ORDER FROM CHAOS? THE EFFECTS OF EARLY LABOR MARKET EXPERIENCES ON ADULT LABOR MARKET OUTCOMES

Rosella Gardecki
David Neumark

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ABSTRACT

This paper examines the consequences of initial periods of "churning," "floundering about," or "mobility" in the labor market to help assess whether faster transitions to stable employment relationships--such as those envisioned by advocates of school-to-work programs--would be likely to lead to better adult labor market outcomes. Our interpretation of the results is that there is at best modest evidence linking early job market stability to better labor market outcomes. We find that adult labor market outcomes (defined as of the late 20s or early to mid-30s) are for the most part unrelated to early labor market experiences for both men and women. This evidence does not provide a compelling case for efforts to explicitly target the school-to-work transition, insofar as this implies changing the structure of youth labor markets so that workers become more firmly attached to employers, industries, or occupations at younger ages.

Rosella Gardecki
Center for Human Resource Research
921 Chatham Lane, Suite 200
Columbus, OH 43221

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

I. Introduction

The goal of the 1994 School-to-Work Opportunities Act is to create an integrated system of youth education, job training, and labor market information, to provide a faster and more successful transition from school to stable employment. The motivation for policies to change school-to-work transitions is the "churning" or "milling about" experienced by youths in the U.S. upon their entry into the labor market, in the form of initial periods of joblessness or a series of "dead-end" jobs, as discussed in a 1990 report of the General Accounting Office. Researchers who advocate integrated school-to-work programs often adopt the position that this chaotic period is costly, and that some type of intervention is needed to impose more order on the transitions of youths to career jobs. For example, a 1994 report of the National Center for Research in Vocational Education (NCRVE) argues that the years spent "floundering" from one job to another represent a "waste of human resources." An integrated school-to-work system is intended to address the school-to-work problem 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 empirical description of youth labor markets in the U.S. as chaotic is probably appropriate, at least for a substantial portion of youths. However, there are two issues that must be addressed in assessing policies that aim in part to create more stable and orderly employment during the school-to-work transition. The first, which is not addressed in this paper, concerns the efficacy of school-to-work programs in relation to transforming the functioning of youth labor markets. The NCRVE report provides a thorough compendium of research on such programs. While this paper is not the place for a critical review of this research, we think it is safe to say that there is, as yet, relatively little persuasive evidence of positive impacts on adult labor market outcomes. First of all, very few studies have focused on labor market outcomes more than a year or two after completion of the programs.

Yet there is evidence that over a period of a few years the beneficial effects of some types of programs disappear, as comparison group members get jobs on their own and the advantages experienced by participants disappear. Second, many studies do not construct a reasonable comparison group, let alone use random assignment. Third, even those that attempt to construct a reasonable comparison group find no beneficial short-term labor market effects, with the possible exception of those students who remained with the employer with whom they "apprenticed" during the program. Finally, some of the evidence suggests that school-to-work programs may discourage postsecondary education. In our view, however, definitive conclusions about explicit school-to-work programs are premature; alternative programs, or alternative methods of evaluating them, may yield more encouraging results.

The second issue, which is the focus of this paper, is the need for school-to-work programs. While school-to-work advocates view the chaotic nature of youth labor markets in the U.S. as costly, labor economics provides alternative perspectives. On the one hand, much research documents positive returns to training and job attachment, suggesting that it is optimal to get workers into steady jobs with lots of training as soon as possible after leaving school. On the other hand, there is ample evidence that workers receive positive returns to job shopping. In this case, funneling workers more quickly into long-term jobs could prove counter-productive if workers who may have otherwise found a good match with an employer no longer do so. Ultimately, the question is whether school-to-work programs would result in better matches than the decentralized methods currently used.

In this paper we seek to better inform the debate regarding school-to-work transitions. It seems to us that, minimally, a case for attempting to replace current methods of job shopping requires evidence that those youths who experience unstable employment, or "floundering about" in their early years in the labor market, suffer longer-term consequences in the labor market. We consider whether there is such evidence by exploring the relationship between a wide range of youth labor market experiences and labor market outcomes of more mature adults, asking whether youths who appear to be in unstable or dead-end jobs early in their careers suffer adverse labor market consequences as adults. We look at wages, benefits, and full-time work, as measures of adult labor market success.

Our evidence has two limitations. First, job matching models imply that the cross-sectional association between adult wages and early job stability is not necessarily 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. If policies to increase job stability do not yield matches as good as those produced by current decentralized methods, then the cross-sectional association overestimates the effect of such policies. From a policy perspective, this means that we have to think carefully about how we might move to a system of more orderly labor market transitions without reducing the quality of job matches. Of more direct concern for this paper, in attempting to estimate the effects of early labor market experiences we must try to eliminate biases from unobserved heterogeneity that leads to less chaotic youth labor market experiences as well as superior adult labor market outcomes; such heterogeneity may be job-match specific as well as individual specific. While we take numerous steps to attempt to eliminate such biases, we may not be completely successful, suggesting that the estimates that we present are best interpreted as upper-bound estimates of any positive effects of early job market stability on adult labor market outcomes. However, given our general findings that less chaotic youth labor market experiences have little effect on adult labor market outcomes, further reduction of heterogeneity bias would only strengthen our conclusions.

Second, our evidence is silent on the issue of whether there is any need for policy intervention. In particular, we would like to know whether there is any sort of market failure that causes individuals to experience less job stability when young than is optimal. We are not aware of any research that addresses this market failure or efficiency question, and we do not address it in this paper. Instead, our evidence addresses the less ambitious but nonetheless important distributional question of whether more orderly transitions would boost the labor market prospects of young workers, particularly those who end up in relatively lower-wage jobs.

II. Existing Research

Most research on youth labor market experiences focuses on short-run employment problems, or short-run effects of training, with little attention paid to the consequences of these experiences for adult career outcomes. A substantial body of research studies the unemployment/employment experiences of young workers in various demographic groups, and their short-term effects on wages (e.g., Ellwood 1982). This research tends to find that there is no permanent "scarring" effect of early unemployment. The only lasting effect is that workers who experience such unemployment accumulate less labor market experience, and because of this may earn less subsequently. Other research studies the short-term effects of labor market training, education, or apprenticeships on early labor market experiences (e.g., Blanchflower and Lynch 1994; Glover 1995; Hotz, et al. 1995; Lynch 1992) and on wages (e.g., Barron, et al. 1989; Bishop 1994; Hotz, et al. 1995; Mincer 1991), generally finding positive effects on wages.

Much less research addresses the issue of job and employment stability, especially longer-term effects. Klerman and Karoly (1994) use the National Longitudinal Survey of Youth (NLSY) to document the amount of "floundering" or "milling about" in which workers engage in their early 20s. They conclude that by their early 20s, most workers have entered a stable job--defined as those lasting at least one year--although high-school dropouts fare worse. However, they do not explore the consequences of different rates of transition to stable employment for later labor market outcomes.

Two papers explicitly address questions relating to "dead-end" jobs. Brown (1982) uses Census of Population data to attempt to characterize jobs as "dead end," based on either within-occupation wage growth or the probability that workers in an occupation remain in the same industry (over a five-year period). He concludes that the industry retention rate is negatively related to unemployment, so that "dead-end" jobs--i.e., those with low retention rates--are associated with a higher likelihood of unemployment. But the results for wage growth are ambiguous. Lynch (1993) studies labor market experiences in entry-level jobs, finding some evidence that on-the-job training decreases the probability of leaving one's first employer, while off-the-job training increases it.

Evidence on the links between youth labor market experiences and later careers is more scant. Most of this research focuses exclusively on training and ignores other dimensions of early labor market experiences (e.g., Adams and Mangum 1986; Murnane, et al. 1994). Lillard and Tan (1986) use data from various NLS cohorts to document some longer-term effects of training, showing that company training has positive effects on wages that last for about 11 years, and that vocational training is associated with less likelihood of unemployment, an effect that lasts for about 12 years. However, they do not address self-selection problems with regard to who receives training, and do not look at dimensions of early labor market experiences other than training.

Like Klerman and Karoly, Light and McGarry (1994) also present evidence on the evolution of job stability among young workers. In addition, though, they examine the consequences of job stability or mobility for wages, finding that early mobility is associated with higher wage growth, consistent with job matching (see also Topel and Ward 1992). On the other hand, mobility that occurs after the first two years in the labor market tends to be associated with lower wage growth, even controlling (albeit with weak identification) for individual-specific effects. The most detailed study of youth labor market experiences and their longer-term consequences is Gritz and MaCurdy (1992), who specify a Markov transition model for five possible states: low-wage employment, high-wage employment, combined low-wage and high-wage employment, training, and nonemployment. Their research is not directed explicitly towards the issues that we consider, but a number of their results pertain to these issues. For example, they find: 1) that there is a great deal of mobility out of low-wage jobs and into high-wage jobs, and relatively little mobility in the other direction, 2) that low-wage jobs are held for relatively short periods, and 3) that training in the early years in the labor market is associated with only marginal increases in employment.

Our work differs from these studies in a number of potentially important ways. First, we take a longer-term view than most of the existing work, examining the links between early labor market experiences and adult experiences. Second, we use a number of methods to reduce the influence of unobserved heterogeneity. Third, we examine a much wider set of early labor market experiences. Fourth, we look at significant adult labor market outcomes in addition to wages, including health and retirement benefits and full-time job holding.

III. The Data and Sample

We use the NLSY for the years 1979-1992, which provides 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. The sample is first restricted to individuals who were neither in the military subsample nor reported any military duty through 1992. We also eliminated any individuals with non-interviews between 1979 and 1986 (for reasons described below), and individuals missing the enrollment data required to date their labor market entry. As Table 1 shows, these restrictions reduce the sample to 9,083.

The next set of restrictions on the sample are imposed to focus on individuals' first years in the labor market. We chose a window of five years, reasoning that this window would be sufficiently long to observe many individuals' transitions from their earliest entrance into the labor market into steadier employment, possibly accompanied by training. The tradeoff is that the longer the window we use, the smaller the sample gets, as it becomes more likely that we get a non-interview, or that some of the questions change and become unusable.[2] One constraint is that the training questions were asked on a consistent basis through 1986, after which they changed and became non-comparable; thus, we cannot look at five-year windows that end after 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 successful school-to-work transitions may entail course work at community or other two-year colleges, we did not necessarily want to restrict our analysis to the period after individuals report no additional schooling. Thus, we chose to date entrance into the labor market as the first year in which individuals no longer report schooling other than that which occurred at two-year colleges.[3] This lets us treat community college enrollment in the same way that we study training, for example.

The effects of these sample restrictions are documented in Table 1. Also, to understand how these restrictions affect the sample composition, we report the mean completed educational level and the 1992 employment rate of the remaining sample. We first delete all observations in school throughout the entire 1979-1986 period, which is a bit over 450 observations. Not surprisingly, this drops those who get the most education, so that mean education in the remaining sample falls. Next, we delete those who first left school in the 1979-1982 period but then returned after 1982, so that the first year after permanently leaving school, as we define it, is too late to be included in our sample. This is an additional 1,400 or so observations, who also have relatively higher schooling. Finally, we delete those whose first year after permanently leaving school is before 1979,[4] or who first left school after 1982. This is the most substantial reduction in the sample size, and because it tends to eliminate those who stayed in school longer, also results in a less-educated final sample. In addition, the reported 1992 employment rates show that these sample selection rules tend to drop those with higher employment rates. Insofar as the school-to-work transition is more problematic for less-educated workers who have later employment problems, our sample is probably more representative of the types of workers in whom we are most interested.

In addition to losing some additional observations with missing or inconsistent data on training spells, or missing data on other variables, we also drop respondents who were not observed in a job during the survey week at any time during the five-year post-schooling period, because we are studying characteristics of early jobs. As shown in Table 1, this is only 6.2 percent of the sample, suggesting that this restriction does not end up obscuring a major part of the youth employment problem. Other than that, there is no lower limit on the amount of time they had to have worked during that period. We are left with a final sample of 2,844 observations with data on the five-year post-schooling period.[5]

These sample selection criteria raise the issue of sample selection bias in the estimates reported below. For the most part, whether or not respondents to the NLSY are in our sample depends on their age. Individuals who are too young may not get into the sample because they do not accumulate five (potential) years in the labor market by 1986, while individuals who are too old may have entered before 1979. However, inclusion in the sample will also be affected by schooling decisions, which affect the timing of labor market entry as we measure it. In particular, we are less likely to include in the sample older members of the NLSY cohort who get little schooling, and younger members who get lots of schooling. This type of sample selection may bias estimates of the return to schooling, if wage offers affect enrollment decisions (see Griliches 1977). However, evidence suggests that such bias may be quite minor, and given that only a fraction of this bias is likely to be transmitted to the other coefficients we are trying to estimate, our results should be largely unaffected by bias from selection into the sample based on schooling.

When we analyze adult wages, we require data on wages for at least one of the years in the 1990-1992 period, as well as valid data on the standard ingredients of wage regressions for the same year. The wage we use is from the CPS questions in the NLSY, and refers to the main job held in the previous week. The sample size for this analysis falls to 2,392.[6] The sample sizes for the analysis of benefits is smaller because of unavailable data for self-employed and part-time workers, and larger when we look at full-time work, because non-employed individuals are then included.

IV. Labor Market Experiences in the Five-Year Post-Schooling Period

Table 2 provides descriptive information on labor market experiences in the first five years after leaving school.[7] As shown in the first column, as of the fifth post-schooling year 15 percent of respondents report one or more years in which they were enrolled in community college. (Of these, a very small number report two or more such years.) The next three columns report on spells of training as of the end of the five-year post-schooling period.[8] As the bottom row of column (2) shows, by the fifth year 27 percent had some type of training during this period. Of these, most had only one spell of training, and only a handful had more than three spells of training. A relatively low number of respondents (8 percent) report one or more spells of on-the-job training over the five-year period, while off-the-job training is by far more prevalent.[9] We are also interested in the duration of training.[10] Although not reported in the table, as of the fifth post-schooling year a bimodal distribution of the duration of training is apparent, with 30 percent of those with training reporting 50 or more weeks of training, and 18 percent reporting four or less weeks of training. This same pattern appears for both on-the-job and off-the-job training.

Columns (5) and (6) of Table 2 turn to labor market or job attachment. Column (5) reports on the amount of work experience accumulated in the five-year post-schooling period, which is constructed from actual weeks worked, unweighted by hours. As the bottom row shows, as of the end of the fifth post-schooling year, individuals had an average of 49.5 months of experience. However, the high average level of labor market attachment reflects considerable heterogeneity. In results not shown in the table, we find that over the first two post-schooling years about 50 percent of the sample had acquired ten or more months of work experience per year, reflecting nearly full-year employment; this percentage rises above 60 percent further into the post-schooling period, consistent with some individuals moving from intermittent (or no) employment to full-year employment. The remaining half (approximately) of the sample that does not work full-year in each year is relatively uniformly distributed across the remaining range of number of months, indicating many individuals who had considerably less than full-time attachment. Column (6) instead looks at the duration and stability of the longest job held. On average the longest job held in this period lasted over 128 weeks, or just over two years. Also, as we would expect, unreported results indicate that the proportion of longest jobs with relatively low tenure drops as we move through the post-schooling period, although this proportion remains relatively high. For example, by the fifth year about 18 percent of the longest tenure jobs were four quarters or less, and about 39 percent were eight quarters or less.

V. The Impact of Early Labor Market Experiences on Adult Labor Market Outcomes

Wages

Next, we estimate the relationship between early labor market experiences and wages and other characteristics of jobs held in or near 1992, after controlling for the usual human capital and other control variables.[11] We first estimate standard log wage regressions separately by sex, with and without including tenure and its square, but adding in the measures of early labor market experiences. Table 3 reports results from a variety of wage regressions estimated for men.[12] Our base specification includes years of education, actual experience and its square, the number of years since the five-year post-schooling period, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, and regions. All of these variables are defined as of the year of the adult wage observation.[13]

Columns (1)-(5) report the estimated coefficients of sets of variables corresponding to various dimensions of youth labor market experiences, when these sets of variables (sometimes only one variable) are added to the base specification one at a time, with the regression estimated separately each time. Thus, for example, the top two entries in column (1) are the estimated coefficients of the numbers of each type of training spell when only the training spell variables are added to the base specification, while row 3 reports the estimated coefficient on spells of community college, when only that variable is added. Finally, the last rows of the table indicate other variations in the specification, which will be explained below. Columns (6)-(9) report results including the sets of variables measuring the early labor market experiences jointly. Only columns (8) and (9) include tenure and its square as of the year of the adult observation. We were concerned that current tenure would also pick up some of the effects of early labor market experiences, and therefore wanted to look at specifications that do not over control by including tenure. On the other hand, we also want to know whether early labor market experiences matter, once we account for "adult" tenure. Table 4 reports estimates of the same specifications for women.

Considering each set of variables independently is useful because it permits us to identify the "gross" effect of a particular set of early labor market experiences. For example, these results consistently suggest that training increases wages. But if we simultaneously control for training and early stability, and find that stability does not matter, one might object that stability matters because it leads to more training. We can test this by looking at regressions in which we include the stability variables only, because in such regressions the effects on training should "load" onto the stability variables. Conversely, we must be cautious about drawing causal inferences from these specifications. For example, if accumulating tenure in the early years causes one to stay in the same industry, then the estimated effect of staying in the same industry may not imply any direct causal effect of doing so. We first consider the estimated impact of training in the early labor market years; we use the discussion of the training results to explain the other variations in the specification, and then proceed more quickly in discussing the results for the measures of early labor market experiences more directly related to job market stability.

For men, as reported in column (1), the estimated coefficient of spells of training is positive for each type of training, although significant only for off-the-job training, which is estimated to boost wages by seven percent. This differs from some of the literature surveyed earlier, in which on-the-job training is associated with higher wages; the difference may arise because the later dates at which we measure wages imply that many individuals have changed jobs, thus losing any firm-specific human capital, or it may reflect the finding from Lillard and Tan (1986) that the effects of on-the-job training diminish over time. Below these estimates, we look instead at accumulated full-time weeks of training, converted to years for the wage regressions, to facilitate comparisons to, for example, returns to years of schooling. Consistent with the effects of spells of training, the estimated effect of years of off-the-job training (but not on-the-job training) is significant, suggesting that a year of such training raises wages by about seven percent.

Part of the reason we find positive effects of earlier spells of training may be not because training actually increases wages, but because higher-wage individuals get training. We approach this heterogeneity question a couple of ways. In column (2), we try to control for omitted ability by including the score from the Armed Forces Qualifying Test (AFQT), based on the Armed Service Vocational Battery Test, given in 1980 to NLSY respondents. To standardize by age, we regressed the AFQT score on single-year age dummy variables, and use the residuals here. In addition, to attempt to capture more heterogeneity, we include the highest grade completed of the mother and the father.[14] When the AFQT test score and family background variables are added to the equation, the estimated coefficient (not reported) of the test score is positive and statistically significant in all specifications, for both men and women.[15] In addition, the estimated effects of the training variables become a bit smaller, with the estimated coefficient for years of training becoming only marginally significant.[16]

Next, we try to control for individual heterogeneity more completely by including the log of the first wage in the five-year post-schooling period in column (3), and then (alternatively) the log of the last wage in column (4).[17] The estimated coefficients of these early wage variables (not reported) were always positive and significant at the five-percent or ten-percent level. However, the inclusion of these early wages has little effect on the estimated coefficients of the training variables, with the estimated effect of years of off-the-job training actually rising.

In column (5), we return to the problem discussed in the Introduction of separating the effect of early labor market experiences (training, job tenure, etc.) from the effect of quality of the job match. If training is associated with good job matches (which are in turn associated with higher wages), then we would not want to interpret the estimates in columns (1)-(4) as measuring the causal effects of training. (The same will be true, in an even more obvious way, when we try to estimate the effects of job attachment during the early years in the labor market.) To solve this problem, we estimate the same regression only for those individuals who are not with the same employer for whom they worked at the end of the five-year post-schooling period. This may lead to understatement of the effects of some early labor market experiences, because we eliminate not just the effect of the quality of the job match, but also the effect of any returns to early labor market experiences (in this case training) that are specific to the employer. The estimated coefficients for off-the-job training spells are essentially unchanged, suggesting that, as expected, this training is largely general.

Next, in columns (6)-(8) we report results for specifications in which we include all of the early labor market variables simultaneously. We estimate the basic specification, then one including AFQT and parents' education as well as the last wage in the five-year post-schooling period (paralleling column (4)), and finally a specification also including tenure and its square. In these specifications the effects of off-the-job training on men's wages are reduced and no longer statistically significant, although the magnitude of the estimated coefficient for spells of training is unchanged.

Finally, column (9) reports results for the subsample of those whose highest grade completed is 12 or less, for the same specification in column (8), to examine whether these individuals benefit relatively more from early training (and, as discussed below, early labor market stability). In this subsample, the returns to years of off-the-job training are higher.[18]

Table 4 reports estimates of the same specifications for women. The differences between the training results for men and women can be summarized simply. For women, in contrast to men, on-the-job training has strong positive effects on wages in most specifications, on the order of a nine- to ten-percent increase in wages for one or more spells of such training, and similarly large effects using the length of training variables. The latter effects remain large and marginally significant in the specifications in columns (6)-(8) including all of the other early labor market variables. The magnitude of the estimated effects of length of on-the-job training may seem implausibly large, but most training programs are much shorter than a year, so most women would capture far less than the estimated 12-18 percent gains for a one year increase that are implied by the estimates. Curiously, the effects of on-the-job training persist in column (5) when the sample is restricted to those not on the same job as at the end of the five-year post-schooling period, suggesting that this training is general. Finally, there is much less evidence of a positive effect of on-the-job training for those with a high school education or less.

We next turn to the other early labor market variables, most of which are related to churning or stability. The estimates in row 3 are from specifications that include the number of spells of community college, which may be regarded as an alternative type of training. Because the wage regressions also include years of schooling, these specifications estimate the returns to community college above and beyond the usual returns to schooling. For men, in Table 3, the estimated effect of community college is always negative and small, but never significant, whether or not the other early labor market variables are included. For women, in Table 4, the estimated effect is positive, in the two to five percent range, and also insignificant. Thus, there is no statistical evidence that community college is a particularly effective method of boosting adult wages (relative to other schooling). These results are robust across all of the specifications.

In row 4 of Tables 3 and 4, we report specifications including longest tenure attained during the five-year post-schooling period. For men, the estimated effect is small, generally negative, and always insignificant. For women the effects are somewhat larger and sometimes significant or marginally so. However, in the specifications excluding the other early labor market variables, the estimated effects are negative, in contrast to early stability boosting later wages. These results suggest that greater attachment to a job in the early years has no independent beneficial effect on adult wages. In row 5 we instead look at general attachment to the labor market by including a control for months of experience accumulated in the immediate post-schooling period. For both men and women, more experience accumulated in this period has if anything negative effects on later wages. Because the wage regression includes adult experience controls and time elapsed since the five-year post-schooling period, the negative effect does not indicate an overall negative return to experience in the early years. Instead, it indicates that, conditional on the level of adult experience, there are negative returns to concentrating experience in the early years, perhaps owing to human capital depreciation.[19]

We next attempt to estimate the effects of job instability or churning in a somewhat different manner, by looking in row 6 at wage differences based on whether a respondent continues to work in the same one-digit industry or occupation as their first job.[20] For men, the results in Table 3 suggest that the effects of remaining in the same industry or occupation are if anything negative. The industry effects are sometimes as large as six or seven percent, and these larger estimates are marginally significant. These findings may reflect young workers tending to begin employment in lower-wage industries and occupations, so that those left in these industries and occupations eventually earn less. These results suggest that reducing "milling about" in the labor market by locking young workers into the industries and occupations in which they first find employment might be the worst of all possible worlds. For women, in contrast, neither variable has a significant or sizable effect on wages, in any of the specifications. We also look at the first industry not from the perspective of mobility, but from the perspective of whether the individual begins working in a lower-paying industry, specifically whether the first job is in a service-producing industry. For both men and women, the estimates in row 7 of the two tables indicate no wage penalty associated with starting in such industries, whether or not the other early labor market variables are included.

Finally, we look at the consequences for adult wages of early job stability or churning measured by the number of employers (row 8) or number of one-digit industries or occupations of employment (row 9) in the five-year post-schooling period. For women, whether or not we control for tenure, include the other early labor market variables, or control for individual heterogeneity, the estimated effects of number of jobs, occupations, or industries are close to zero and insignificant. For men, each additional employer is associated with wages that are lower by two to three percent. These estimates are significant only in the specifications excluding the other early labor market variables and tenure, and including observations on those who might be on the same job as at the end of the five-year post-schooling period (i.e., columns (1)-(4)).[21] Like for women, the estimated effects of number of industries or occupations are small and insignificant.[22] Thus, again the combined results imply little or no effect of job churning conditional on job tenure as an adult, and provide little evidence of positive returns to early job market stability per se.

To summarize, echoing existing research on training, we find that training in the immediate post-schooling period boosts adult wages for both men and women. But the direct "churning" measures--number of employers, number of industries, whether one changed industries, etc.--are not negatively related to adult wages. As a summary measure of the importance of early labor market experiences, for the specifications including the full set of early labor market variables we report p-values for the joint significance of the estimated coefficients of these variables. Even when training is included among this set of variables, the set of estimates is not significant in most of the specifications. When the training variables are excluded from this set of variables, the lack of consequence of early churning and instability is even more apparent, as the p-values rise and are near or above .2 for the specifications for both men and women. Similarly, we report the increment to the adjusted R-squared from including the early labor market variables. The small increments show that the entire set of variables explains very little of the variation in adult wages (even including training, which provides most of the additional explanatory power).

Sibling Estimates

An alternative approach to eliminating bias from individual-specific (as opposed to job-match-specific) heterogeneity is to assume that the unobservable characteristics that are correlated with other regressors are common to siblings within the same family. For example, an unobserved family "work ethic" may make siblings from one family more likely to accumulate early labor market experience than siblings from other families, and may also increase labor market success independently of early experience, thus biasing upward the cross-section estimate of the effect of early experience on adult wages. If this "work ethic" is purely a family effect, differencing across siblings can remove this source of bias. Relying on the assumption that only the family-specific unobservable is correlated with the regressors, we match the NLSY household identification codes to create a sibling sub-sample, and control for family effects by differencing the wage equation across same-sex siblings.[23]

To obtain a larger sample, we shortened the five-year post-schooling window to a three-year window for this analysis only.[24] The matched sibling sample for the three-year post-schooling period includes 337 males in the sample of brothers, and 290 females in the sample of sisters (there can be more than two siblings per family). Table 5 presents the OLS and fixed family effects estimates for the early labor market variables included jointly, for men and women. Given the much smaller sibling sample, any conclusions about the effects of differencing should be drawn from comparisons of the within-family estimates with the cross-section estimates for the same sample. Because the cross-section estimates for this sample differ in some ways from the full-sample estimates, the sibling data teach us more about the potential biases from omitted variables common to siblings than about unbiased estimates for the population as a whole. Tenure controls are included in columns (3), (4), (7), and (8). Given the small samples, we restrict the training variables to be simply the total number of spells or cumulative length of training spells.

The estimated coefficients of the training variables are positive but insignificant for men, and rise somewhat in the within-family estimates. For women, the OLS estimates of the effect of years of training are also positive and large, although insignificant, but are unchanged in the within-family estimates. Turning to the other early labor market variables, there is some evidence consistent with heterogeneity bias leading to overstatement of the negative consequences of churning in the early years in the labor market, or conversely overstatement of the positive effects of early stability. For example, for men the within-family estimates of the effect of the number of occupations are less negative than the cross-section estimates, and the within-family estimates of the effect of staying in the same occupation as the first job are more negative. For women the within-family estimates of the effect of number of industries are also less negative than the cross-section estimates, while the within-family estimates of the effect of staying in the same industry are more negative. On the other hand, there is evidence in the opposite direction, including that for the within-family estimates of the effects of staying in the same industry as the first job and the number of employers for men, and staying in the same occupation for women.

Thus, on balance, we do not find convincing evidence from the sibling data that cross-section estimates either overstate (as we would expect) or understate the positive effects of early job market stability or training. We emphasize, though, that our within-family estimates of the returns to training and other early labor market experiences are the first of which we are aware (in contrast to the extensive literature on sibling estimates of the return to schooling), and we think that further research along these lines is needed to clarify the contribution of such evidence.

Benefits

We now turn to other adult labor market outcomes. First, we study employee benefits, focusing on the two that are probably of greatest value and most strongly associated with "good" jobs: health insurance and pension plans. In the next subsection, we examine the consequences of early labor market experiences for the probability of holding a full-time job as an adult. Given the relatively small impacts of controlling for individual or match-specific heterogeneity in the wage equations, we focus only on the basic specifications for the full sample.

In Table 6, we report estimated effects on the probability of provision of health insurance and retirement plans, based on probit estimates. We report results using the control variables from the earlier wage equations, both with and without tenure controls, and including the early labor market variables jointly. The number of training spells is positively related to the provision of both of these benefits for men and women, with the estimated effects as high as .11, although the estimates are not significant. The estimated effects of length of training are more disparate and sometimes negative, but also never significant. For men the number of community college spells in the early years is significantly positively associated with health insurance.

Turning to the churning measures, for men there is mixed evidence regarding the effects of early stability on receipt of benefits. In the specification excluding current tenure, longest job tenure in the early years is significantly positively related to receipt of health benefits, while on the other hand staying in the same occupation as the first job is negatively related to the receipt of both types of benefits, although not significantly. For women, there is more consistent evidence pointing to beneficial effects of early labor market stability. In particular, remaining in the same occupation has a positive effect on the probability of receiving health insurance that is significant in the specification excluding current tenure, with the probability boosted by .06, while the number of employers is negatively related to the probability of receiving these benefits in some specifications, and significant in the case of retirement plans. On the other hand, the estimated effects of staying in the same industry are negative, and sometimes marginally significant.[25] As for the wage equations, the estimated effects of the early labor market variables are jointly significant in some of the specifications, but rarely so when we focus on the early labor market variables other than training. Overall, we regard the evidence in Table 6 as providing at best weak evidence that for women but not for men, churning is costly in terms of benefits on adult jobs.

Full-Time Employment

In addition to looking at benefits, another potentially important dimension of labor market careers is full-time work. For example, Blau and Kahn (1996) document that holding a full-time job for less-educated workers, especially women, is sufficiently rare to believe that it partly stems from difficulties in finding and holding on to such jobs.

Estimates of specifications paralleling those for benefits are reported in Table 7; the dependent variable is defined as one for those working full-time (35 or more hours per week), and zero for those working less or not working at all. For men, we find little or no evidence of effects of early labor experiences other than training on the probability of full-time employment as an adult. For women some of the estimated effects are larger, but they do not point unambiguously in the direction of early stability increasing the likelihood of full-time work as an adult. For example, although generally not significant, the estimated effects of remaining in the same industry or occupation as the first job are positive, but so are the estimated effects of number of jobs, industries, and occupations. Overall, the results for full-time job holding parallel many of our earlier results in showing little if any consistent evidence of positive effects of early labor market stability.

VI. Conclusions

The goal of this paper is to shed light on the consequences of initial periods of "churning," "floundering about," or "mobility" in the labor market, and to help assess whether faster transitions to stable employment relationships--such as those envisioned by some school-to-work programs--would be likely to lead to better labor market outcomes. We find that adult labor market outcomes (defined as of the late 20s or early to mid-30s) are for the most part unrelated to the stability of early labor market experiences, especially for men, although training in the early years has lasting benefits. Some of the results for women may provide weak evidence that, for them, early job market stability has some beneficial effects. This difference between the sexes may arise because women need to signal their attachment to the labor market when they are young in order to acquire jobs with high wages and generous benefits as adults. However, this is another reason to interpret any positive effects of early job stability for women as probably reflecting upper-bound estimates of the causal effects.

In our view, the evidence does not provide a compelling case for efforts to explicitly target the school-to-work transition, insofar as this implies changing the structure of youth labor markets so that young persons become more firmly attached to employers, industries, or occupations, at younger ages. Of course, some components of school-to-work programs may still be useful. For example, there may be information problems in the labor market, so that improving young persons' information about labor market opportunities would be helpful. Such efforts might include the incorporation of work experience into schooling, or greater communication and interaction between employers and schools. Furthermore, the relatively consistent evidence we find that early training helps workers might rationalize increased training for young workers.

Finally, our empirical analysis examines the effects of early labor market experiences in the jobs in which young workers presently tend to enter the labor market. Our results would not be generalizable to school-to-work programs that substantially alter the types of jobs held by new labor market entrants. At this point, though, we have uncovered relatively little evidence supporting youth labor market policies that create more "order" in the transitions of youths to work.

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Footnotes

1. See also 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) and Stern, et al. (1994).

2. A period of only two or three years seemed too short, given evidence that youths often spend their first year or two in the labor market in pursuits that are not strongly related to their careers, and only then start to make labor market decisions with career goals in mind (Osterman 1980).

3. The results were not sensitive to the alternative of dating entry into the labor market following the last observed enrollment of any type, including community college.

4. Because of the limited information available in the NLSY on years prior to 1979, we can only date labor market entry accurately for those who were in school in 1978 (which is reported).

5. In this sample, the (weighted) average age is 20.6 as of the first post-schooling observation.

6. The (weighted) average age in the sample used for the adult wage analysis is 31.9.

7. We adjust the NLSY sample weights to reflect differences in the probability of remaining in the sample given our rules for selection into the sample, based on race, sex, and education. All descriptive statistics are based on weighted data. Appendix Table A1 provides descriptive statistics for the variables not covered in Table 2.

8. From 1979 to 1986, respondents are questioned regarding three spells of training which lasted at least one month; the one-month restriction leads us to underestimate training relative to other data sources. For each training spell, information is elicited on the starting date, the ending date, hours per week attended, weeks per training spell, and type of program. Because training data were not collected in 1987, no ending date information is given for the (up to) three training spells reported in progress as of the 1986 interview. Consequently, the 1987 interview date is used as the date of completion for programs that had not ended by that interview.

9. On-the-job training undoubtedly understates informal training (see Loewenstein and Spletzer

1994). Off-the-job training is training obtained at barber or beauty schools, business schools or colleges, vocational or technical institutes, nurses' programs, flight schools, or through correspondence courses. Information on apprenticeship training is also available. We include corresponding controls for apprenticeship training in all of the equations we estimate, but given the extremely low frequency of reported apprenticeship training (less than one percent of the sample), we do not report results for this type of training.

10. The variables measuring length of training are used to create a total hours of training variable, which is then divided by 40 to obtain weeks of training based on a 40-hour week.

11. To increase the sample, if a wage (or benefit measure) was not available in 1992, we tried to obtain an observation from 1991, and then from 1990. About 75 percent of the observations came from 1992. A disproportionate share of the rest came from 1990, because the poor white oversample was eliminated in 1991. To control for unobserved differences among the regular and oversampled individuals, year dummy variables are included in all specifications. For the analysis of full-time job holding, we used observations from 1990 or 1991 only if the person dropped out of the sample in subsequent years.

12. We round reported coefficients to two digits, unless more are required to accurately assess whether an estimated coefficient is statistically significant at the five- or ten-percent level.

13. Because returns to early labor market experiences may be realized partly through the industry, occupation, and union status of adult jobs, we do not control for these variables.

14. We include a dummy variable indicating missing data on parents' education, and set the parents' education variable to zero when data are missing.

15. Because the wage regressions effectively use residuals from a first-stage test score regression, our standard errors are potentially understated. However, we cannot implement the simple Murphy and Topel (1985) procedure for correcting the standard errors, or use joint estimation to get efficient estimates and the right standard errors, because we estimate the test score equation

on the full sample of individuals (about 10,000) for whom test scores are available, whereas the wage equations are estimated for a much smaller sample. We use this larger sample for the test scores to remove the mean age effects, rather than the age effects that might arise in our particular sample, which could reflect schooling effects that are partly endogenous. Any bias in the standard errors is likely to be inconsequential because: 1) the test score regression is run on a huge sample, with few variables all of which are very precisely estimated, 2) the bias in the standard errors is largely in the test score coefficients (which are not our main concern), and 3) we are not reporting many significant results anyway, so that any downward bias in the standard errors does not work in favor of our conclusions.

16. This parallels other research indicating that the cross-sectional association between wages and training is largely not attributable to heterogeneity (e.g., Holzer 1990; Mincer 1991).

17. The last wage in the post-schooling period may provide a better test than the first wage, because the last wage is measured closer to the overtaking age, and hence should be less influenced by training financed out of wages.

18. We do not report results for the subsample with more than 12 years of schooling, because of the small sample sizes.

19. The set of coefficient estimates always implied positive overall returns to experience.

20. The NLSY provides information on up to five jobs per year for each respondent, one of which is also identified as the "current or most recent job." The detailed information for the (up to) four additional jobs per year only covers those jobs in which the respondent worked more than 20 hours per week and for at least nine consecutive weeks. As a result, this information may give a non-representative sample of the types of jobs held in the early years in the labor market. The current or most recent job, in contrast, is not restricted to such jobs, and as a result provides a random sample of the jobs held, although at the cost of obtaining a less complete work history. Consequently, we use information on the current or most recent job held in each year to

obtain data on characteristics of the jobs held in the five-year post-schooling period.

21. The NLSY does have a created variable measuring number of jobs ever reported, although as noted in the previous footnote, the other detailed information on these jobs is only reported under some conditions. To examine the sensitivity of our results for the number of employers or jobs, we reestimated the specifications using this alternative variable instead. The mean number of jobs using this variable was 5.66 for men, and 5.25 for women for the adult wage sample, roughly double the mean number (of employers) using the other measure. However, the regression results were unaffected, with even weaker effects of number of employers. For example, in specifications corresponding to column (1) of Tables 3 and 4, the estimated coefficients (standard errors) were -.003 (.004) for men, and .002 (.005) for women. For specifications corresponding to column (8), the estimates were -.001 (.006) and .007 (.006).

22. Appendix Table A1 shows that the mean numbers of industries and occupations slightly exceed the mean number of employers for men. The larger discrepancy occurs for occupation, although this result is not too surprising since individuals can change occupations at the same employer. The much smaller discrepancy for industry may reflect measurement error in the industry classification or in the tenure variable used to identify changes of employers.

23. Using sibling differences to control for unobserved heterogeneity has a potential advantage relative to using early wages. In particular, some of the early labor market experiences may affect the early wage and the adult wage roughly equally. In this case, we might conclude that early labor experiences do not affect adult wages, when in fact all we are finding is that they do not affect wage growth. (That is, including the early wage on the right-hand side is similar to specifying a wage growth regression.) In the sibling differences, this is not a problem.

24. The wage regression estimates for the three-year post-schooling unmatched sample were similar to those for the five-year unmatched sample, so we do not report them.

25. For both health and pension benefits, the results were similar when we included the early

labor market variables individually in separate estimations, and when we looked only at those with a high school education or less. They were a bit weaker if we included part-time and unincorporated self-employed workers (for whom benefits data are not reported), coding them as not receiving these benefits.

Table 1: NLSY Sample Construction

Early Labor Market Analysis	Number of Observations Remaining	Average Education of of Remaining Group	Employment Rate in 1992 of Remaining Group
Full sample	12686	...	...
Delete military	10716	...	...
Delete non-interviews 1979-1986	9121	12.94	.864
Delete observations missing enrollment data to define five-year post-schooling period	9083	12.95	.864
Define subsample with five-year post-schooling period:			
Delete observations in school all years 1979-1986	8625	12.74	.858
Delete observations first left school 1979-1982, but returned after 1982	7228	12.50	.854
Delete observations first left school after 1982, or "permanently" left school before 1979	3261	12.02	.836
Delete observations with inconsistent or missing training data in five-year post-schooling period	3200	...	...
Delete observations missing data on early tenure, experience, wages, etc., in five-year post-schooling period	3031	...	...
Delete observations not observed with survey-week job at any time during five-year post-schooling period	2844	12.08	.861
Adult Wage Analysis			
Delete observations with no wage 1990-1992	2397	...	...
Delete observations with missing wage regression controls	2392	...	...

Table 2: Descriptive Statistics for Selected Early Labor Market Experience Variables, Overall and By Demographic Group[a]

	Fraction with Community College (1)	Fraction with Any Training (2)	Fraction with On-the-Job Training (3)	Fraction with Off-the-Job Training (4)	Months of Labor Market Experience (5)	Weeks of Tenure, Longest Job (6)	Number of Observations (7)
Male	.14	.27	.08	.20	52.92	135.78	1372
	(.35)	(.45)	(.27)	(.40)	(14.49)	(62.54)	
Female	.16	.27	.08	.20	46.31	121.63	1472
	(.37)	(.44)	(.27)	(.40)	(18.19)	(63.04)	
White	.15	.27	.08	.20	51.13	132.53	1676
	(.36)	(.45)	(.28)	(.40)	(15.70)	(62.29)	
Black	.15	.25	.06	.20	40.69	106.30	689
	(.35)	(.44)	(.24)	(.40)	(19.77)	(62.17)	
Non-black Hispanic	.19	.25	.05	.19	44.54	116.21	479
	(.39)	(.43)	(.22)	(.39)	(19.04)	(66.94)	
≤H.S. Education	.07	.26	.05	.22	44.43	108.17	2180
	(.25)	(.44)	(.22)	(.42)	(18.75)	(61.20)	
Overall Mean	.15	.27	.08	.20	49.52	128.50	2844
	(.36)	(.44)	(.27)	(.40)	(16.82)	(63.19)	

a. Standard deviations are reported in parentheses. Estimates are based on weighted data.

Table 3: OLS Estimates of Effects of Early Labor Market Experiences on Adult Wages, Controlling for Adult Characteristics, Males[a]

	Early Variables Entered Individually					Early Variables Entered Jointly			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. # of training spells by type:									
# on-the-job	.03	.02	.00	.00	.01	.03	.02	.02	.03
	(.05)	(.05)	(.05)	(.05)	(.06)	(.06)	(.07)	(.07)	(.08)
# off-the-job	.07	.05	.05	.05	.06	.049	.05	.05	-.02
	(.02)	(.02)	(.02)	(.02)	(.03)	(.034)	(.04)	(.04)	(.04)
2. Years of training by type:									
Years on-the-job	.02	.01	-.00	.01	.01	.01	-.00	-.00	.07
	(.05)	(.05)	(.05)	(.05)	(.05)	(.06)	(.07)	(.07)	(.07)
Years off-the-job	.07	.05	.07	.06	.06	.03	.02	.02	.11
	(.03)	(.03)	(.03)	(.03)	(.03)	(.04)	(.04)	(.04)	(.04)
3. Community college spells	-.02	-.03	-.02	-.02	-.03	-.03	-.04	-.04	-.04
	(.03)	(.03)	(.03)	(.03)	(.04)	(.03)	(.04)	(.04)	(.05)
4. Longest job tenure (years)	-.00	.00	-.00	-.01	-.01	-.03	-.02	-.03	-.04
	(.02)	(.01)	(.01)	(.01)	(.02)	(.02)	(.03)	(.03)	(.03)
5. Months of experience	-.00	-.00	-.00	-.004	-.0028	.00	-.00	.00	-.00
	(.002)	(.002)	(.002)	(.002)	(.0017)	(.002)	(.002)	(.002)	(.002)
6. Same ind./occ. as first job:									
Same industry	-.00	-.01	-.02	-.02	-.05	-.02	-.06	-.06	-.07
	(.03)	(.03)	(.03)	(.03)	(.04)	(.03)	(.04)	(.04)	(.04)
Same occupation	-.04	-.03	-.04	-.04	-.03	-.04	-.03	-.03	-.03
	(.05)	(.03)	(.03)	(.03)	(.04)	(.03)	(.04)	(.04)	(.04)
7. First job in service-producing industry	-.03	-.03	.01	.01	.00	-.03	-.00	-.00	-.04
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
8. Number of employers	-.02	-.03	-.02	-.02	-.016	-.033	-.02	-.02	-.02
	(.01)	(.01)	(.01)	(.01)	(.012)	(.017)	(.02)	(.02)	(.02)
9. Number of ind./occ.:									
Number of industries	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.02
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)
Number of occupations	-.01	-.016	-.01	-.01	-.01	-.01	-.01	.01	.00
	(.01)	(.012)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)
P-value for early labor market variables (F-test)	...	...	...	...	...	.05	.36	.43	.001
P-value for non-training early labor market variables (F-test)	...	...	...	...	...	.21	.33	.45	.18
Increment to adjusted R^2 from early labor market variables	...	...	...	...	...	.01	.00	.00	.02
AFQT and parents' education included	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log first wage included	No	No	Yes	No	No	No	No	No	No
Log last wage included	No	No	No	Yes	Yes	No	Yes	Yes	Yes
Tenure and its square included	No	No	No	No	No	No	No	Yes	Yes
Obs. not on same job	No	No	No	No	Yes	No	Yes	Yes	Yes
Sample by schooling	All	All	All	All	All	All	All	All	≤ H.S educ.
N	1227	1227	1227	1227	951	1227	951	951	794

a. Dependent variable is log hourly wage, deflated to constant dollars using the PCE deflator. Standard errors of coefficient estimates are reported in parentheses. In addition to the early labor market experience variables, the specifications all include the following variables defined for 1992 (or 1990 or 1991): years of education, actual experience and its square, years lapsed between the end of the post-schooling period and the current observation, corresponding variables for apprenticeship training, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, regions, and the year of the observation. In columns (1)-(5) each set of coefficients in a numbered row is estimated from a separate wage regression.

Table 4: OLS Estimates of Effects of Early Labor Market Experiences on Adult Wages, Controlling for Adult Characteristics, Females[a]

	Early Variables Entered Individually					Early Variables Entered Jointly			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1. # of training spells by type:									
# on-the-job	.10	.09	.09	.09	.10	.065	.06	.07	.08
	(.04)	(.04)	(.04)	(.04)	(.05)	(.044)	(.06)	(.06)	(.07)
# off-the-job	-.02	-.02	-.02	-.02	-.02	-.00	.00	.01	-.01
	(.02)	(.02)	(.02)	(.02)	(.02)	(.03)	(.03)	(.03)	(.04)
2. Years of training by type:									
Years on-the-job	.16	.16	.15	.14	.18	.12	.14	.14	-.02
	(.06)	(.06)	(.06)	(.06)	(.08)	(.07)	(.09)	(.09)	(.15)
Years off-the-job	-.03	-.03	-.03	-.02	-.03	-.03	-.05	-.05	.03
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.04)	(.04)	(.05)
3. Community college spells	.03	.02	.02	.02	.04	.03	.03	.02	.05
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.04)
4. Longest job tenure (years)	-.01	-.01	-.017	-.03	-.028	.01	.01	.01	.01
	(.01)	(.01)	(.014)	(.01)	(.017)	(.02)	(.03)	(.03)	(.03)
5. Months of experience	-.002	-.002	-.003	-.004	-.003	-.0032	-.004	-.0034	-.00
	(.001)	(.001)	(.001)	(.001)	(.001)	(.0017)	(.002)	(.0019)	(.002)
6. Same ind./occ. as first job:									
Same industry	.02	.02	.01	.01	.02	.02	.01	.01	-.03
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Same occupation	.00	.00	-.00	-.01	-.01	.00	-.00	-.00	-.01
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
7. First job in service-producing industry	-.01	-.01	-.00	-.01	.00	-.01	-.01	-.00	.00
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
8. Number of employers	-.00	-.00	.00	.00	.01	.00	.02	.02	.02
	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)	(.02)
9. Number of ind./occ.:									
Number of industries	.01	.01	.01	.01	.01	.01	.01	.01	-.00
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)
Number of occupations	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.02
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)
P-value for early labor market variables (F-test)	...	...	...	...	...	.23	.14	.22	.94
P-value for non-training early labor market variables (F-test)	...	...	...	...	...	.56	.26	.52	.89
Increment to adjusted R^2 from early labor market variables	...	...	...	...	...	.00	.00	.00	-.01
AFQT and parents' education included	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log first wage included	No	No	Yes	No	No	No	No	No	No
Log last wage included	No	No	No	Yes	Yes	No	Yes	Yes	Yes
Tenure and its square included	No	No	No	No	No	No	No	Yes	Yes
Obs. not on same job	No	No	No	No	Yes	No	Yes	Yes	Yes
Sample by schooling	All	All	All	All	All	All	All	All	≤ H.S. educ.
N	1165	1165	1165	1165	933	1165	933	933	719

a. Dependent variable is log hourly wage, deflated to constant dollars using the PCE deflator. Standard errors of coefficient estimates are reported in parentheses. In addition to the early labor market experience variables, the specifications all include the following variables defined for 1992 (or 1990 or 1991): years of education, actual experience and its square, years lapsed between the end of the post-schooling period and the current observation, corresponding variables for apprenticeship training, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, regions, and the year of the observation. In columns (1)-(5) each set of coefficients in a numbered row is estimated from a separate wage regression.

Table 5: Estimates of Effects of Early Labor Market Experiences on Adult Wages, Controlling for Adult Characteristics, Sibling Samples[a]

	Males				Females			
	OLS	FE	OLS	FE	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total # of training spells	.02	.05	.03	.07	.02	-.04	.01	-.04
	(.08)	(.10)	(.08)	(.10)	(.07)	(.10)	(.07)	(.09)
Years of training	.02	.12	.01	.06	.09	.09	.08	.09
	(.11)	(.16)	(.10)	(.16)	(.07)	(.09)	(.06)	(.08)
Community college spells	.08	.06	.09	.07	.04	.07	.05	.07
	(.07)	(.09)	(.07)	(.09)	(.07)	(.09)	(.06)	(.09)
Longest job tenure (years)	.03	.02	.03	.03	.00	.01	-.01	.02
	(.05)	(.06)	(.05)	(.06)	(.06)	(.09)	(.06)	(.08)
Months of experience	-.00	-.01	-.00	-.01	.00	-.00	.00	-.00
	(.005)	(.01)	(.005)	(.01)	(.01)	(.01)	(.01)	(.01)
Same industry as first job	-.03	.05	-.05	.03	.04	-.03	-.01	-.07
	(.06)	(.07)	(.06)	(.07)	(.06)	(.08)	(.06)	(.08)
Same occupation as first job	-.05	-.09	-.07	-.11	.10	.13	.07	.11
	(.06)	(.06)	(.06)	(.06)	(.06)	(.08)	(.06)	(.08)
First job in service-producing industry	-.00	-.04	-.01	-.03	-.11	-.09	-.10	-.08
	(.05)	(.07)	(.05)	(.07)	(.07)	(.09)	(.07)	(.09)
Number of employers	-.04	-.06	-.03	-.05	.00	-.01	.01	.02
	(.04)	(.05)	(.04)	(.05)	(.05)	(.06)	(.05)	(.06)
Number of industries	-.02	-.03	-.01	-.02	-.03	-.01	-.03	-.00
	(.06)	(.07)	(.06)	(.07)	(.07)	(.09)	(.07)	(.09)
Number of occupations	-.07	-.04	-.08	-.06	-.02	-.05	-.03	-.07
	(.06)	(.07)	(.06)	(.07)	(.08)	(.09)	(.07)	(.09)
Tenure and its square included	No	No	Yes	Yes	No	No	Yes	Yes
P-value for early labor market variables (F-test)	.68	.46	.59	.54	.58	.86	.73	.81
P-value for non-training early labor market variables (F-test)	.52	.51	.44	.52	.62	.79	.77	.76
N	337	337	337	337	290	290	290	290

a. Dependent variable is log hourly wage, deflated to constant dollars using the PCE deflator. Standard errors of coefficient estimates are reported in parentheses. In addition to the early labor market experience variables, the specifications all include the following variables defined for 1992 (or 1990 or 1991): years of education, actual experience and its square, years lapsed between the end of the post-schooling period and the current observation, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, regions, and the year of the observation. The sibling sample is constructed using a three-year window for the post-schooling period. The OLS specifications in columns (1) and (5) are identical to those in column (6) of Tables 3 and 4. In every column, the coefficients of the early labor market experience variables are estimated jointly.

Table 6: Estimates of Effects of Early Labor Market Experiences on Probability of Working at Jobs that Provide Health Insurance and Retirement Plans, Controlling for Adult Characteristics[a]

	Males				Females			
	Health Insurance		Retirement Plan		Health Insurance		Retirement Plan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# of training spells by type:								
# on-the-job	.05	.04	.11	.11	.07	.08	.07	.07
	(.08)	(.08)	(.09)	(.09)	(.06)	(.06)	(.07)	(.07)
# off-the-job	.05	.05	.05	.05	.04	.05	.01	.02
	(.04)	(.04)	(.05)	(.05)	(.04)	(.04)	(.04)	(.04)
Years of training by type:								
Years on-the-job	-.07	-.06	-.10	-.09	-.01	-.02	.08	.07
	(.06)	(.06)	(.08)	(.08)	(.10)	(.10)	(.12)	(.12)
Years off-the-job	.01	.02	-.00	.01	-.06	-.07	-.01	-.01
	(.05)	(.05)	(.06)	(.06)	(.05)	(.05)	(.06)	(.06)
Community college spells	.09	.08	.00	-.01	.02	.01	.04	.04
	(.04)	(.04)	(.04)	(.04)	(.03)	(.03)	(.04)	(.04)
Longest job tenure (years)	.04	.03	.01	-.01	.02	.01	.03	.02
	(.02)	(.02)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Months of experience	-.01	-.0034	-.01	-.002	-.01	-.004	-.01	-.0048
	(.002)	(.0020)	(.005)	(.003)	(.002)	(.002)	(.002)	(.0025)
Same industry as first job	-.02	-.04	-.00	-.02	-.045	-.053	-.03	-.04
	(.03)	(.03)	(.04)	(.04)	(.034)	(.033)	(.04)	(.04)
Same occupation as first job	-.04	-.047	-.05	-.05	.06	.052	.04	.03
	(.03)	(.033)	(.04)	(.04)	(.03)	(.034)	(.04)	(.04)
First job in service-producing industry	-.04	-.05	-.02	-.03	-.00	-.01	-.06	-.065
	(.03)	(.03)	(.03)	(.03)	(.04)	(.04)	(.04)	(.043)
Number of employers	.00	.01	-.02	-.01	-.02	-.02	-.04	-.04
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Number of industries	-.01	-.01	-.02	-.02	-.00	-.00	.01	.01
	(.01)	(.01)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Number of occupations	-.01	-.01	-.01	-.01	-.00	-.00	.01	.01
	(.01)	(.01)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Tenure and its square included	No	Yes	No	Yes	No	Yes	No	Yes
P-value for early labor market variables (LR-test)	.02	.10	.08	.22	.002	.04	.02	.18
P-value for non-training early labor market variables (LR-test)	.42	.30	.14	.11	.19	.09	.60	.54
N	1081	1081	1067	1067	995	995	967	967

a. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives (in parentheses), are reported. In addition to the early labor market experience variables, the specifications all include the following variables defined for 1992 (or 1990 or 1991): years of education, actual experience and its square, years lapsed between the end of the post-schooling period and the current observation, corresponding variables for apprenticeship training, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, regions, and the year of the observation. This sample excluded the unincorporated self-employed and those working less than 20 hours per week, for whom data on benefits are not reported. In every column, the coefficients of the early labor market experience variables are estimated jointly.

Table 7: Estimates of Effects of Early Labor Market Experiences on Probability of Having a Full-Time Job, Controlling for Adult Characteristics[a]

	Males		Females	
	(1)	(2)	(3)	(4)
# of training spells by type:				
# on-the-job	.07	.06	.09	.10
	(.06)	(.06)	(.06)	(.06)
# off-the-job	.05	.04	.01	.01
	(.02)	(.02)	(.03)	(.03)
Years of training by type:				
Years on-the-job	-.01	-.01	.05	.03
	(.05)	(.05)	(.11)	(.11)
Years off-the-job	-.04	-.04	.00	-.00
	(.02)	(.02)	(.04)	(.04)
Community college spells	-.00	-.01	-.048	-.06
	(.02)	(.02)	(.033)	(.03)
Longest job tenure (years)	-.00	-.01	.044	.03
	(.01)	(.01)	(.026)	(.03)
Months of experience	-.002	-.0016	-.01	-.01
	(.001)	(.0010)	(.002)	(.002)
Same industry as first job	.00	-.00	.04	.05
	(.02)	(.02)	(.04)	(.04)
Same occupation as first job	-.02	-.02	.03	.03
	(.02)	(.02)	(.03)	(.03)
First job in service-producing industry	-.01	-.01	.01	.01
	(.02)	(.01)	(.04)	(.04)
Number of jobs	-.00	-.00	.02	.02
	(.01)	(.01)	(.02)	(.02)
Number of industries	.00	.00	.03	.03
	(.01)	(.01)	(.02)	(.02)
Number of occupations	-.015	-.015	.02	.02
	(.008)	(.007)	(.02)	(.02)
Tenure and its square included	No	Yes	No	Yes
P-value for early labor market variables (LR-test)	.04	.10	.00	.00
P-value for non-training early labor market variables (LR-test)	.23	.24	.52	.52
N	1333	1333	1439	1439

a. Full-time is defined as 35 or more hours of work per week. The sample includes working and non-working individuals. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives (in parentheses), are reported. In addition to the early labor market experience variables, the specifications all include the following variables defined for 1992 (or 1990 or 1991): years of education, actual experience and its square, years lapsed between the end of the post-schooling period and the current observation, corresponding variables for apprenticeship training, and dummy variables for currently married, black, non-black Hispanic, residence in an SMSA, regions, and the year of the observation. In every column, the coefficients of the early labor market experience variables are estimated jointly.

Appendix Table A1: Descriptive Statistics[a]

	Males (1)	Females (2)
Early labor market experience:		
Number of training spells	.35 (.65)	.38 (.74)
On-the-job	.09 (.33)	.12 (.45)
Off-the-job	.25 (.55)	.26 (.61)
Apprenticeship	.02 (.13)	.003 (.06)
Full-time-equivalent years of training	.18 (.52)	.18 (.55)
On-the-job	.05 (.28)	.04 (.30)
Off-the-job	.11 (.38)	.14 (.47)
Apprenticeship	.02 (.21)	.001 (.04)
Number of spells of community college	.17 (.46)	.20 (.49)
Same current industry as first job	.31	.26
Same current occupation as first job	.24	.31
Started in service-producing industry	.56	.81
Number of employers first five years	2.47 (1.25)	2.40 (1.19)
Number of industries first five years	2.51 (1.21)	2.14 (1.06)
Number of occupations first five years	2.82 (1.19)	2.29 (1.09)
Log first wage ($1992)	6.53 (.48)	6.33 (.50)
Log last wage ($1992)	6.78 (.54)	6.49 (.54)

	Males (3)	Female (4)
Adult labor market variables:		
Log wage ($1990)	7.01 (.56)	6.65 (.58)
Job provides health insurance	.77	.75
Job provides retirement plan	.58	.59
Employed full-time	.92	.59
Highest grade completed	13.02 (2.18)	13.17 (1.94)
Actual experience	10.81 (2.19)	9.50 (3.03)
Tenure	4.95 (4.22)	4.28 (4.06)
Currently married	.61	.66
Black	.11	.12
Non-black Hispanic	.07	.05
Resides in SMSA	.77	.78
South	.21	.22
Northeast	.28	.23
North central	.33	.38
Not at same job as at end of five-year post-schooling period	.70	.77
AFQT (standardized)	5.98 (20.09)	8.98 (17.38)
Mother's highest grade completed	11.72 (2.63)	11.50 (2.45)
Father's highest grade completed	12.05 (3.43)	11.93 (3.43)

a. Standard deviations are reported in parentheses. Estimates are based on weighted data. For the early labor market experience variables, the sample has 2,844 observations. For the adult variables, the sample has 2,392 observations (for most variables); the sample in columns (3) and (4) and in the last two rows of columns (1) and (2) is restricted to those working for a wage, except for the "employed full-time" variable. The means for parents' education are computed only for those with valid data. Only the early labor market experience variables not summarized in Table 2 are included here.