decisions.

The sensitivity of the results to alternative sources of variation in unemployment is considered by constructing (U_{ijt} - $U_{\cdot jt}$), the deviation of the individual-level unemployment rate from the cohort- year average, and using both "components" of the unemployment rate U_{ijt} as instruments. In the latter specification, using Newey's (1985) overidentification test is particularly informative, because there are, conceptually, two different instrumental variables (even though they each represent sets of instrumental variables)—the cohort-average unemployment rates, for which the identifying assumptions are more plausible a priori, and the deviations, for which the overidentification test may be most relevant.

Finally, this analysis is repeated using $U_{ij\cdot}$, $U_{\cdot j\cdot}$, and $(U_{ij\cdot} - U_{\cdot j\cdot})$, defining

$$\textbf{(4)} \qquad U_{ij\cdot} = \frac{1}{T}\sum_{t=1}^{T} U_{ijt}$$

where T is the number of years in the post-schooling period, and

$$\textbf{(5)} \qquad U_{\cdot j\cdot} = \frac{1}{N_j}\sum_{i\varepsilon j} U_{ij\cdot}$$

That is, $U_{ij\cdot}$ is the average unemployment rate faced by individual i in cohort j over the five-year post- schooling period, and $U_{\cdot j\cdot}$ is the average unemployment rate faced by individuals in cohort j over the five-year post-schooling period. As in the specification in which the unemployment rates from each of the five years are used, $U_{\cdot j\cdot}$ is the most plausibly exogenous variable, because it removes individual cross-sectional variation that may be related to migration and residence decisions. One advantage of using these averages is that they make the estimated effects of early unemployment rates in the first-stage regressions easier to interpret, because there is one coefficient rather than a set of coefficients for the unemployment rates from each of the first five years. On the other hand, the pattern of unemployment rates over the initial post-schooling period may be important, and the five-year averages discard this information.

Results

The main results are reported in Tables 2-5, where the estimates are reported first for longest tenure attained in the five-year post-schooling period, for men and women, and then for the number of jobs held during this period, also for men and women. In these tables, contemporaneous tenure is excluded as a control variable in the log wage equations, because it is likely to be strongly linked—and to some extent caused by—early tenure or job attachment; thus, these might be interpreted as "reduced form" estimates of the effects of early job stability. However, the effects of early job stability net of adult tenure are also of interest, in part to test whether early job stability has benefits other than those reflected in the length of attachments between workers and firms. Thus, Table 6 explores the sensitivity of the results to adding current tenure (and its square) to the wage equation, for the key specifications chosen from the other tables. Some caution must be exercised in interpreting these latter estimates, however, given that current tenure, like early tenure, may be endogenous (e.g., Abraham and Farber, 1987; Altonji and Shakotko, 1987); if the "returns" to adult tenure are overstated and adult tenure and early tenure are positively correlated, then including adult tenure will bias downward any estimated beneficial effects of early job stability.

Column (1) of Table 2 reports OLS estimates of a standard log wage equation supplemented by the longest tenure attained in the initial five-year post-schooling period. Paralleling the results in GN, the estimated coefficient of the longest tenure attained is small (.01) and statistically insignificant. The other estimated coefficients are standard. The low estimated return to schooling (.04) is attributable to the inclusion of the AFQT score; with the test score excluded, the estimated return to schooling is .06.[18]

Columns (2) and (3) report results from the first attempt to remove bias from omitted job match quality, by instrumenting for longest tenure attained with early unemployment rates. The first-stage estimates are reported in column (2). The estimated coefficients of unemployment rates for each of the five post-schooling years vary in sign, with the results suggesting that unemployment rates in the early part of this period increase longest tenure attained, while unemployment rates in the fourth and fifth years reduce it, although the latter effect is smaller. As will become clear, the general finding throughout the tables is that high unemployment rates during the early years in the labor market increase early job stability, consistent with the predominant effect of slack youth labor markets being to deter search or encourage job stability for other reasons. Column (3) reports the corresponding IV estimates of the wage equation. Contrary to expectations of downward bias in the OLS estimate of the effect of longest tenure, the IV estimate of the effect of this variable rises (to .08), and is nearly statistically significant at the ten-percent level.[19]

The last few rows of the columns report results of specification tests. First, the F-statistic for the instruments in the first-stage regression is 11.4, indicating that small sample biases are not an issue (Bound, et al., 1995), consistent with the large change in the coefficient estimate upon instrumenting. Second, the Hausman test comes close to rejecting exogeneity of longest tenure attained in the wage equation.[20] Third, the instruments pass Newey's overidentification test, although this test may not be particularly meaningful here because one might argue that there is only one instrument—the early unemployment rate—which has been arbitrarily divided into five years. Finally, the instruments fail the overidentification test when the minimum unemployment rate on the current job is excluded from the wage equation, as discussed above in relation to Beaudry and DiNardo (1991).

Columns (4) and (5) report results from using the average unemployment rate experienced by each entry cohort, in each year, rather than the unemployment rate experienced by the individual, in order to obtain a more exogenous instrument. The first-stage results in column (4) are quite similar, suggesting that early unemployment raises longest tenure attained in the immediate post-schooling period, and later unemployment lowers it, again by less. Note also that the estimated effects of unemployment are larger in column (4) than in column (2), consistent with a migration response to local unemployment rates that biases the estimates in column (2) towards weaker effects of variation in labor market conditions. The IV estimate of the effect of longest tenure attained on the wage (.08) is again considerably above the OLS estimate, and statistically significant.[21] The specification test results are also similar, although in this case—perhaps reflecting the greater aggregation (and exogeneity) of the cohort average unemployment rates—the p-values for the overidentification tests are higher, and the restrictions are not rejected whether or not the minimum unemployment rate is excluded from the wage equation.[22] In addition, the exogeneity of longest tenure attained is rejected, with a p-value of .02.

The estimates in columns (6) and (7) use both the cohort average unemployment rates, and the deviations of the individual rates from these, as instruments. Again, the wage equation results are very similar to those in columns (2)-(5), with the estimated effect of longest tenure attained positive (.07) and statistically significant. In this specification, the overidentification test has a stronger interpretation, since it seems reasonable to assume that the cohort average unemployment rates provide valid instruments a priori, and to interpret the test as informative with respect to the individual-level deviations. As long as the minimum unemployment rate on the current job is included in the wage equation, the instruments easily pass the overidentification test. However, the evidence from the first-stage regressions indicates that only the cohort average unemployment rates have significant effects on early job stability.

The final two columns of the table use the averages of the unemployment rate variables over the five years, $U_{\cdot j \cdot}$ and ($U_{ij\cdot}$ -$U_{\cdot j \cdot}$). In this case, in the first-stage estimation the cohort average unemployment rate averaged over the five post-schooling years provides all of the explanatory power. Its estimated coefficient is positive, again indicating that high unemployment during the immediate post-schooling period results in longer job attachment during this period. In the wage equation estimation, again, the IV estimate of the effect of longest tenure attained is positive (.13) and statistically significant. The Hausman test rejects the exogeneity of longest tenure attained in the wage equation at the six-percent level.

Thus, in all of the specifications in Table 2, the evidence indicates that longest tenure attained in the immediate post-schooling period—i.e., during the school-to-work transition—has a positive effect on adult wages. In addition, the estimated magnitude is large, with an additional year of tenure leading to adult wages that are higher by seven to 13 percent. Of course, this should not be thought of as a return to tenure per se, since attention is restricted to the five-year post-schooling period. In particular, as reported in Table 1, the standard deviation of longest tenure attained is 1.65 for men. Thus, the estimates in Table 2 imply that a one standard deviation increase in longest tenure attained results in adult wages that are higher by 11.6 to 21.5 percent, numbers that are high but perhaps not implausible. (Below, it is suggested that these estimates may be upward biased, but still reflect positive effects.)

Finally, recall that these specifications do not include contemporaneous tenure. Column (1) of Table 6 reports the results of estimating the wage equation with contemporaneous tenure controls included. The specification from columns (6) and (7) of Table 2 is used, since this specification uses the most information on early unemployment while still easily satisfying the overidentifying restrictions. As expected, the estimated effect of early tenure falls; in particular, the estimated effect of an additional year of early tenure on the adult wage, conditional on adult tenure, is five percent and no longer statistically significant. Thus, at least some of the beneficial effect of early job stability arises through its effect on adult tenure, which is not surprising.

Table 3 repeats this analysis for the sample of women. Having described the analysis in Table 2 in detail, these results can be discussed more briefly. In the OLS estimates in column (1), the effect of longest tenure attained is small (.02) as it is for men, although in this case the estimate is statistically significant. Looking at columns (2)-(7), the IV estimates indicate substantially larger returns to early tenure, ranging from 12 to 24 percent. The estimates in columns (4) and (5), which use the cross-cohort variation in early unemployment rates, are preferable on a priori grounds to those in columns (2) and (3), while in columns (6) and (7), in which the individual deviations are also used, there is some weak evidence against the overidentifying restrictions (the p-value is .16) even when the minimum unemployment rate on the current job is included as a control variable, suggesting that the specification in columns (4) and (5) is preferred overall. The estimated effect of .16 in column (5) implies that a one standard deviation increase in longest tenure attained increases adult wages of women by 25.4 percent. As for men (and perhaps more so), in the first-stage estimates the overall effect of early unemployment on longest tenure attained is positive. Thus, the estimates in columns (1)-(7) are qualitatively similar to those for men, although the beneficial effects of early tenure are stronger for women.

The only difference relative to men comes in columns (8) and (9), when the unemployment rate variables averaged over the five post-schooling years are used. In this case, the sign of the first-stage estimate of the cohort average unemployment rate is negative and insignificant, whereas it was positive and significant for men. In addition, the F-statistic for the instruments in the first-stage regression is only 2.4, compared with much larger values in the other columns. This suggests that small sample bias may be non-negligible, and that, more generally, the instruments in this specification do not provide much identifying information. This is also reflected in the much larger standard error of the estimated coefficient of longest tenure attained in column (9). Thus, the estimates in columns (8) and (9) are not very informative.

Finally, the estimated effect of longest tenure attained falls, but remains positive (.14) and statistically significant, once controls for contemporaneous tenure are included in the preferred specification, as reported in column (2) of Table 6. Note also that the small positive effect of early tenure in the OLS estimates goes away after conditioning on current tenure. Overall, then, the beneficial effects of early job stability—as measured by longest tenure attained—are stronger for women, appearing even conditional on adult tenure.

Tables 4 and 5 turn to results for the number of jobs held during the five-year post-schooling period. For men, the OLS results in column (1) of Table 4 indicate a small (-.014) negative, significant effect of number of jobs held. Using the individual-level unemployment rates to instrument for number of jobs held, in columns (2) and (3), results in a small change in the estimated effect of the number of jobs held, with the estimate falling to zero. However, the F-statistic for the instruments in the first-stage regression is only 1.9, and the overidentifying restrictions are rejected (at the ten-percent level) even when the minimum unemployment rate on the current job is included. Thus, these IV estimates are not reliable.

In columns (4) and (5) the cohort average unemployment rates are instead used as instruments. In this case, the F-statistic for the instruments in the first-stage regression jumps to 9.4, and the overidentifying restrictions are not rejected. The first-stage estimates indicate that higher unemployment during the school-to-work transition results in fewer jobs held, consistent with the results in Tables 2 and 3, because the number of jobs is inversely related to stability. Finally, the IV estimates of the wage equation indicate that number of jobs held in the immediate post-schooling period has a sizable negative (-.08) and significant effect on adult wages.

Columns (6)-(9) report the estimates first using the cohort average unemployment rates and individual-level deviations from these, defined over each of the five post-schooling years, and then using the five-year averages. In columns (7) and (9) the IV estimates of the effect of number of jobs held are smaller than in column (5), and insignificant. However, in these estimations the p-values for the overidentifying restrictions are quite low (.18 and .08), suggesting that the estimates in column (5) are preferred, presumably because the estimates in columns (6)-(9) use the individual-level unemployment rates that may be partly endogenous. The estimate of -.08 in column (5) implies that a one standard deviation (2.63) increase in the number of jobs held in the immediate post-schooling period lowers adult wages by 21 percent.

Finally, column (3) of Table 6 reports estimates with controls for contemporaneous tenure in the wage equation, for the preferred specification. The small negative estimated effect of number of jobs held in the OLS estimates is no longer present after controlling for adult tenure. The IV results indicate a negative coefficient on number of jobs held in the immediate post-schooling period, although as for longest tenure attained the estimated effect conditional on adult tenure is smaller (falling from -.08 to -.06) and insignificant, implying that some of the effect of early job stability arises through higher adult tenure.

Paralleling Table 4, Table 5 reports results for women for the number of jobs in the immediate post-schooling period. The OLS estimates in column (1) indicate no effect of number of jobs held. In the remaining columns of the table, the IV estimates of the effect of number of jobs held range from positive to negative, although they are never statistically significant. However, at least in columns (2)-(7), the F-statistics in the first-stage regressions are rather low (2.4-2.9), and in all columns the overidentifying restrictions are rejected at the five- or ten-percent level even when the minimum unemployment rate on the current job is included. Thus, for women, the data (using these instruments, at least) do not appear to permit us to identify the exogenous effects of number of jobs held in the post-schooling period.

The final analysis addresses the issue of measurement of local unemployment rates. As explained in Section III, the unemployment rates for individuals residing outside of metropolitan areas (as well as those residing in metropolitan areas for which unemployment rates are not reported in *Employment and Earnings*) are rates for the entire non-metropolitan area of their state of residence. Consequently, unemployment rates may measure local labor market conditions much more accurately for those residing in SMSAs for which separate unemployment rates are reported in *Employment and Earnings*.[23] The estimates of the key specifications, both with and without adult tenure included, were therefore recomputed using only observations on individuals residing in this subset of SMSAs in each of the early labor market years as well as 1992 (since the unemployment rate for each of these years is required).[24]

The results are reported in Table 7. For the most part, the magnitudes of the estimated coefficients of local unemployment rates in the first-stage regressions and in the IV estimates of the wage equations are larger in absolute value, consistent with a reduction in measurement error in unemployment rates. More importantly, for almost all of the estimates the evidence of beneficial effects of early job stability is stronger than in Table 6 and the corresponding estimates in Tables 2-5. The most notable difference, perhaps, is that even in the specifications including adult tenure, the estimated effects of early job stability for men are positive, and significant or nearly so at the ten- percent level (or better). Thus, for this subsample for which the instruments are measured more accurately, there is relatively strong evidence of positive returns to early job stability for men and women, even conditional on adult tenure.[25]

Discussion and Conclusions

Relatively consistent evidence emerges from most of the estimations. For both men and women, most of the IV estimates for specifications that are not rejected by the data indicate a positive return to early job stability, in contrast to the OLS estimates that indicate essentially no return. Interestingly, these IV results are contrary to the hypotheses with which this study began. The presumption was that OLS estimates of the effects of early job stability were biased towards finding a positive effect of early job stability because of omitted job match quality that is positively associated with this stability; the IV results, as just noted, instead indicate that the OLS estimates of the effects of early job stability are biased downward. Thus, the evidence requires an alternative explanation. This section discusses a few possible alternative explanations of the results, and considers the consistency of the evidence with each of them.

One possibility is that the early unemployment rates are invalid instruments, owing to a correlation between the unobservable in the wage equation and the instruments.[26] The positive relationship between early unemployment rates and early job stability apparent in the first-stage estimates could arise because hires that occur in slack labor markets tend to be better matches, as employers can be more selective in their hiring decisions. In this case, the instrument is positively correlated with the match-specific component of the error, , biasing the IV estimate of the effect of early job stability upward. However, the IV estimate of the effect of early job stability should be biased upward *more than* the OLS estimate only if the positive correlation between the regressor (S) and the match quality component of the error term in equation (1) is exacerbated. This would require, for example, that individuals with high predicted S based on high early unemployment rates are in good matches to a greater extent than are those with high observed S (which may be influenced by a number of factors not limited to early labor market conditions). But if the observed variation in S also reflects self-selection of those with good matches into stable early jobs, it is difficult to see why the bias would not, on net, be lessened—although perhaps still present—in the IV estimates.

Therefore, other explanations of the IV results—maintaining for now the assumption that the instruments are valid—are considered. What is required, in particular, is a source of negative correlation between early job stability and adult wages. Such a negative correlation could arise if the unobserved component μ of the wage equation is primarily an omitted fixed individual effect—independent of the job match—that is positively correlated with adult wages but negatively correlated with early job stability. One reason the latter correlation might arise is if the returns to job search rise faster with this unobservable than do the costs of job search. The indirect opportunity costs of search are presumably higher for higher-wage individuals, although these opportunity costs may be quite low if search occurs while employed. However, the returns to job shopping may be considerably higher for higher-wage individuals, if the returns are characterized as roughly proportional to the current wage. Such higher returns would generate a negative correlation between the error term and S in equation (1), rather than a positive correlation, as the higher-wage individuals engage in relatively more job shopping.[27] In this case the instrumental variables estimate of the effect of early job stability—in which variation in this stability is exogenously driven by labor market conditions, and not endogenously driven by unobserved components of the individual's productivity—would be more positive than the OLS estimate, because the latter is biased *downward*.

One way to assess explanations regarding unobservables, such as this one, is to look at evidence on observables that should behave in the same way as the unobservable. In this particular case, it seems reasonable to suppose that education and the unobserved wage or productivity component should have similar relationships with early job stability. However, Tables 2-5 show that education is positively correlated with early job stability (as is AFQT, although these results are not reported in the tables). It is possible that the relationship is different for the unobservable fixed effect, if the unobservable matters mainly *within* education and ability levels. But it is probably best to be skeptical about an hypothesized unobservable related to wages or productivity that is inversely related to education and measured ability.

Alternatively, however, suppose that there is heterogeneity in the returns to search. With such heterogeneity, those with relatively higher returns to search exhibit *less* early job stability, do more searching and more successful searching, and find better matches as adults, generating a negative correlation between early job stability and adult wages. But when early labor market conditions provide the identifying variation in early job stability, the negative correlation between early job stability and the quality of the adult job match is broken, and the IV estimate of the effect of early job stability on adult wages should rise relative to the OLS estimate. For example, those with predicted high early job stability are less likely to be those with low returns to search—who end up with worse matches and lower adult wages—than those who endogenously choose high early job stability.[28,29] In addition to explaining the differences between the IV and OLS estimates, the explanation based on heterogeneous returns to search is easier to reconcile with the positive estimated relationship between education and early job stability. In particular, those with more education may have lower returns to search because they enter the labor market with much better information about their skills and abilities, and with human capital investments that are more occupation specific.

The fact that the IV estimates indicate net positive returns to early job stability in the earliest years in the labor market is not inconsistent with an explanation based on variation in returns to search. Although the explanation does require some positive returns to job shopping, it does not require that there be no positive returns to job stability in the earliest years in the labor market. Rather, there can be competing beneficial effects of job stability and job shopping.[30] However, according to this explanation, selection of those with low returns to search—and hence worse adult matches—into careers marked by early job stability, leads OLS estimates to understate the relative return to job stability, while IV estimates suggest that, on balance, the returns to job stability in the earliest years in the labor market are greater.

Finally, the earlier discussion argued against the possibility that a positive correlation between unemployment rates and match quality leads the IV estimate of the effect of early job stability to be more upward biased than the OLS estimate. The argument was that the positive relationship between μ_{it} and $S_{it'}$—reflecting those with good matches experiencing early job stability—would outweigh this source of bias. However, if, as suggested here, μ_{it} and $S_{it'}$ are negatively correlated, then upward bias in the IV estimates relative to the OLS estimates is a more plausible possibility, and suggests that the IV estimates of the positive effects of early job stability could be overstated. The truth would then lie somewhere in between the OLS estimates (because the OLS estimates would be prone only to the downward bias from this negative correlation), and the IV estimates of quite large positive effects. On net, though, because the OLS estimates of the effects of early job stability are essentially zero, the results would still imply beneficial effects of early job stability.

Overall, then, once account is taken of the endogenous determination of early job stability, the evidence indicates that there are positive effects of this stability on adult wages, suggesting that policies that exogenously increase early job stability might have net beneficial effects. This conclusion should, however, be treated cautiously for four reasons.

First, there may be other possible explanations of the differences between the OLS results indicating no effects of early job stability, and the IV results indicating beneficial effects, which merit further investigation. Particularly given that the results from correcting for the endogeneity of early job stability are at odds with the initial hypothesis, and that the explanation of these results based on heterogeneity in the returns to search was developed ex post, there is good reason to consider other explanations.

Second, the estimates rely on the identifying assumption that the unemployment rate during the early years in the labor market is a valid instrument for early job stability. As always with instrumental variables approaches, caution should be exercised in drawing overly-strong conclusions until a consensus emerges from complementary evidence using other data sources and alternative identifying assumptions. In this particular case, however, past work by Beaudry and DiNardo (1991), and replication of that work with the data set used in this paper (which differs from the data sets they analyzed), suggest that the identifying assumptions are plausible.

Third, the policy evaluation that this paper attempts does not consider the effects of actual policies—such as school-to-work programs—implemented to encourage early job stability. Study of real-world programs is likely to lead to additional insights about alternative means by which early job stability can be encouraged, and the effects of doing so.

Finally, the evidence is silent on the issue of whether there is any need for policy intervention. In particular, individuals might choose to forego early job stability even if it would ultimately result in higher wages. Such behavior may maximize utility, and there is no obvious market failure that causes individuals to experience less job stability when young than is optimal, although their own short-sightedness may be the culprit. In addition to estimating the effects of alternative policies, the need for intervention is an important area of inquiry for those interested in policies that might transform the workings of youth labor markets in the U.S.

Endnotes

1. See the Commission on the Skills of the American Workforce (1990), Hamilton (1990), Lerman and Pouncy (1990), Glazer (1993), and other work reviewed in Heckman (1993).

2. For example, Section 3 of the Act lists as two of its purposes "to increase opportunities for minorities, women, and individuals with disabilities, by enabling individuals to prepare for careers that are not traditional for their race, gender, or disability...," and "to motivate all youths, including low-achieving youths, school dropouts, and youths with disabilities, to stay in or return to school or a classroom setting and strive to succeed, by providing enriched learning experiences and assistance in obtaining good jobs..."

3. See also Hotz, et al. (1995), and Light and McGarry (1994). GN also looked at other adult labor market outcomes, including benefits and full-time employment, and concluded similarly that early job market stability was largely unrelated to these outcomes.

4. GN attempted to remove the bias from omitted match quality by examining effects of early job market experiences for those who had changed jobs since the end of the immediate post-schooling period (defined below), although this generates its own selection bias problems by possibly discarding the best matches.

5. Because of the multitude of factors affecting selection into the sample, the standard (but in this case much more minor) issue of selection into employment is ignored. For the age ranges covered by this data set, labor force participation rates of both men and women in the U.S. are very high (e.g., for ages 25-34, the rate was 93.8 for men and 73.9 for women in 1992). In addition, as will become apparent below, it is not possible to define some of the key variables used in the wage equation for the non-employed; this makes it difficult to correct for bias from selection into employment, since variables that appear in the wage equation should also appear in the employment equation, which compares market and shadow wages.

6. Because jobs could have started before the five-year post-schooling period, this variable is greater than five for about five percent of the observations.

7. This variable may count as separate jobs incidents of individuals leaving and then returning to an employer.

8. GN relied mainly on NLSY data on the "current or most recent job," or the "CPS job." However, as reported in that paper, the OLS results using a job count based on this measure were very similar to those using the complete count. GN focused on the more restrictive job information because of interest in many additional details regarding the job, which were not available for all jobs.

Estimates for the number of jobs were also computed examining the sensitivity of the results to including the cohort entering in 1979, imputing the number of jobs held in the first year using data on jobs in the first year from the other three entering cohorts, based on the median number of jobs held in the first year by age, race, sex, education, SMSA, and marital status. The conclusions were the same as those reported below.

9. Metropolitan area unemployment rates are reported for about 75 percent of the SMSAs identified in the NLSY. The Geocode file includes the actual continuous unemployment rate. The public use file contains a measure of the unemployment rate collapsed into categories to preserve confidentiality.

10. These average rates are higher than published annual aggregate unemployment rates based on household data for these years, partly reflecting the sample composition, and partly reflecting the fact that the NLSY uses March unemployment figures based on Local Area Unemployment Statistics that are not seasonally adjusted, while March has a sizable positive seasonal factor (in the household data).

11. The samples are smaller than those in GN because of unavailable data on unemployment rates in the NLSY Geocode supplement, and because here observations from 1991 and 1990 are dropped, whereas GN used observations from these years if they were unavailable in 1992.

12. Because μ_{it} is defined over a number of years, it can be thought of as the match quality of the longest job held, as an average over this period, or even as a set of five variables for match quality in each of the five years.

13. This possibility motivated the approach used in GN of attempting to eliminate the bias from μ by looking at a sample of people who had changed jobs since the end of the five-year period.

14. Bartik (1996) argues that local employment growth provides a better measure of labor demand shifts than does local unemployment, because the latter may be influenced by efficiency wages–as suggested, perhaps, by evidence of a "wage curve" (Blanchflower and Oswald, 1990)–and labor supply shifts. However, he is specifically interested in labor demand shifts, whereas in this paper the interest is in any exogenous variation in factors affecting the ease or difficulty with which young workers obtain or lose jobs.

15. This parallels Ellwood (1982) and Corcoran (1982), who briefly describe attempts to use the local unemployment rate to predict employment or hours of young workers, in attempting to distinguish between heterogeneity and state dependence in youth labor market outcomes.

16. For about one-sixth of the observations, this minimum rate occurred during the first five years in the labor market.

17. If anything, the evidence indicates that the current wage is positively related to the current unemployment rate, perhaps reflecting findings similar to those reported by Blanchflower and Oswald (1990).

18. In addition, the estimated non-white differential is negative and significant when AFQT is excluded, paralleling results in Neal and Johnson (1996).

19. As noted in the tables, in all cases the tables report enough digits to assess whether the estimated coefficient is significant at the five- or ten-percent level.

20. The test is computed just for the coefficient of this variable, rather than the whole set of coefficients.

21. Unless otherwise specified, such statements refer to statistical significance at the five-percent level.

22. This is not surprising in this specification, since a highly-aggregated form of the unemployment rate in the immediate post-schooling period (with no geographical variation) is used.

23. Bartik (1996) argues that metropolitan areas–defined to some extent as distinct commuting areas–constitute local labor markets, while states do not.

24. Note that this does not condition on continued residence in a particular SMSA, but on continued residence in a broad set of SMSAs. As a result, this sample selection rule may induce little if any selection bias in terms of migration decisions.

25. The same specifications reported in Table 7 were also estimated for the subsample with less than 16 years of schooling, and the smaller subsample with 12 or less years of schooling, to assess whether the "returns" to early job stability vary by level of schooling. The estimates were qualitatively similar for both subsamples, with no clear pattern in the point estimates of higher or lower returns for those with less schooling.

26. This is not inconsistent with the test results reported in the table, which are tests of *overidentifying* restrictions.

27. Alternatively, in a standard life-cycle utility-maximization model (e.g., Ghez and Becker, 1975) individuals are more likely to consume leisure when their wages are low, which they may do by moving from job to job, presumably with spells of non-employment in between. Higher-wage individuals, either because of income effects or liquidity constraints, might be more likely to consume more leisure when young.

28. There is ample evidence in the search literature that unobserved heterogeneity is an important source of variation in reservation wages (Kiefer and Devine, 1991). Unobserved variation in the returns to and costs of search would generate unobserved variation in the reservation wage, although unobserved productivity differentials would have the same effect. There does not appear to be direct evidence on heterogeneity in the returns to search; given variation in the specificity of human capital across workers and jobs–which would generate variation in wage changes with job changes–it may not be possible to obtain such evidence.

29. There is an additional indirect test of this explanation of the differences between the OLS and IV estimates of the effects of early job stability. While those with higher returns to search may exhibit less early *job* stability, they would not be likely to exhibit less early *employment* stability. That is, they may move around a lot among jobs, but they probably have characteristics (including their higher returns to search) that make them more likely to be employed a larger fraction of the time during their school-to-work transition (unless income effects on leisure predominate). To test this, similar models to those reported in the tables, but substituting total experience during the five-year post-schooling period for the early job stability measures, were estimated (excluding "adult" experience, corresponding to the exclusion of "adult" tenure in Tables 2-5). The prediction is that for early experience, in contrast to early job stability, the IV estimates should be no more likely to indicate positive returns than the OLS estimates. This was indeed the case. While the OLS estimates were significant and positive, the IV estimates were no larger and were insignificant, and the p-values from Hausman tests of the exogeneity of early experience were in the .6-.9 range, rather than .05 or less for early job stability. However, in general for these specifications the instrumental variables were less informative.

30. Some evidence reported by Hashimoto and Miller (1997) is potentially consistent with the existence of these competing beneficial effects of job stability and job shopping. They find that separations during the early years in the labor market that are due to firing or to quits for family reasons are negatively related to adult wages; these may represent exogenously determined separations, and hence reflect benefits of early job stability *per se*, although these results may also reflect heterogeneity bias (e.g., who gets fired). On the other hand, early quits for other (unspecified) reasons are positively related to adult wages, which may reflect returns to search.

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Table 1. Descriptive Statistics

	Men (1)	Women (2)
Adult variables:		
Log adult wage	6.81	6.55
	(.54)	(.47)
Schooling	11.92	12.32
	(1.65)	(1.45)
Experience	10.69	9.43
	(2.42)	(3.18)
Tenure	4.42	4.29
	(4.11)	(4.08)
Currently married	.54	.58
Non-white	.26	.28
Current unemployment rate	8.16	7.98
	(2.48)	(2.41)
Minimum unemployment rate on current job	4.99	4.87
	(1.76)	(1.74)
Variables from initial five-year post-schooling period:		
Longest tenure attained	2.84	2.55
	(1.65)	(1.59)
Number of jobs held	3.72	3.20
	(2.63)	(2.24)
Year of labor market entry	1980.6	1980.6
Unemployment rate, by year of entry		
U_{ij1}	8.07	7.85
	(3.01)	(2.75)
U_{ij2}	9.51	9.23
	(3.68)	(3.38)
U_{ij3}	9.86	9.71
	(3.71)	(3.62)
U_{ij4}	9.86	8.52
	(3.78)	(3.80)
U_{ij5}	9.17	8.81
	(3.75)	(3.60)
Average unemployment rate, five post-schooling years		
$U_{.j.}$	9.30	8.99
	(2.94)	(2.79)

For most rows, there are 860 observations for men, and 773 observations for women. The number of jobs held variable in this table is defined only for those entering the labor market in 1980-1982, so the sample sizes fall to 693 men and 635 women. For the average unemployment rate over the initial five post-schooling years, there are more observations, because we compute the average even if data are missing for some years. Means are reported, with standard deviations in parentheses.

Table 2: Results for Effects of Longest Tenure Attained on Log Adult Wage, Men

	OLS	First stage	IV	First stage	IV	First stage	IV	First stage	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Longest tenure attained, five-year post-schooling period	.01	...	.08	...	.08	...	.07	...	.13
	(.01)		(.05)		(.04)		(.03)		(.06)
Schooling	.04	.08	.03	.04	.03	.04	.03	.09	.03
	(.01)	(.03)	(.01)	(.03)	(.01)	(.03)	(.01)	(.03)	(.01)
Experience	.06	-.05	.06	-.20	.06	-.20	.06	-.01	.05
	(.03)	(.10)	(.04)	(.10)	(.04)	(.10)	(.04)	(.10)	(.04)
Experience squared (10^{-2}	-.17	2.2	-.25	3.4	-.25	3.4	-.25	2.0	-.31
	(.18)	(.55)	(.19)	(.56)	(.19)	(.56)	(.19)	(.53)	(.20)
Currently married	.17	-.07	.17	-.10	.17	-.11	.17	-.02	.17
	(.03)	(.10)	(.03)	(.10)	(.03)	(.10)	(.03)	(.10)	(.03)
Non-white	-.05	-.07	-.05	-.03	-.05	-.03	-.05	-.03	-.05
	(.05)	(.14)	(.05)	(.14)	(.05)	(.14)	(.05)	(.13)	(.05)
Minimum unemployment rate on current job	-.04	.15	-.05	.11	-.05	.13	-.05	.15	-.05
	(.01)	(.05)	(.02)	(.04)	(.02)	(.05)	(.02)	(.05)	(.02)
Current unemployment rate	.01	-.04	.02	-.03	.02	-.03	.02	-.06	.02
	(.01)	(.03)	(.01)	(.03)	(.01)	(.03)	(.01)	(.03)	(.01)
U_{ij1}	...	-.00	...	...	...	...	...	...	...
		(.03)							
U_{ij2}	...	.11	...	...	...	...	...	...	...
		(.03)							
U_{ij3}	...	-.01	...	...	...	...	...	...	...
		(.02)							
U_{ij4}	...	-.05	...	...	...	...	...	...	...
		(.02)							
U_{ij5}	...	-.06	...	...	...	...	...	...	...
		(.02)							
$U_{.j1}$	...	...	...	.30	...	.30	...	...	...
				(.05)		(.05)			
$U_{.j3}$	...	...	...	.04	...	.04	...	...	...
				(.04)		(.04)			
$U_{.j5}$	...	...	...	-.13	...	-.13	...	...	...
				(.04)		(.05)			
$U_{.j.}$	...	...	...	...	...	...	...	1.12	...
								(.20)	
$(U_{ij1}-U_{.j1})$	...	...	...	...	...	-.04	...	...	...
						(.03)			
$(U_{ij2}-U_{.j2})$	...	...	...	...	...	.04	...	...	...
						(.03)			
$(U_{ij3}-U_{.j3})$	...	...	...	...	...	-.00	...	...	...
						(.03)			
$(U_{ij4}-U_{.j4})$	...	...	...	...	...	-.02	...	...	...
						(.03)			
$(U_{ij5}-U_{.j5})$	...	...	...	...	...	.00	...	...	...
						(.03)			
$(U_{ij.}-U_{.j.})$	...	...	...	...	...	...	...	-.006	...
								(.02)	
F-statistic for instruments in first stage	...	11.4	...	34.3	...	13.1	...	16.1	...
P-value for Hausman exogeneity test	...	...	.12	...	.02	...	.04	...	.06
P-value for test of overidentifying restrictions	...	...	.18	...	.78	...	.45	...	.38
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded	...	...	.02	...	.80	...	.09	...	.01

There are 860 observations in columns (1)-(7), and 942 in columns (8)-(9). In addition to the reported coefficients, controls are included for residence in an SMSA and four Census regions, AFQT (standardized for age), and mother's and father's education (with dummy variables for missing data). Because there are only four entry cohorts, only three of the coefficients of the $U_{.jt}$ can be identified. In this and the following tables, enough digits are reported to assess whether the estimated coefficients are significant at the five- or ten-percent level. The Hausman test is computed only for the coefficient of the instrumented variable.

Table 3: Results for Effects of Longest Tenure Attained on Log Adult Wage, Women

	OLS (1)	First stage (2)	IV (3)	First stage (4)	IV (5)	First stage (6)	IV (7)	First stage (8)	IV (9)
Longest tenure attained, five-year post-schooling period	.02 (.01)	...	.24 (.08)	...	.16 (.04)	...	.12 (.04)	...	-.06 (.15)
Schooling	.05 (.01)	.13 (.04)	.02 (.02)	.10 (.03)	.03 (.01)	.10 (.03)	.04 (.01)	.14 (.03)	.06 (.02)
Experience	.02 (.02)	.04 (.07)	-.01 (.03)	-.03 (.06)	.00 (.02)	-.02 (.06)	.00 (.02)	.07 (.06)	.02 (.02)
Experience squared (10^{-2}	.16 (.12)	1.7 (.40)	-.10 (.17)	2.2 (.39)	-.01 (.14)	2.2 (.39)	.04 (.13)	1.4 (.38)	.25 (.23)
Currently married	.02 (.03)	.12 (.09)	.00 (.04)	.11 (.09)	.01 (.03)	.10 (.09)	.01 (.03)	.06 (.09)	.02 (.03)
Non-white	.03 (.04)	.13 (.13)	.00 (.05)	.15 (.12)	.01 (.04)	.18 (.12)	.02 (.04)	.13 (.12)	.05 (.04)
Minimum unemployment rate on current job	-.07 (.01)	.03 (.05)	-.08 (.02)	.05 (.04)	-.08 (.01)	-.02 (.05)	-.08 (.01)	-.01 (.04)	-.07 (.01)
Current unemployment rate	.03 (.01)	-.06 (.03)	.04 (.01)	-.04 (.03)	.03 (.01)	-.05 (.03)	.03 (.01)	-.04 (.03)	.02 (.01)
U_{ij1}	...	.01 (.03)	...	...	...	...	...	...	...
U_{ij2}	...	.08 (.02)	...	...	...	...	...	...	...
U_{ij3}	...	-.01 (.02)	...	...	...	...	...	...	...
U_{ij4}	...	-.02 (.02)	...	...	...	...	...	...	...
U_{ij5}	...	-.01 (.02)	...	...	...	...	...	...	...
$U_{.j1}$	...	...	...	.34 (.12)	...	.34 (.12)	...	...	...
$U_{.j3}$	...	...	...	.02 (.04)	...	.03 (.04)	...	...	...
$U_{.j5}$	...	...	...	-.02 (.08)	...	-.03 (.08)	...	...	...
$U_{.j.}$	...	...	...	...	...	...	...	-.24 (.19)	...
$(U_{ij1}-U_{.j1})$	...	...	...	...	...	-.02 (.03)	...	...	...
$(U_{ij2}-U_{.j2})$	...	...	...	...	...	-.01 (.03)	...	...	...
$(U_{ij3}-U_{.j3})$	...	...	...	...	...	.00 (.03)	...	...	...
$(U_{ij4}-U_{.j4})$	...	...	...	...	...	.02 (.03)	...	...	...
$(U_{ij5}-U_{.j5})$	...	...	...	...	...	.05 (.03)	...	...	...
$(U_{ij.}-U_{.j.})$	...	...	...	...	...	...	...	.04 (.03)	...
F-statistic for instruments in first stage	...	5.3	...	24.9	...	10.8	...	2.4	...
P-value for Hausman exogeneity test	...	...	.00	...	.00	...	.00	...	.65
P-value for test of overidentifying restrictions	...	...	.48	...	.89	...	.16	...	.46
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded	...	...	.00	...	.97	...	.00	...	.09

There are 773 observations in columns (1)-(7), and 842 in columns (8)-(9). See notes to Table 2.

Table 4: Results for Effects of Number of Jobs Held on Log Adult Wage, Men

	OLS (1)	First stage (2)	IV (3)	First stage (4)	IV (5)	First stage (6)	IV (7)	First stage (8)	IV (9)
Number of jobs held, five-year post-schooling period	-.014 (.007)	...	.00 (.06)	...	-.08 (.04)	...	-.03 (.04)	...	-.06 (.04)
Schooling	.04 (.01)	-.18 (.07)	.04 (.02)	-.15 (.07)	.024 (.015)	-.15 (.07)	.03 (.01)	-.19 (.06)	.03 (.01)
Experience	.06 (.04)	1.3 (.23)	.04 (.02)	1.4 (.23)	.14 (.07)	1.5 (.23)	.08 (.06)	1.4 (.22)	.12 (.06)
Experience squared (10^{-2}	-.12 (.23)	-8.2 (1.3)	-.00 (.50)	-9.5 (1.3)	-.62 (.41)	-9.6 (1.3)	-.23 (.35)	-9.3 (1.2)	-.47 (.39)
Currently married	.17 (.04)	.05 (.20)	.17 (.04)	.07 (.20)	.17 (.04)	.08 (.20)	.17 (.04)	.06 (.19)	.17 (.04)
Non-white	-.07 (.05)	-.35 (.28)	-.07 (.05)	-.35 (.28)	-.09 (.06)	-.43 (.28)	-.08 (.06)	-.21 (.26)	-.09 (.05)
Minimum unemployment rate on current job	-.04 (.02)	-.14 (.11)	-.04 (.02)	-.23 (.09)	-.06 (.02)	-.15 (.11)	-.05 (.02)	-.20 (.09)	-.05 (.02)
Current unemployment rate	.016 (.011)	.11 (.06)	.01 (.01)	.08 (.06)	.02 (.01)	.11 (.06)	.02 (.01)	.12 (.06)	.015 (.011)
U_{ij1}	...	.04 (.07)	...	...	...	...	...	...	...
U_{ij2}	...	-.11 (.06)	...	...	...	...	...	...	...
U_{ij3}	...	-.003 (.04)	...	...	...	...	...	...	...
U_{ij4}	...	.08 (.05)	...	...	...	...	...	...	...
U_{ij5}	...	-.10 (.07)	...	...	...	...	...	...	...
$U_{.j1}$	...	...	...	-.40 (.09)	...	-.41 (.09)	...	...	...
$U_{.j3}$	...	...	...	-.16 (.08)	...	-.17 (.08)	...	...	...
$U_{.j.}$	...	...	...	...	...	...	...	-1.6 (.37)	...
$(U_{ij1}-U_{.j1})$	...	...	...	...	...	.09 (.07)	...	...	...
$(U_{ij2}-U_{.j2})$	...	...	...	...	...	-.02 (.06)	...	...	...
$(U_{ij3}-U_{.j3})$	...	...	...	...	...	-.01 (.06)	...	...	...
$(U_{ij4}-U_{.j4})$	...	...	...	...	...	-.08 (.07)	...	...	...
$(U_{ij5}-U_{.j5})$	...	...	...	...	...	-.03 (.07)	...	...	...
$(U_{ij.}-U_{.j.})$	...	...	...	...	...	...	...	-.08 (.05)	...
F-statistic for instruments in first stage	...	1.9	...	9.4	...	3.6	...	10.4	...
P-value for Hausman exogeneity test	...	...	.80	...	.13	...	.67	...	.27
P-value for test of overidentifying restrictions	...	...	.08	...	.90	...	.18	...	.08
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded	...	...	.05	...	.89	...	.03	...	.00

There are 693 observations in columns (1)-(7), and 761 in columns (8)-(9). See notes to Table 2.

Table 5: Results for Effects of Number of Jobs Held on Log Adult Wage, Women

	OLS	First stage	IV	First stage	IV	First stage	IV	First stage	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Number of jobs held,	-.00	...	.03	...	-.14	...	.00	...	.07
five-year post-schooling period	(.01)		(.05)		(.10)		(.04)		(.05)
Schooling	.06	-.04	.06	-.04	.05	-.05	.06	-.04	.06
	(.01)	(.07)	(.01)	(.07)	(.02)	(.07)	(.01)	(.07)	(.01)
Experience	.01	.90	-.01	.93	.14	.93	.01	.85	-.05
	(.02)	(.14)	(.05)	(.14)	(.10)	(.14)	(.04)	(.14)	(.05)
Experience squared (10^{-2}	.25	-6.1	.40	-6.3	-.61	-6.3	.27	-5.8	.65
	(.15)	(.87)	(.33)	(.88)	(.65)	(.88)	(.28)	(.84)	(.35)
Currently married	.02	-.36	.03	-.31	-.02	-.37	.02	-.26	.03
	(.03)	(.18)	(.03)	(.18)	(.05)	(.18)	(.03)	(.17)	(.03)
Non-white	.03	-.98	.06	-.91	-.10	-.93	.03	-.86	.10
	(.04)	(.25)	(.06)	(.25)	(.11)	(.25)	(.06)	(.24)	(.06)
Minimum unemployment rate	-.07	.10	-.07	-.05	-.08	.12	-.07	.14	-.07
on current job	(.01)	(.10)	(.01)	(.08)	(.02)	(.10)	(.01)	(.09)	(.01)
Current unemployment rate	.02	.08	.02	.01	.03	.07	.02	.05	.03
	(.01)	(.06)	(.01)	(.06)	(.01)	(.06)	(.01)	(.05)	(.01)
U_{ij1}	...	-.08	...	...	...	...	...	...	...
		(.06)							
U_{ij2}	...	-.01	...	...	...	...	...	...	...
		(.05)							
U_{ij3}	...	-.06	...	...	...	...	...	...	...
		(.04)							
U_{ij4}	...	-.02	...	...	...	...	...	...	...
		(.05)							
U_{ij5}	...	-.02	...	...	...	...	...	...	...
		(.07)							
$U_{.j1}$	...	...	...	-.26	...	-.26	...	...	...
				(.14)		(.14)			
$U_{.j3}$	...	...	...	-.17	...	-.18	...	...	...
				(.08)		(.08)			
$U_{.j.}$	...	...	...	...	...	...	...	-.31	...
								(.40)	
$(U_{ij1}-U_{.j1})$	...	...	...	...	...	-.07	...	...	...
						(.05)			
$(U_{ij2}-U_{.jt})$	...	...	...	...	...	.01	...	...	...
						(.05)			
$(U_{ij3}-U_{.j3})$	...	...	...	...	...	-.01	...	...	...
						(.06)			
$(U_{ij4}-U_{.j4})$	...	...	...	...	...	-.13	...	...	...
						(.06)			
$(U_{ij5}-U_{.j5})$	...	...	...	...	...	.02	...	...	...
						(.07)			
$(U_{ij.}-U_{.j.})$	...	...	...	...	...	...	...	-.17	...
								(.05)	
F-statistic for instruments in first stage	...	2.5	...	2.4	...	2.9	...	6.2	...
P-value for Hausman exogeneity test	...	...	.60	...	.16	...	.93	...	.18
P-value for test of overidentifying restrictions	...	...	.00	...	.05	...	.01	...	.07
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded	...	...	.00	...	.03	...	.00	...	.44

There are 635 observations in columns (1)-(7), and 697 in columns (8)-(9). See notes to Table 2.

Table 6: Estimates Including Contemporaneous Tenure Controls

	Men (1)	Women (2)	Men (3)	Women (4)
OLS log wage equation estimates:				
Longest tenure attained, five-year post-schooling period	-.006 (. 01)	.015 (.012)	...	...
Number of jobs held, five-year post-schooling period	...	...	-.008 (.007)	.004 (.007)
IV log wage equation estimates:				
Longest tenure attained, five-year post-schooling period	.05 (.04)	.14 (.04)	...	...
Number of jobs held, five-year post-schooling period	...	...	-.06 (.05)	.04 (.04)
Tenure	.04 (.01)	.08 (.02)	.03 (.02)	.07 (.01)
Tenure squared (10^{-2}	-.19 (.12)	-.55 (.13)	-.22 (.15)	-.35 (.11)
Minimum unemployment rate on current job	-.05 (.02)	-.08 (.01)	-.06 (.02)	-.08 (.01)
Current unemployment rate	.02 (.01)	.03 (.01)	.02 (.01)	.02 (.01)
First stage estimates:				
$U_{.j1}$	.29 (.05)	.36 (.11)	-.33 (.09)	-.19 (.13)
$U_{.j3}$	.02 (.04)	.03 (.04)	-.12 (.08)	-.16 (.08)
$U_{.j5}$	-.13 (.04)	-.00 (.08)	...	...
$(U_{ij1}-U_{.j1})$	-.03 (.03)	...	...	-.07 (.05)
$(U_{ij2}-U_{.j2})$	.02 (.03)	...	...	.02 (.05)
$(U_{ij3}-U_{.j3})$	.01 (.03)	...	...	-.00 (.05)
$(U_{ij4}-U_{.j4})$	-.02 (.03)	...	...	-.13 (.06)
$(U_{ij5}-U_{.j5})$	.01 (.03)	...	...	.00 (.07)
F-statistic for instruments in first stage	13.4	26.0	6.6	2.6
P-value for Hausman exogeneity test	.12	.00	.30	.43
P-value for test of overidentifying restrictions	.44	.98	.85	.11
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded.	.07	.99	.84	.02

Specifications in columns (1) and (4) correspond to those in columns (6) and (7) of Tables 2 and 5. Specifications in columns (2) and (3) correspond to those in columns (4) and (5) of Tables 3 and 4. Only selected coefficients of the log wage equations are shown.

Table 7. Estimates Restricted to SMSA Observations

	Excluding tenure				Including tenure			
	Men	Women	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS log wage equation estimates:								
Longest tenure attained, five-year post-schooling period	.023	.017	...	...	.015	.023	...	...
	(.016)	(.014)			(. 016)	(.015)		
Number of jobs held, five-year post-schooling period	...	...	-.022	-.010	...	...	-.015	-.007
			(.009)	(.009)			(.009)	(.009)
IV log wage equation estimates:								
Longest tenure attained, five-year post-schooling period	.11	.18	...	...	.073	.14	...	...
	(.05)	(.06)			(.045)	(.05)		
Number of jobs held, five-year post-schooling period	...	...	-.14	.015	...	...	-.121	.03
			(.07)	(.05)			(.074)	(.05)
Tenure	...	...	...	...	.06	.09	.05	.05
					(.02)	(.02)	(.02)	(.02)
Tenure squared (10^{-2}	...	...	...	...	-.31	-.62	-.41	-.25
					(.16)	(.18)	(.22)	(.15)
Minimum unemployment rate on current job	-.08	-.08	-.11	-.08	-.08	-.08	-.10	-.08
	(.02)	(.02)	(.04)	(.02)	(.02)	(.02)	(.04)	(.02)
Current unemployment rate	.05	.04	.08	.04	.05	.04	.07	.03
	(.02)	(.01)	(.02)	(.01)	(.01)	(.01)	(.02)	(.01)
First stage estimates:								
$U_{.j1}$	.26	.48	-.45	-.33	.28	.61	-.39	-.26
	(.10)	(.29)	(.15)	(.21)	(.09)	(.27)	(.14)	(.21)
$U_{.j3}$	-.08	.02	-.15	-.24	-.10	.05	-.10	-.24
	(.07)	(.10)	(.13)	(.12)	(.07)	(.09)	(.12)	(.12)
$U_{.j5}$	-.20	.09	...	...	-.21	.17	...	...
	(.10)	(.20)			(.09)	(.19)		
$(U_{ij1}-U_{.j1})$	-.02	...	...	-.03	-.01	...	...	-.03
	(.05)			(.07)	(.05)			(.07)
$(U_{ij2}-U_{.j2})$	.09	...	...	-.04	.06	...	...	-.04
	(.05)			(.07)	(.05)			(.07)
$(U_{ij3}-U_{.j3})$	-.07	...	...	.07	-.05	...	...	.08
	(.05)			(.08)	(.05)			(.08)
$(U_{ij4}-U_{.j4})$	.03	...	...	-.20	.04	...	...	-.21
	(.05)			(.10)	(.05)			(.09)
$(U_{ij5}-U_{.j5})$	.03	...	...	.07	-.03	...	...	.08
	(.05)			(.10)	(.05)			(.09)
F-statistic for instruments in first stage	7.2	10.7	4.7	1.5	8.6	12.8	3.7	1.7
P-value for Hausman exogeneity test	.05	.01	.07	.63	.16	.02	.15	.48
P-value for test of overidentifying restrictions	.32	.97	.91	.11	.32	.85	.77	.37
P-value for test of overidentifying restrictions when minimum unemployment rate on current job is excluded	.02	.98	.90	.02	.02	.86	.76	.11
N	474	434	384	363	474	434	384	363

Specifications in columns (1), (4), (5), and (8) correspond to those in columns (6) and (7) of Tables 2 and 5. Specifications in columns (2), (3), (6), and (7) correspond to those in columns (4) and (5) of Tables 3 and 4. Only selected coefficients of the log wage equations are shown. Observations are restricted to the subset of those individuals residing–in the early labor market years and 1992–in the set of SMSAs for which separate unemployment rates are reported in *Employment and Earnings* for March of each year.