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10 Teenage Unemployment: Permanent Scars or Temporary Blemishes?

David T. Ellwood

Teenage unemployment poses a puzzle for economists. Its causes and consequences are not well understood becuase of conflicting economic analyses. The human capital model suggests that since investment should be quite heavy in the early years, teenage unemployment carries with it heavy costs. But search theory suggests that shopping around is a necessary and desirable activity, particularly for those with little information about opportunities in the labor market. There is also concern that early labor force attachment may be weak, raising the possibility that early unemployment may just represent consumption of leisure. This chapter focuses on the longer-term consequences of early spells out of work for male teenagers.

The fundamental problem in capturing the long-term effects of unemployment is separating differences in employment and wages which are causally related to early unemployment, from the differences due to unobserved personal characteristics correlated with early unemployment. Whereas elsewhere in economics researchers routinely assume homogeneity of tastes and preferences, heterogeneity lies at the very heart of the issue here. Separating the individual component is the primary challenge faced in this chapter.

This chapter is divided into three sections. The first simply describes the early labor market experience of the young men in this sample. Strangely, there is little published data tracing the experience of a complete cohort over four years. In most other work the high rates of attrition and reentrance into the sample over the period at least open the possibility of distorting the underlying pattern. The second section extends the

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work of Heckman and Chamberlain to test the long-term effects of early employment on future employment. The final section uses a Sims-type causality model to measure the impact of work experience on wages.

I conclude that the effects of a period without work do not end with that spell. A teenager who spends time out of work in one year will probably spend less time working in the next than he would have had he worked the entire year. Furthermore, the lost work experience will also be reflected in lower wages. At the same time, my data provide no evidence that early unemployment sets off a vicious cycle of recurrent unemployment. The reduced employment effects die off very quickly. What appear to persist are effects of lost work experience on wages.

Scars—In Theory and Practice

It is useful to begin by examining the implications of early unemployment according to several of the more common labor theories. Perhaps most prominent in its prediction of long-term effects is human capital theory. While the theory is not concerned with early unemployment inducing later unemployment, its emphasis on human investment early in the job career to explain the concave pattern of aggregate age-earnings profiles implicitly imposes heavy costs on the unfortunate young person who misses out on early investment opportunities. If no investment takes place during the period without employment, the entire profile is shifted back. Even if retirement is also delayed, the present value of the entire earnings streams must now be discounted over the lost time.

The dual labor market theorists paint an equally bleak picture. Poor work habits develop over the periods of discouragement, catalyzing weak labor force attachment and alienation. The result is a vicious cycle of unemployment followed by deterioration followed by more unemployment. Pervading the institutional literature is the related notion of tracking. Teenagers face only a limited number of entry-level jobs which lead to better jobs. Those who miss good jobs early are permamently tracked onto inferior ladders.

One troubling question is whether early unemployment is largely a result of a job shortage or of weak labor force attachment. Most theories that predict long-term impacts of unemployment emphasize the involuntary nature of early unemployment. If much of it is "voluntary," it still may be reasonable to consider whether there are long-term consequences. Teenage unemployment cannot be strictly voluntary since it is so strongly countercyclical. But it is possible that some portion of the problem is due to weak attachment. Young people may take jobs only when they are readily available. Early experience may quicken labor force attachment and reinforce desirable work skills. If it is considered socially desirable to hasten the assimilation process, then it would be desirable to make jobs readily available to the young.

A slightly more sophisticated argument emphasizes the severe informational problems of the young in the labor market. Teenagers and employers are involved in an elaborate game of mixing and matching skills and jobs, but there is relatively little information available to either party. The employers rely heavily on evidence of past work experience in making hiring decisions because they need to separate persons with poor work skills and weak attachments from those with superior work qualities. Employers avoid hiring workers who have been out of school for some time but have little experience, so those workers who were involuntarily unemployed are inappropriately typed as poor workers. The problems may be exacerbated in recessionary times. If employers are slow to adjust their expectations for experience from young applicants, cohorts entering a weak labor market will suffer. Of course, permanent damage need not occur at all. Early unemployment may simply be productive job search or simple consumption of leisure.

There is a small but rapidly growing literature testing the long-term effects of early spells of unemployment (see for example Becker and Hills [1978], Stevenson [1978]). These papers conclude that early unemployment has sizable long-term effects. The methodology usually involves regressions of wages or weeks worked of persons beyond their teens on duration and/or spells of teenage unemployment several years earlier. Although most pay lip service to the difficulty of controlling for individual differences, it is typical to include several background variables as a control in the equations. This methodology is troubling. If there is a true job shortage employers are likely to hire the highest quality workers first. If early unemployment is in part a reflection of weak attachment, then some persons with unemployment are also low-quality workers. In either case, early unemployment is certain to be highly correlated with aspects of worker quality. The findings of these studies document persistence very convincingly but serious questions remain about whether early experience has causal effects in later economic behavior.

I conclude that while long-term effects do exist, they may be a good deal smaller than the literature suggests.

The Data

Current published data tends to obfuscate early patterns of market experience. Data from the Current Population Survey are currently published by age group and school enrollment status. Throughout this chapter, I will concentrate only on those persons out of school. I see many fewer possibilities for long-term effects of unemployment during school. The composition of the 16–19 year old out-of-school labor force is very different from that of the 20–24 age group. The 16–19 year old group includes early dropouts and high school graduates. The 20–24 year old group includes persons with little school but eight years of experience along with recent college graduates. To look across different age groups and to draw conclusions about the patterns of unemployment as persons age is to invite error.

Ideally, one should like to follow a cohort of persons permanently out of school over five or ten years. The National Longitudinal Survey of Young Men—the so-called "Parnes data"—allows such an examination. Some 5225 young men between the ages of 14 and 24 were interviewed in 1966. They were then reinterviewed annually through 1971, then again in 1973, and again in 1975. Typically, respondents were interviewed in November about their current labor force status and most recent wage as well as about their experience over the past year. The sample chosen for analysis here was a group of roughly 750 young men who left school "permanently" in 1965, 1966, or 1967 with less than fourteen years of education. Unfortunately, this period was the height of the Vietnam war. Thus slightly over half the sample is not observed in the four full years after they left school, primarily because of military service. The 364 young men who remain do appear to be somewhat less prone to unemployment and time out of the labor force. Persons who were observed in the first full year out of school but were not observed in some later year had a labor force participation rate of 84.1%, an unemployment rate of 7.1%, and an employment rate of 78.2%. Persons who remained in the sample had rates of 86.1%, 5.0%, and 81.8% respectively. This sample selection is an obvious source of potential bias and will be addressed in more detail later.

Another well-known "problem" with the Parnes data is that they show very different rates of employment and unemployment than do published statistics derived for the CPS.[1] The longitudinal data used here show much higher employment rates and lower unemployment rates than the CPS data. For a discussion of the likely reasons for these differences see Freeman and Medoff (chapter 4 of this volume). The sample selection and CPS comparison suggest that the NLS sample may miss some of the longer-term unemployed persons, for whom unemployment could have the most serious consequences. Thus the current sample could serve to underrepresent the long-term consequences of early labor market experience.

Few of the young men in the survey data leave school in November. In the year of leaving school, retrospective labor force figures cover both time in and out of school. After numerous attempts to adjust for the problem, I finally decided to simply omit the first part-year of experience. In later sections when I refer to the first year of experience, I refer to the first full survey year after graduation or dropping out.

10.1 The Early Labor Market Experience

The labor market position of young men improves dramatically during the first four years out of school. Table 10.1 shows that while an average

Table 10.1 Unemployment Rate, Employment Ratio, and Labor Force Participation Rate for Young Men during First Four Years after Leaving School in 1965, 1966, or 1967 with Less than Thirteen Years of Schooling

	Unemployment rate[a]	Employment rate[b]	Labor force participation rate[c]
Year 1	5.0	81.8	86.1
Year 2	6.4	84.7	90.5
Year 3	4.8	89.3	93.8
Year 4	5.4	90.0	95.0

[a]Average weeks unemployed/average weeks in labor force.

[b]Average weeks employed/52.

[c]Average week in labor force/52.

of nearly 20% are without work in the first year, only 10% are not working three years later. Labor force participation rates rise precipitously, from 86% to 95%. The marked improvement is countercyclical in this case since for roughly two-thirds of the sample (those leaving school in 1966 and 1967) the fourth full year out of school comes during 1970 or 1971—recessionary years. Indeed, if the overall economic picture had remained stable over this period, even more rapid improvement would likely have occurred. Almost immediately, however, the unemployment rate shows up as a questionable indicator of labor market performance for this group. While the other statistics, most notably the employment ratio, show clear improvement over time, the unemployment rate follows no clear pattern. Although it is possible that the unemployment rate accurately captures the relative number of persons seeking work but unable to find it, it is also possible that the unchanging unemployment statistic misrepresents the trend in the labor market position of young men. In these retrospective figures, unemployment may well mean something different to persons one year out of school than to persons four years out. As the young men age, they may become increasingly reluctant to report themselves as out of the labor force even if they are not spending time in productive job search. Another alternative is that in later years only a hard core cannot find jobs. These persons become discouraged and drop out of the labor force. Either way the distinction between unemployment and time out of the labor force is blurred.

The steady improvement in the employment rate of the cohort masks remarkably dynamic labor force patterns. The initial years of employment experience are pocketed with spells of unemployment and time out of the labor force. Only 18% of all young men in this sample have four-year employment histories unmarred by a spell out of work. Table 10.2 shows that nearly 40% of all young men spend time out of the labor force in their first year, while just over one-quarter report unemploy-

Table 10.2 **Probability of Unemployment, Time out of the Labor Force, and Time Not Employed during First Four Years after Leaving School**

	Probability of unemployment	Probability of time out of labor force	Probability of time not employed
Year 1	26.9%	40.1%	56.6%
Year 2	27.5	31.9	51.1
Year 3	23.0	23.6	40.9
Year 4	21.9	24.1	38.2

ment. Overall, 57% of these young men spent some time out of work. The probabilities of adverse experiences decline substantially over the period. Yet even in the fourth year out of school when the overall employment ratio is 90%, almost 40% spend some time not employed. And while the labor force participation rate hovers at 95% in that fourth year, one-quarter spend some time neither working nor looking for work.

Perhaps the most dramatic result in these first few tables is the prominence of time out of the labor force. Nearly 40% of the sample self-report time spent neither working nor looking in the first years. These 40% report average spells of eighteen weeks—more than four months—during a period of very low unemployment. Perhaps these are discouraged workers. Yet three-quarters of them spent no time unemployed at all during that first year. Of course, some may have had severe unemployment problems in the part-year preceding the first survey year. Still, four months is a remarkably long time to be discouraged, particularly when one's peers are reporting a 5% unemployment rate. The sample selection rules, which appear to discriminate against the nonemployed, make the results seem even more dramatic. The rapid rise in labor force participation rates and employment rates during the downward swing of the business cycle must almost certainly indicate increasing labor force attachment.

One important concern is whether to regard reported unemployment as a separate experience from reported time out of the labor force. The evidence cited thus far suggests that retrospective unemployment figures do not appear to capture the essence of the employment situation. While the distinction between those actively seeking work and those who are not seems particularly important in this group, the line is poorly drawn using retrospective employment figures. Of course, few labor force statistics are derived from retrospective data. Still, the standard CPS question about whether the teenager has done anything to look for work in the past four weeks (a specific method must be listed) may not separate them too much more efficiently.

Unfortunately, if it is difficult to separate the truly unemployed from those with weak labor force attachment in surveys, it may be equally difficult for employers. Thus those persons who are seriously searching for work but have been unable to find it may suffer from guilt by association.

This brief section has painted a pattern of change and diversity. Early in their careers young men spend a great deal of time without work. By their fourth year, however, most workers are settling into a more stable and presumably permanent work situation. The next section shows that while the early years are periods of rapid improvement for the young men overall, adverse experiences persist.

10.1.1 The Persistent Pattern of Adverse Labor Market Experiences

Early labor market experiences foretell future ones. Persons who escape unemployment early will likely escape it later. Figures 10.1, 10.2, and 10.3 are probability trees for unemployment, time out of the labor force, and time not employed for the four periods. Each branch corresponds to one period. A one indicates that unemployment or nonemployment was experienced in the period, a zero indicates that it was not. Above the line in any branch is the probability of being in that state *conditional* on being at the previous branch. Below the line in parentheses is the *unconditional* probability of being on that branch (or the proportion of all persons who are found on that branch). The bottom number is the average weeks of unemployment in that period by persons on that branch. Thus in figure 10.1, 53.1% of persons who had been unemployed in their first year were unemployed in their second year. Just over 14% of all persons had unemployment both periods and these persons averaged 14.2 weeks of unemployment in the second year.

All three figures demonstrate striking persistence in the labor force experiences. The probability of unemployment (nonemployment) in the second period conditional on first period spells is .531 (.631), while those who escaped early problems have only a .180 (.354) probability of unemployment (nonemployment). By the fourth period, boys with three straight years with unemployment are seven times more likely to become unemployed than those with three straight years without it.

This sort of probability tree is common in the literature (see Heckman and Willis [1977]; Heckman [1978a and 1978b]); however, the patterns can be misleading. If spells are long, say ten weeks, and if spells are distributed randomly throughout the year, then 20% of all the unemployed in one year will have spells which overlap into the next one. This would cause a much higher probability of unemployment in the second year conditional on having experienced it in the first, regardless of the underlying pattern. In this sort of table, there is no straightforward way of making an adjustment for this problem.

Happily, overlap problems do not affect probabilities of third or fourth period events conditional on the first period event. Table 10.3 reveals

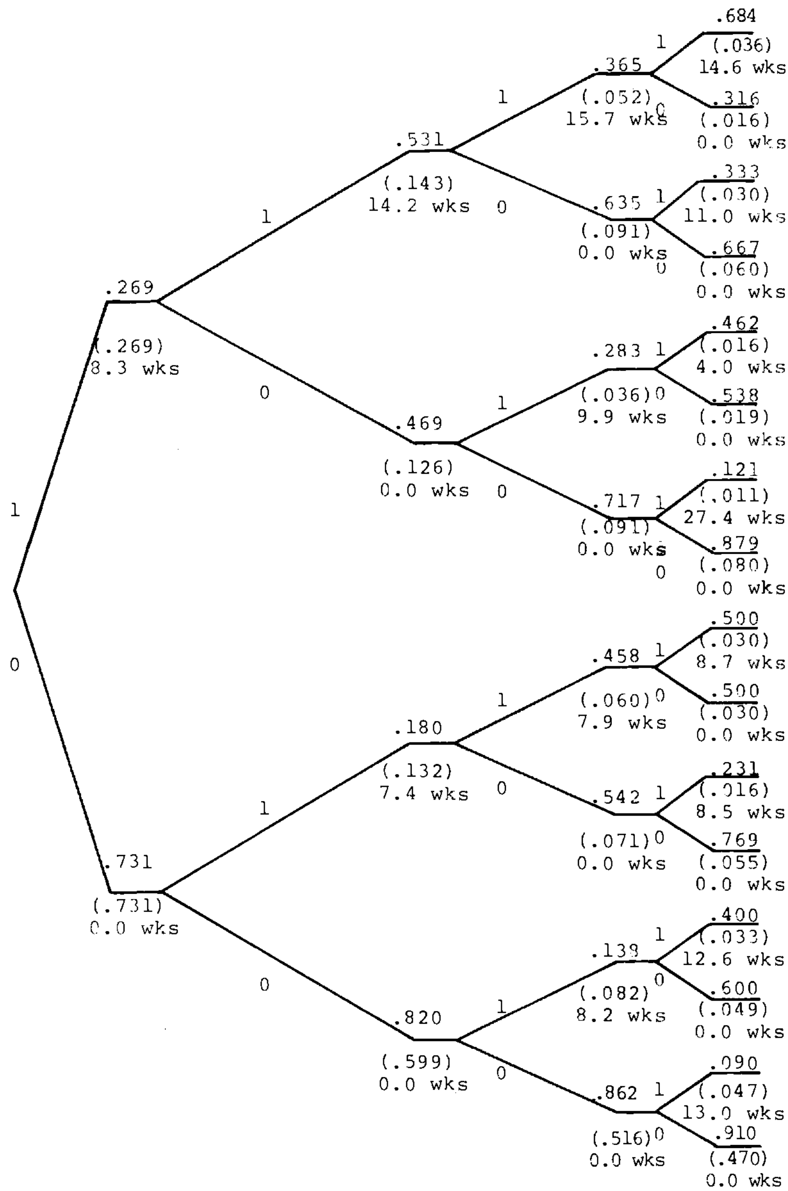

Fig. 10.1 Probability Tree of Weeks Unemployed in First Four Years out of School ($N = 364$)

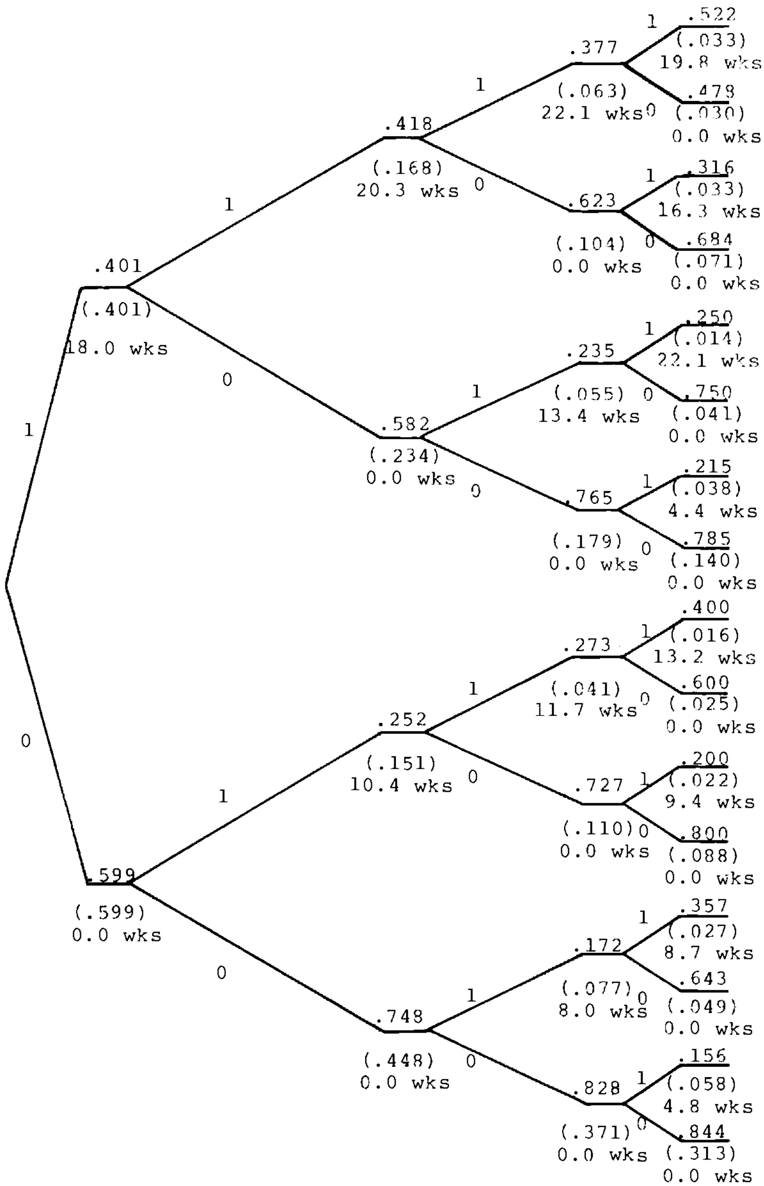

Fig. 10.2 Probability Tree of Weeks out of the Labor Force in First Four Years out of School ($N = 364$)

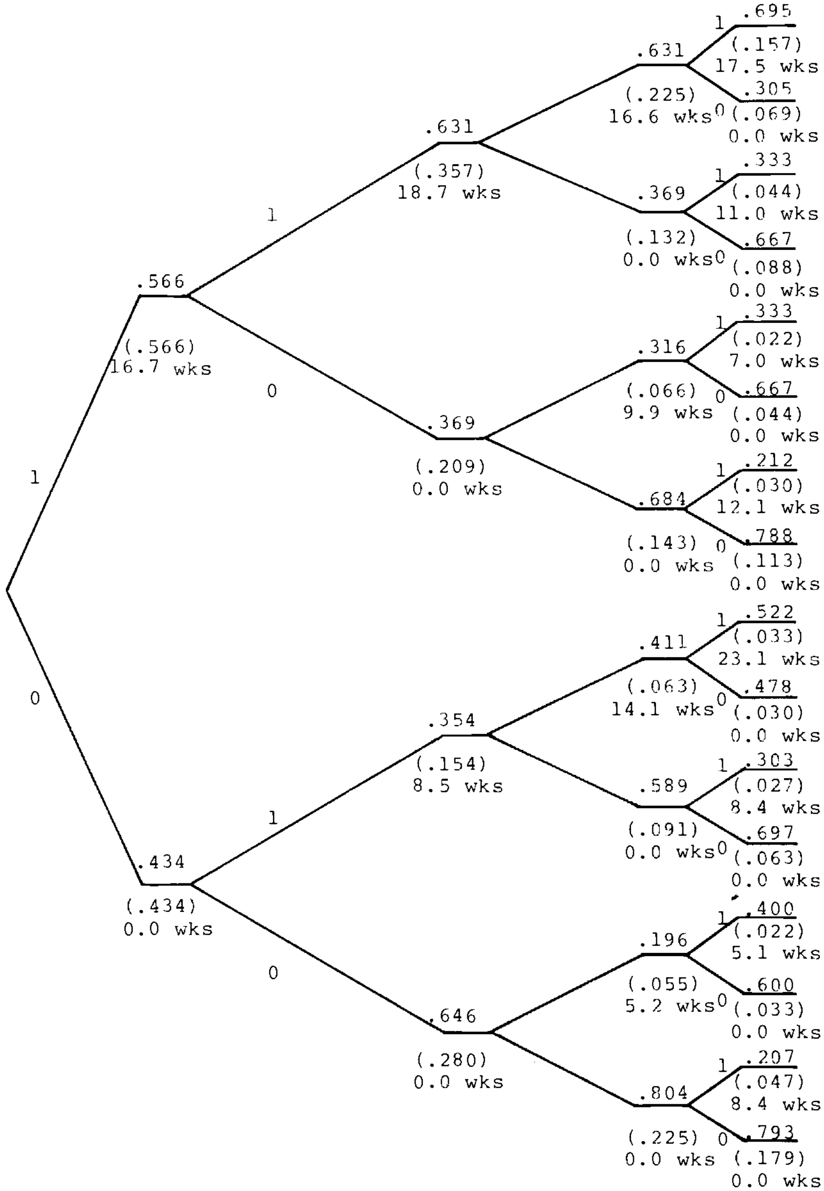

Fig. 10.3 Probability Tree of Weeks Not Employed in First Four Years out of School ($N = 364$)

that persons with poor first period records are likely to have poor records three or four years later. Persons who spent time out of work in the first period have a .447 probability of similar problems in the final year as contrasted to a .297 probability for those persons with uninterrupted work histories in the first year.

A somewhat more appealing measure of persistence is a simple correlation matrix. Table 10.4 provides the correlations for weeks of unemployment over the first four years and for the weeks not employed. Once again the persistence is prominent, but not quite so prominent as might be expected. Weeks not employed shows a one-year correlation of about .5, but it decays rapidly. Within two years the value falls to around .25. Remarkably, weeks unemployed show far less persistence and the pattern of decay is erratic. Adjacent year correlations (σ_{12}, σ_{23}, σ_{34}) show some stability, but hover at only about .3, a figure roughly comparable to the correlation between weeks not employed one or two years removed. The correlation between unemployment in the first and third years (σ_{13}) shows evidence of slight decay, but σ_{24} shows no such evidence. Then, dramatically, σ_{14} falls to .08. The unorthodox behavior of the unemployment figures once again reinforces the earlier concerns about the quality of unemployment measure (at least this retrospective measure) for this age group.

Both the unemployment and nonemployment correlations are more stable than would be generated by a first-order Markov process. The stability suggests that individual differences are an important part of the underlying process or that the process is of higher order. Unemployment and nonemployment are not events randomly distributed over this population of young men. If early unemployment or nonemployment is nothing more than search and matching of workers and jobs, then for

Table 10.3 **Probability of Adverse Market Experiences in Later Years Conditional on Early Experience**

	Unemployment	Time OLF	Time not employed
P(1 in year 2/ 1 in year 1)	.531	.418	.631
P(1 in year 2/ 0 in year 1)	.180	.252	.354
P(1 in year 3/ 1 in year 1)	.327	.294	.514
P(1 in year 3/ 0 in year 1)	.194	.197	.272
P(1 in year 4/ 1 in year 1)	.345	.294	.447
P(1 in year 4/ 0 in year 1)	.172	.205	.297

Table 10.4 **Correlation Matrix for Weeks Unemployed and Weeks Not Employed during the First Four Years out of School**

	Weeks unemployed			
Weeks unemployed	Year 1	Year 2	Year 3	Year 4
Year 1	1.00	.27	.20	.08
Year 2		1.00	.27	.26
Year 3			1.00	.39
Year 4				1.00
	Weeks not employed			
Weeks not employed	Year 1	Year 2	Year 3	Year 4
Year 1	1.00	.54	.34	.25
Year 2		1.00	.46	.34
Year 3			1.00	.47
Year 4				1.00

some at least the process is quite protracted. Since adverse employment patterns are a problem of a subclass of youngsters, programs to aid them ought to be targeted to those with early problems.

The critical question of this chapter still remains: Is the persistence a reflection only of individual differences or is future employent causally related to past experience?

10.2 The Impact of Early Unemployment on Future Unemployment: Heterogeneity and State Dependence

Persistence of labor market behavior has been noted in numerous other settings, most notably in the labor force participation of married women. A newly developing literature seeks to separate the effects of individual differences in behavior (heterogeneity) from changes in behavior induced by a previous event (state dependence). The unique character of longitudinal data allows one to control for unobserved individual characteristics in a way that no strictly cross-sectional data set does. Although there are serious conceptual problems with this formulation, the following model is continuous time will help illustrate the methodology currently employed in the literature (the problems will be considered later):

$$Y_{it} = X_{it}\beta_t + \gamma_t Y_{it-1} + \delta_{it} + U_{it}$$

Here Y_{it} is the time person i was in a particular state during period t (i.e., weeks worked), X_{it} is a vector of exogenous variables, δ_{it} is an individual constant, U_{it} is a random component. This is simply a model of a first-order Markov process with an individual component. In this example, δ_{it} is the control for heterogeneity, γ_t is the test of state dependence. Such an equation cannot be estimated from cross-sectional data because there will be more parameters than observations since each individual is accorded his own intercept. Cross-sectional estimates made without the inclusion of δ_{it} will create upward bias in the state dependence coefficient unless that part of δ_{it} which is correlated with Y_{it-1} is fully captured by a linear combination of the Xs.

By imposing restrictions on δ_{it}, one can estimate γ_t from longitudinal data. The individual component can be controlled using data from previous years. The simplest assumption is to fix the individual component over time, $\delta_{it} = \delta_i$. To simplify the example further, assume $\beta_t = \beta$, $\gamma_t = \gamma$ and that $Cov\ (U_{it}, U_{it-1}) = 0$. Simple differencing eliminates the nuisance parameter δ_i. Thus

$$Y_{it} - Y_{it-1} = (X_{it} - X_{it-1})\ \beta + \gamma(Y_{it-1} - Y_{it-2}) + U_{it} - U_{it-1}$$

Of course all exogenous variables which are invariant over time are also eliminated with this approach. Since the focus here is with the state dependence parameter, γ, this is a source of no concern. The term $(Y_{it-1} - Y_{it-2})$ is now negatively correlated with the error term, so OLS results will be negatively biased. However, Y_{it-2} and X_{it-1} can be used as instruments for this term and consistent results will be generated. Note that absolutely no distributional restrictions are imposed on the δ_i across individuals since they are simply differenced away.

Heckman (1978a, 1978b) has developed an appealing and more general counterpart to this model for the discrete case. Heckman's model transforms the dichotomous variable into a continuous one by assuming the event occurs whenever a continuous latent variable (Y^*_{it}) crosses a threshold—here assumed to be zero. A dummy variable d_{it} is assumed to be one when $Y^*_{it} > 0$ and zero otherwise. Exogenous variables X_{it} are allowed. Using a variance components error structure in Heckman's model, we can allow each individual to have his own individual component, δ_{it}, freely varying over time for the moment. One case of Heckman's somewhat more general model is then:

$$Y^*_{it} = X_{it}\ \beta_t + \sum_{j=1}^{k} \gamma_{it-j}\ d_{it-j} + \delta_{it} + \varepsilon_{it}$$

Setting $\gamma_{it-j} = \gamma_{t-j}$ and $\delta_{it} = \delta_i$ and assuming the δ_i and the ε_{it} are IID normal provide for an estimable model. Heckman offers a heuristic proof of identifiability which relies on the ordering of unconditional probabilities. Suppose $t = 2$ and the Xs are constant over time. Then conditional on X_i and δ_i, in the absence of state dependence, the probability of the sequence (1, 0) (one in first period, zero in the second) is equal to the probability of the sequence (0, 1). In the presence of state dependence, however, $P(1, 0) < P(0, 1)$. State dependence increases the likelihood that persons who experience the event in the first period will experience it again in the second. Therefore $P(1, 1)$ is increased and $P(1, 0)$ is reduced. $P(0, 1)$ on the other hand is unaffected since the event was not experienced in the first period. This relation holds for each individual; it must hold in aggregate. Thus simple run sequences alone allow testing for the presence of state dependence under particular functional form assumptions. Run sequences covering more time periods allow testing of less restrictive functional forms.

Heckman suggests this approach can be usefully applied to a variety of situations, including spells of unemployment. Several features of the Heckman model make its usefulness in this and related situations questionable. For purposes of this discussion, let us divide early job history into only two states: employed and not employed. The fundamental problem is that the model breaks a continuous time event into artificial periods. When the chosen interval is long relative to average length of stay in a state, there is inevitably an assymetry in the definition of states. Often periods are chosen to be one year long. A person is observationally reported to have been in a particular state for that period if and only if he or she experienced the state *at any time* during the period. In the current example persons who experience time out of work any time over a year receive ones, persons who do not receive zeros. Thus to be in a state, one need experience only one week of nonemployment, but to be out of the state one need experience fifty-two weeks of unemployment. If we simply redefine state 1 and having experienced any *employment*, a very different pattern of states emerges. Virtually everyone is always in state 1. The presence or absence of state dependence may depend on which state is accorded the special privilege of being designated as the 1.

On the other hand, if the periods are short relative to the spells, then state dependence exists almost by assumption. If spells tend to be longer than periods then the probability of being in the state conditional on having been in it in the previous period is high. Indeed, even if spells tend to be four or five times shorter than the periods, one can predict with certainty that at least 20 to 25% of persons who experience the event one period will experience it again in the next period simply because spells overlap.

The arbitrary designation of time periods and states means an observed data point (1, 1) may represent a host of very different histories. One person may have been in the state continuously for two periods. Another may have been in it only a few days but those days happened to overlap two periods. Still a third person might have had several spells in the state in each period. These problems represent more than lost efficiency. They imply peculiar results. The problem of overlapping spells is particularly troubling in the current treatment. If spells last an average of thirteen weeks, then one-fourth of all spells in one year will overlap into another. This implies that even if the spell has no long-term effect, $P(1, 1)$ is increased. Since the $P(1|1) > P(1|0)$ there appears to be state dependence where there is none. Although these problems are particularly acute in the Heckman formulation using years as periods, they are also present to some degree in the continuous model presented earlier, as we shall see below.

One way to minimize these problems is to use point in time sampling. At the start of each time period persons are interviewed and their current state recorded. There is no asymmetry in the definition of states in this case. And if spells tend to be shorter than periods, overlap problems are less serious. Of course, there is great loss of information in this approach. More importantly, since spells of employment frequently last several years, the chosen periods may have to be quite long.

Obviously, the notion of state dependence is a confusing one. In the next few paragraphs I present a nontechnical discussion in an attempt to clarify some of the concepts. For a more technical treatment see Chamberlain (1978 and 1979).

A complete analysis of heterogeneity and state dependence would treat each event in continuous time with a particular starting and ending date. We must separate two distinct types of state dependence. Once a person has entered a particular state, say employment, there is a tendency to remain there for some period of time. The probability of remaining in some state is always higher than the probability of entering it from another if the time interval is short enough. Virtually all persons who work one minute will work the next, regardless of their underlying propensity to work over a month, year, or decade. Traditionally this inertia has been captured with a Markov model. Conditional on being in a state, a person has a certain escape probability over a given period of time *which may be quite independent of his past history of spells or states.*

For example, a young black male teenager who is unemployed this week could be far more likely to be unemployed next week than if he had been employed this week simply because it is hard for young blacks to find jobs. It could be that nothing about his work history or his duration of current unemployment influences his ability to get a job; yet being unemployed now indicates that he is less likely to be employed next week. Unemployment doesn't change the individual per se, it is just a difficult state for the teenager to escape. Heterogeneity must imply that each individual has his or her own escape probability from each state. Let us label this form of state dependence simple Markov-type persistence. The key notion is that it is what state one is in that counts, not his past history. This persistence is unquestionably present in all human endeavors to some degree.

If the force of escape from one or another state *is* influenced by previous experience, then the second form of state dependence—experience dependence—is present. Exit probabilities may rise or fall with time in the current spell. Work history may influence the likelihood of employment when a teenager is unemployed. Experience dependence corresponds most closely to the conception of state dependence described in the literature. A person is actually "changed" by a particular event. Models which postulate that the accumulation or depreciation of human capital or of information or even of signals of worker quality alters the likelihood of work all imply an altered force of escape from one state or another because of the individual's past experience. Ideally, it is this form of state dependence that we seek to capture.

Simple Markov-type persistence certainly is not uninteresting. The distribution of forces of escape will strongly influence the concentration of unemployment across individuals. Macroeconomic policies can alter escape rates and may provide great benefit to those with otherwise very low rates of escape from unemployment. But if experience dependence is not present, once a spell is over so is its impact.

Unfortunately, the current models capture both Markov-type persistence and experience dependence simultaneously. Markov persistence requires two heterogeneity parameters: the force of escape from each state. In the Heckman formulation this implies an individual intercept δ_i and an individual coefficient on the person's state last period. This can be modeled (omitting the Xs):

$$Y^*_{it} = \delta_i + \psi_i d_{it-1} + \sum_{j=2}^{k} \gamma_{t-j} di_{t-j} + \varepsilon_{it}$$

If the time periods are quite short, then δ_i effectively captures the Markov-type probability of *entering* the 1 state; ψ_i, the probability of *remaining* in it. With short periods d_{it-1} captures the persons most recent state—the "current state" while the state in "next period" is being determined. Markov persistence virtually guarantees that ψ_i will be positive as the period shrinks. Experience dependence requires previous job history—not just that the current state alter the probability of entering or remaining in a state. Thus coefficients on d_{it-2}, d_{it-3} . . . are nonzero. The γ_{t-j} here captures this experience dependence.[2]

Estimation of this model is complicated by the fact that the δ_i and ψ_i are highly correlated with d_{it-1} and the d_{it-j} since high values of the individual components increase the likelihood that any $d_{ij} = 1$. Estimating the equation assuming $\psi_i = \Psi$ may substantially upward bias the γ_{t-j} coefficients because the omitted term $(\psi_i - \Psi)d_{it-1}$ is positively correlated the d_{it-j}. Previous work using this model has overestimated experience dependence for two reasons. First, the coefficient on the once lagged d_{it} inevitably reflects not only experience dependence but also Markov persistence. Second, because the coefficient on d_{it-1} is constrained to equality across individuals, the γ_{t-j} also captures some Markov-type persistence. Heterogeneity has simply not been properly controlled for.

The continuous model described at the beginning of this section also inadvertently captures some Markov-type persistence in the state dependence parameter. Suppose weeks worked is the dependent variable. Then it is tempting to regard δ_i as the expected weeks worked in year t given an individual's two escape probabilities. However, even in the presence of Markov persistence alone, the individual's expected weeks worked will be greater if he begins the period working than if he enters without work. A preceding year's weeks worked help predict the person's state at the end of that year and therefore at the start of the current year. Anyone who worked fifty-two weeks in year $t + 1$ was working at the start of year t. He will certainly be expected to have more weeks worked in year t than an identical individual who begins year t out of work. Even conditional on δ_i, weeks worked in one year are correlated with weeks worked in the next because they help predict the person's state at the start of the next period. The correct model is thus

$$Y_{it} = \delta_i + \psi_i b_{it} + \gamma Y_{it-1} + U_{it}$$

where b_{it} is now a dummy variable capturing the person's state at the *beginning* of year t. In this model δ_i and ψ_i are reflective of the two Markov escape probabilities and γ is a measure of true experience dependence. Even if we know δ_i with certainty, we could not estimate this equation because ψ_i varies with each individual and is highly correlated with b_{it} and Y_{it-1}.

When we difference, however, the advantages of this continuous formulation become clearer:

$$Y_{it} - Y_{it-1} = \psi_i(b_{it} - b_{it-1}) + \gamma(Y_{it-1} - Y_{it-2}) \\ + U_{it} - U_{it-1}$$

There is only a bias problem for persons who change their beginning state from one period to the next. Otherwise $(b_{it} - b_{it-1}) = 0$ and ψ_i vanishes. One cannot estimate the equation for these persons only because b_{it} is correlated with U_{it-1} and conditioning on it will introduce bias.[3] But in the present sample, nearly 90% of all persons are observed in the same state at the start of any two consecutive years, so the bias on γ may be quite small.

Including $b_{it} - b_{it-1}$ (using b_{it-1} as an instrument) will reduce the bias but will not fully eliminate it. At the same time γ will not fully capture experience dependence because δ_i and ψ_i are average yearly probabilities which will in part reflect some experience dependence if the underlying forces of escape are high. In the presence of these offsetting "biases," I regard γ as a rough measure of experience dependence. Any better measures require complete work histories and present serious methodological problems.

In this continuous model, identification was achieved with the imposition of three important restrictions: $\delta_{it} = \delta_i$, $\psi_{it} = \psi_i$ and $Cov(U_{it}, U_{it-1}) = 0$. If any of these restrictions are false, spurious state dependence can be generated. Probably the most serious concern for this group is nonstationarity of the individual components δ_i and ψ_i. If weeks worked is the endogenous variable, δ_i and ψ_i might be seen as that part of maturity, ability, or labor force attachment not captured by the Xs. Since these may grow or decay over time, it seems desirable to free up the individual components. Although we cannot let the components decay or grow at different rates, a model allowing $\delta_{it} = \lambda_t\delta_i$ and $\psi_{it} = \lambda_t\psi_i$ can be estimated using four years of data. We solve for δ_i in the third year equations and substitute it into the fourth:

$$Y_{i3} = \lambda_3\delta_i + \lambda_3\psi_i b_{i3} + \gamma_3 Y_{i2} + X_{i3}\beta_3 + U_{i3}$$

So

$$\delta_i = -\psi_i b_{i3} + \frac{1}{\lambda_3}\left(Y_{i3} - \gamma_3 Y_{i2} - X_{i3}\beta_3 - U_{i3}\right)$$

Substituting into the equation for Y_{i4}

$$Y_{i4} = \lambda_4\psi_i\,(b_{i4} - b_{i3}) + \left(\gamma_4 + \frac{\lambda_4}{\lambda_3}\right) Y_{i3} - \frac{\lambda_4}{\lambda_3}\gamma_3 Y_{i2} +$$

$$X_{i4}\,\beta_4 - \frac{\lambda_4}{\lambda_3} X_{i3}\beta_3 + U_{i4} - \frac{\lambda_4}{\lambda_3} U_{i3}$$

The effects of the first term have been discussed earlier. The only other problem is that Y_{i3} is correlated with the error term, Y_{i1} is not however, and serves as a natural instrument for Y_{i3}. If we constrain $\gamma_4 = \gamma_3$ we can obtain estimates of γ and λ_4/λ_3 although we cannot tell which is which since they enter the equation symmetrically.

The restriction $Cov(U_{it}, U_{it-1}) = 0$ helps to highlight an important distinction between state dependence and serial correlation. In the absence of strong Xs which change over time, there is no meaningful empirical distinction between serial correlation and state dependence. However, in the presence of Xs the distinction is important. State dependence implies that a change in X will cause a change in Y not only in the present period but in future periods as well, because the initial increase in Y induces future increases in Y. If serial correlation is present, a change in X will have its full force immediately, with no damped response into the future. In the case of unemployment, one might ask whether a weak labor market now induces more unemployment in the future even when the labor market regains its strength. If the answer is yes, then state dependence may be present. Otherwise, state dependence probably is not present. Unfortunately, it is likely to be virtually impossible to capture both serial correlation and a nonstationarity of individual specific constant. The only reasonable approach I can see is to assume that both serial correlation and nonstationarity are captured using a time specific coefficient on the individual effect. These models then were used to estimate the long-run effects of unemployment.

10.2.1 Empirical Results

Before performing the more complicated tests for state dependence described above, we might try to find "natural experiments" which would reveal it much more simply. Local unemployment rates vary dramatically over time and across locales. One natural experiment would be to test whether persons who enter a weak labor market which later turns strong fare less well than those who enter a strong market which remains strong. A unique feature of the "Parnes data" is the availability of an area unemployment rate for most persons in each year. The rate for small local areas about the size of an SMSA was derived from a twelve-month average of monthly local unemployment rates from the Current Population Survey. Presumably the area unemployment is only slightly correlated with individual effects, so with a few controls for individual characteristics, we might simply test the importance of lagged unemployment rate in equations with both current and lagged unemployment rates. If entering a weak labor market left long-term scars, then the lagged rate should be negative and significant. Unfortunately, the area rate behaved very poorly. Even in equations without the lagged rate, the coefficient on the current rate, though usually of the correct sign, was rarely significant and was highly unstable. When the lagged rate was included, the results were invariably insignificant and occasionally even the sign on the current rate was perverse.

Even though the area rates performed poorly on this data, this experiment should be performed on other samples if possible. Ultimately, a conclusion resting on such a simple methodology would be the most compelling test for the long-run effects of short-run macropolicy.[4]

The techniques described in the previous section were applied to weeks worked and to weeks unemployed. Weeks worked were chosen over weeks not worked only because they seem conceptually easier to deal with. Obviously since weeks not worked are simply fifty-two less weeks worked, the results would be identical except for the constant term and a sign change on the coefficients of the exogenous variables if the alternative variable was used. There were 298 observations in the final sample.

There is a purely statistical problem associated with the use of the various controls for heterogeneity in equations predicting weeks worked or weeks unemployed. Both are limited dependent variables; they cannot exceed fifty-two or fall below zero. The importance of the problem is most evident in the case of weeks worked. As weeks worked approach fifty-two the estimate of state dependence will approach zero if controls are made for heterogeneity. Statistically, the limited variable will induce an artifically negative correlation between once lagged weeks and the error term. The results follows from the fact that if lagged weeks are large, the positive end of the distribution of the error term is likely to be truncated. Intuitively, once weeks worked approach fifty-two, regardless of the true strength of state dependence, the next years' weeks cannot be pushed above fifty-two. This problem is of greater concern in later years when more and more of the young men approach fifty-two weeks employment. There are well-known methodologies to correct truncated dependent variables. These typically do not apply to situations where a lagged dependent variable is correlated with the error term for reasons other than truncation. Heterogeneity further complicates the problem. No attempt was made to develop the appropriate truncation corrections for these equations. If we view the solution to the truncation problem as the inclusion of a truncation correction variable, the problem is unlikely to be particularly acute in the difference equations. In these situations only the *change* in the truncation variable is omitted, and these changes will be relatively small, particularly as persons approach fifty-two weeks. Actually, persons who remain at fifty-two weeks in all three years impart no bias at all in the absence of exogenous variables. They simply provide no information since $y_{it} - y_{it-1} = 0$.[5]

The wage rate normally appears in labor supply equations. At the same time human capital theory suggests that work experience will be associated with higher wages as individuals invest in on-the-job training. To prevent the wage variable from capturing any effects of increased investment, the variable LW_{it} reflects the wage at the beginning of period t while WW_t equals weeks worked during year t. To eliminate potential bias, the various equations (because weeks worked in year $t-1$ and therefore U_{t-1} alters the wage in year t) the wage variables were always instrumented with LW_{it-1} and LW_{it-2} in equations controlling for heterogeneity. All strictly exogenous variables are measured at the beginning of each period.

Table 10.7 presents the results of regressions of weeks worked and weeks unemployed on the once lagged counterparts. The only correction for heterogeneity is the inclusion of a few personal characteristics like age, race, and level of schooling. As anticipated, lagged values of weeks worked and weeks unemployed have sizeable coefficients and small standard errors. As in previous examples in this paper the results for weeks worked are far more stable than those for weeks unemployed.

When all years are estimated as a system and the coefficient on lagged weeks unemployed is constrained to equality over all three years, the coefficient is .27; the coefficient on weeks worked, .39. The results again suggest substantial persistence of early experience. Still, even without controlling for heterogeneity, the coefficient on weeks unemployed is low. Even if this were the correct estimate of state dependence, a twenty-six-week spell of unemployment would induce just two extra weeks of unemployment two years later. An equal spell without work would induce a four-week spell two years later according to these results. With appropriate corrections for heterogeneity, state dependence estimates should fall to even lower levels.

One control for heterogeneity is differencing. This eliminates any stationary person effects. The second is to include the state at the beginning of each period. Difference equation results are displayed on tables 10.8 and 10.9. In equations (1) and (2), twice lagged weeks unemployed and weeks employed, and once lagged lag wage, and beginning state dummies, serve as the principle instruments to the lagged differences on weeks unemployed, weeks worked, lag wage, and beginning states re-

Table 10.5 Definitions of Variables Used in Regressions

AGE_t	Age at start of year t.
$AREA_t$	Area unemployment rate at start of year t.
$BLACK$	Race dummy (1 = nonwhite).
EM_t	Employment dummy (1 = employed) at start of year t.
LW_t	Log of wage at start of year t.
MAR_t	Marriage dummy (1 = married) at start of year t.
$SCHOOL$	Years of school completed.
$SMSA_t$	SMSA dummy (1 = resides in SMSA) at start of year t.
$SOUTH_t$	South dummy (1 = resides in South) at start of year t.
UN_t	Unemployment dummy (1 = unemployed) at start of year t.
WW_t	Weeks worked in year t.
WUN_t	Weeks unemployed in year t.
$Dxxxx$	Change in variable $xxxx$.

Table 10.6 Means and Standard Deviations for Variables Used in Regressions

	Mean	S.D.		Mean	S.D.
AGE 2	18.8	1.98	*SMSA* 3	.664	.473
AREA 2	4.33	1.72	*SMSA* 4	.668	.472
AREA 3	4.22	1.85	*SOUTH* 2	.446	.497
AREA 4	4.59	1.93	*SOUTH* 3	.432	.496
BLACK	.383	.487	*SOUTH* 4	.422	.495
EM 2	.899	.301	*UN* 2	.060	.238
EM 3	.932	.251	*UN* 3	.050	.219
EM 4	.946	.225	*UN* 4	.037	.189
LW 2	.673	.491	*WW* 1	43.4	12.77
LW 3	.826	.442	*WW* 2	45.2	11.45
LW 4	.947	.433	*WW* 3	47.1	9.78
MAR 2	.292	.455	*WW* 4	47.2	10.64
MAR 3	.446	.498	*WUN* 1	2.53	6.28
MAR 4	.507	.500	*WUN* 2	2.88	7.27
SCHOOL	11.2	1.51	*WUN* 3	2.33	6.33
SMSA 2	.634	.482	*WUN* 4	2.41	7.44

Table 10.7 Regressions of Weeks Worked and Weeks Unemployed on Once-lagged Values

	Dependent Variables					
	Weeks worked			Weeks unemployed		
	WW_4 ($t=4$)	WW_3 ($t=3$)	WW_2 ($t=2$)	WUN_4 ($t=4$)	WUN_3 ($t=3$)	WUN_2 ($t=2$)
$BLACK$	−.442	.596	−1.54	.370	.328	1.25
	(1.31)	(1.16)	(1.40)	(.945)	(.847)	(.961)
$SCHOOL$	.348	.239	.541	−.364	−.497	−.073
	(.431)	(.384)	(.450)	(.310)	(.278)	(.306)
AGE_2	.140	.048	.442	−.154	−.369	.005
	(.326)	(.293)	(.355)	(.235)	(.211)	(.242)
$SMSA_t$	−2.55	−1.78	.910	.824	−.331	1.08
	(1.33)	(1.19)	(1.37)	(.943)	(.867)	(.932)
$SOUTH_t$	−.082	.298	3.48	−.768	−1.01	−2.23
	(1.38)	(1.26)	(2.33)	(1.00)	(.914)	(1.03)
MAR_t	2.94	.667	1.45	−1.25	−1.11	−1.36
	(1.22)	(1.09)	(1.43)	(.875)	(.789)	(.967)
$AREA_t$	.193	−.236	−.464	−.148	.042	.356
	(.308)	(.291)	(.372)	(.222)	(.211)	(.255)
LW_t	.686	2.54	1.31	−.741	1.00	−1.18
	(1.64)	(1.54)	(1.49)	(1.16)	(1.12)	(1.01)
WW_{t-1}	.378	.399	.354	—	—	—
	(.062)	(.046)	(.049)			
WUN_{t-1}	—	—	—	.359	.163	.300
				(.067)	(.051)	(.065)
SEE	9.54	8.44	10.1	6.87	6.12	6.89
R^2	.23	.28	.25	.18	.10	.13

spectively. The equations also include changes in residence, marital status, and area unemployment rate. The personal characteristic variables remain to capture any systematic changes in the dependent variables.

Efficiency can be gained, however, with the use of three stage least squares because both error terms contain the residuals from the third year. Equations (3) and (4) are the unconstrained three stage least squares results. For these equations weeks worked and weeks unemployed in the first year were used as the primary instruments. Finally, in equation (5) the coefficients on all variables shown were constrained to equality across the two years.

The results in the unemployment equations are quite striking. All evidence of state dependence is eliminated. The coefficients on the lagged change in weeks unemployed are rarely positive and never significant. Indeed, there is even a hint in the results of negative state dependence. Persons with unusually high unemployment one year will have unusually low unemployment the next. Note also the poor performance of the change in beginning state dummies, DUN_t. The standard errors are always quite high and in four of five cases the sign is incorrect. Very few persons change states, so DUN_t is virtually always zero and its coefficient is derived using instrumental variables. These facts no doubt explain a large part of the perverse results. Nonetheless, there appears to be relatively little Markov persistence in unemployment not captured by δ_i. Even without controlling for nonstationarity or serial correlation then, persistence of unemployment—as distinguished from nonemployment—can be entirely attributed to heterogeneity rather than state dependence.

The results for weeks worked are quite different. Although corrections for heterogeneity substantially reduce the coefficient on the lagged dependent variable, some experience dependence remains. The experience dependence parameter varies from .08 to .19 across years and specifications. In the constrained 3SLS equation its value is .13 and is nearly twice its standard error in spite of being derived using instrumental variables. Unless the results are due to serial correlation, this coefficient indicates that persons who work an extra thirty weeks one year will work an additional four during the next as a direct result of this extra employment.

There is also strong evidence for the presence of Markov persistence. On average, persons who are working at the beginning of a year are expected to work five weeks more in that year than if they are out of

Table 10.8 **Difference Equation Results for Weeks Unemployed**

	Method and dependent variable				
	IV[a]		3SLS[b]		Constrained 3SLS[b]
Variable	(1) $DWUN_4$	(2) $DWUN_3$	(3) $DWUN_4$	(4) $DWUN_3$	(5) $DWUN_4$ $DWUN_3$
$DSMSA_t$	−4.51	0.28	−4.41	0.48	−1.89
	(1.78)	(1.94)	(1.88)	(1.94)	(1.29)
$DSOUTH_t$	−2.75	2.18	−2.89	2.07	−0.26
	(4.99)	(4.61)	(4.91)	(4.59)	(3.32)
$DMAR_t$	−0.43	1.30	−0.78	1.33	0.69
	(1.57)	(1.24)	(1.65)	(1.24)	(0.97)
$DAREA_t$	−0.68	0.06	−0.64	0.07	−0.35
	(0.36)	(0.46)	(0.38)	(0.46)	(0.28)
DLW_t	7.34	1.12	7.24	0.51	1.15
	(3.07)	(2.20)	(5.15)	(1.99)	(1.77)
DUN_t	2.06	−2.45	−2.21	−2.14	−1.39
	(2.67)	(2.20)	(6.99)	(2.20)	(2.04)
$DWUN_{t-1}$	−0.05	−0.002	−0.09	0.001	−0.04
	(0.07)	(0.102)	(0.07)	(0.10)	(0.09)

NOTE: Standard errors in parentheses. All equations include year dummies, *AGE2*, *BLACK*, and *SCHOOL*.

[a]Instruments include all past and future values of WW_{t-2}, WUN_{t-2}.

[b] Instruments include all past and future values of *SMSA*, *SOUTH*, *MAR*, *AREA*, WW_1, WUN_1.

Table 10.9 Difference Equation Results for Weeks Worked

	Method and dependent variable				
	IV[a]		3SLS[b]		Constrained 3SLS[b]
Variable	(1) DWW_4	(2) DWW_3	(3) DWW_4	(4) DWW_3	(5) DWW_4 DWW_3
$DSMSA_t$	4.36	4.54	5.61	3.77	3.97
	(2.66)	(2.60)	(2.75)	(2.58)	(1.79)
$DSOUTH_t$	13.75	1.64	15.57	1.75	7.31
	(7.50)	(6.17)	(7.18)	(6.10)	(4.56)
$DMAR_t$	−0.69	−1.75	0.76	−1.59	−1.22
	(2.36)	(1.68)	(2.40)	(1.67)	(1.35)
$DAREA_t$	0.47	−0.12	0.48	−0.12	0.25
	(0.53)	(0.62)	(0.54)	(0.62)	(0.39)
DLW_t	1.06	−1.98	−3.14	−1.06	−0.54
	(4.54)	(2.68)	(7.72)	(2.65)	(2.39)
DEM_t	3.54	4.92	3.75	5.34	4.63
	(3.18)	(2.40)	(7.35)	(2.39)	(2.22)
DWW_{t-1}	0.19	0.12	0.08	0.14	0.13
	(0.10)	(0.08)	(0.25)	(0.08)	(0.07)

NOTE: Standard errors in parentheses. All equations include year dummies, *AGE2*, *BLACK*, and *SCHOOL*.

[a]Instruments include all past and future values of *SMSA*, *SOUTH*, *MAR*, *AREA*, WW_{t-2}, WUN_{t-2}, LW_{t-1}, EM_{t-1}.

[b]Instruments include all past and future values of *SMSA*, *SOUTH*, *MAR*, *AREA*, WW_1, WUN_1, LW_2, EM_2.

work. Excluding this parameter does seriously upward bias the experience dependence parameter. In the constrained 3SLS equation with this omitted, the dependence parameter is 0.21.

In sharp contrast to the results for unemployment, then, controls for heterogeneity do not eliminate the experience dependence estimate and the beginning state variable performs well. This is perhaps the most conclusive evidence that the retrospective unemployment rates have little meaning. Unemployment as measured here does not beget unemployment. Nonemployment begets nonemployment. Or, even more convincingly, employment begets employment. The results suggest real gains from work.

One disappointment in the results is the poor showing of the exogenous variables. Most were insignificant in the constrained three-stage equations. The *SMSA*, *SOUTH*, and *MAR* variables were not expected to perform well since few persons moved or got married. But the performance of the area variable was unanticipated. Its sign was often incorrect; its magnitude was usually low; and its standard error was always high. The lack of strong exogenous variables prevents certain isolation of serial correlation and state dependence. It is possible that the results are evidence only that shocks persist, not that a terminated spell has lasting effects. Corrections for nonstationarity, however, should capture much of the effects of serial correlation.

A second surprise was the very weak performance of the wage in all equations and specifications. Even in the equations that do not control for heterogeneity (table 10.7), the coefficients on LW_t are quite small and never significant—at most a 10% increase in wage increases weeks worked by a trifling two days! In the difference equations, the standard errors are inevitably quite high and most signs are incorrect. Using the change in wage rather than the absolute level does little to improve the performance of this measure. Although perplexing, these results are strongly verified in the next section. *Measured* wage, of course, may be quite different from potential wage if the youngster is investing in on-the-job training.

Nonstationarity, because of some forms of serial correlation or changes in work attachment, might be a source of serious bias in the results. Sharply changing employment rates resulting from rising or decaying heterogeneity *unrelated* to employment could be spuriously picked up as experience dependence. Including age, race, marital status, and an intercept in the difference equations captures systematic changes and helps to minimize the problem. Corrections for nonstationarity require four years of data. Thus nonstationarity can only be tested between the third and fourth years.

Table 10.10 presents the results for weeks unemployed and weeks worked designed to isolate the effects of nonstationarity and state dependence. Once again the unemployment equation behaves badly, WUN_3 failing even to change sign. The weeks-worked equation, however, performs surprisingly well. Although the standard error in the twice lagged weeks worked is large, so too is its magnitude. The coefficients imply a nonstationarity parameter (ratio of the individual effects in years three and four) of 0.76 and a state dependence parameter of 0.11. (Although the specification allows either parameter to be 0.76 or 0.11, it is clear from context which is which.) The heterogeneity parameter does show some decay (capturing some serial correlation no doubt), but the experience dependence parameter is nearly identical to that derived in the constrained 3SLS specification.

This analysis illustrates the critical importance of controlling for heterogeneity. Controls eliminated all of the apparent state dependence in unemployment equations. They reduced by two-thirds the dependence parameter in the weeks-worked equations. Previous studies, which used only additional demographic variables to control for heterogeneity, have seriously overstated the true long-term impact of teenage unemployment on future labor market performance.

The conclusion then is that working does have some benefit beyond the current year. Someone working an extra thirty weeks in one year will

Table 10.10 **Instrumental Variable Equations Allowing Nonstationarity of Individual Component**

	Dependent variable			Dependent variable	
	WW 4	*WUN* 4		WW 4	*WUN* 4
SMSA 4	4.42	−4.03	*LW* 4	−7.13	11.96
	(2.71)	(2.08)		(6.67)	(5.23)
SMSA 3	−5.67	4.70	*LW* 3	1.65	−7.08
	(2.59)	(1.99)		(4.34)	(3.45)
SOUTH 4	15.37	−3.92	*DEM* 4	3.64	—
	(6.65)	(4.96)		(3.01)	
SOUTH 3	−16.97	4.47	*WW* 3	0.87	—
	(6.84)	(3.10)		(0.19)	
MAR 4	2.01	−2.76	*WW* 2	−0.084	—
	(2.42)	(1.75)		(0.098)	
MAR 3	1.23	0.25	*DUN* 4	—	−0.04
	(2.29)	(1.66)			(3.03)
AREA 4	0.30	0.04	*WUN* 3	—	0.43
	(0.50)	(0.39)			(0.20)
AREA 3	0.31	0.08	*WUN* 2	—	0.081
	(0.54)	(0.39)			(0.072)
			SEE	10.5	7.84

NOTE: Equations also include *BLACK*, *SCHOOL*, *AGE* 2. Instruments include *SMSA* 4, *SMSA* 3, *SOUTH* 4, *SOUTH* 3, *MAR* 4, *MAR* 3, *AREA* 4, *AREA* 3, *BLACK*, *SCHOOL*, AGE_2, WW_2, WW_1, WUN_2, WUN_1, LW_3, LW_2, EM_3, EM_2, UN_3, UN_2.

perhaps work an extra four in the next. This result does not distinguish between voluntary and involuntary time out of work. Work may improve skills, open new options for employment, or simply increase work attachment.

Nonetheless, in absolute terms, the long-run impact is relatively small. Even thirty weeks out of work have virtually no impact after one or two years. For this group of youngsters there is no evidence of a long-term cycle of recurring periods without employment induced by an early episode out of work—experience dependence yes, but a serious "permanent scar," no.

These estimates are not perfect. There are potential biases in both directions. Nevertheless, I find the evidence that teenage nonemployment exhibits short-term state dependence quite compelling. There are, however, three important caveats. First, this evidence is for a group of teenagers who entered the labor force in extremely favorable times. In this period it may have been the case that jobs were readily available for most youngsters. The seventies have brought a substantially worse job outlook. In this environment the effects of employment and the lack of it may be very different. Second, this is not a random sample of young persons. Some of the long term nonemployed may have been excluded from the sample. These persons may gain and lose more from being in or out of work. Finally, the sample here is too small to separate effects on specific groups. It may be that one can isolate stronger effects among blacks or low income persons.

These concerns notwithstanding, the current evidence is clear. Teenage nonemployment has real but short-lived adverse effects on teenage employment prospects.

10.3 The Impact of Work Experience on Wages

The second potential cost of being out of work is that the lost experience will translate into reduced wages. In the long run, reduced wages could be a far more important cost of unemployment. Lost experience could travel with the worker over his life. Each job may serve as a stepping stone to another. Lost experience at least delays the start of the young worker's climb. Worse, it may track the worker into a less desirable chain of jobs. This final section attempts to separate the cost of lost experience from differences in individual earning capacity correlated with work experience.

Assessing the true impact of work experience in a particular year apart from heterogeneity is a very complex problem. The triangular structure of wages whereby work experience influences wages which in turn influences future work experience, in combination with the direct experience dependence from work experience, creates a hopelessly tangled collection of heterogeneity terms with coefficients which vary over time.

The problems can best be understood by starting with a multiequation system. Let LW_{it} be the natural log of wages of individual *i at the start of* year *t*, X_{it} a vector of exogenous variables, and WW_{it} be weeks worked in year *t*. One model of wages and employment is:

$$LW_{it} = X_{it}\beta_t + \sum_{j=1}^{t-1} \alpha_{tt-j}\, WW_{it-j} + \lambda_{it} + \varepsilon_{it} \tag{1}$$

$$WW_{it} = X_{it}\beta_t + \gamma_t\, WW_{it-1} + \omega_t LW_{it} + \delta_{it} + \Psi_{it} b_{it} + U_{it} \tag{2}$$

Here equation (1) is just a straightforward human capital-type wage equation; equation (2) is just the labor supply relation from the previous section. λ_{it} is a heterogeneity term in the wage equation, δ_{it} and Ψ_{it} are the individual components in the weeks-worked model. Note that α_{tt-j} is almost certainly not going to be constant across weeks worked in different years since the flattening profile suggests diminished investment over time.

Only lagged weeks worked appear in the wage equation. Thus the system is triangular and a reduced form equation can be derived in a straightforward fashion. If we assume $\lambda_{it} = \lambda_i$, $\delta_{it} = \delta_i$, and $\Psi_{it} = \Psi_i$ and if we condition on WW_{i1}, the reduced form equation will have the following form.

$$LW_{it} = \sum_{j=2}^{t} X_{ij}A_j + B_t WW_{i1} + C_t\delta_i + \sum_{j=2}^{t} D_t\Psi_i d_{it} + E_t\lambda_i + \sum_{j=2}^{t} F_t U_{it} + \sum_{j=2}^{t} G_t\varepsilon_{it}$$

The coefficient on WW_{i1} in the correctly estimated reduced-form equation captures the full impact of early unemployment on the wage in year t. Previous authors have estimated equations of this type in the past but have included few controls for heterogeneity or Markov persistence.

The reduced-form equation helps point out the dual biases present in OLS estimation of this equation. Early experience may be correlated with the individual component in wages, λ_i ("ability"), upward biasing the coefficient on WW_{i1}. This bias grows over time because λ_i affects wages each year which alters future weeks worked which in turn influences future wages. At the same time, early experience is correlated with later experience in part because of the individual components of experience, δ_i, Ψ_i ("work attachment" and "ease of finding a job"). Since experience yields positive benefits, the coefficient on WW_{i1} is further biased because early experience inappropriately captures some of the effects of later experience. This effect also grows over time; each year brings new experience correlated with first year's experience. (In practice, of course, most workers eventually hit roughly fifty-two weeks of employment each year so the correlation is not perpetual.) Thus previous estimates of the long-term impacts of early employment experience may be severely biased. One other feature of the equation should be noted. The equation includes *all* Xs between year 2 and year t. Exclusion of these is yet another source of potential bias.

Yet even this rather complicated model leaves much to be desired. Human capital theories suggest that persons may select differently shaped profiles. Persons with early unemployment and nonemployment may have flatter schedules. Blue-collar workers have slower wage growth than their white-collar counterparts. If the return to experience is systematically lower for persons lacking some early work experience, the coefficient will be further biased upward. Similarly, the individual components may not be stationary over time, introducing even more bias.

Even ignoring the inadequacies with the current model, however, it is virtually impossible to get consistent estimates of the coefficient on weeks worked in the first year. Simple differencing does not eliminate the heterogeneity components since the coefficients on all are changing over time. Equally troubling, WW_{i1} is fixed over time. Differencing yields only the change in its coefficient, not its overall magnitude. The only hope for estimation is to find an instrument correlated with WW_{i1} but partially uncorrelated with δ_i, Ψ_i, or λ_i. One such instrument might be the area unemployment rate in year 1. It is not currently in the equations and the inclusion of race and residence dummies along with schooling may eliminate most of its correlation with the individual effects. Unfortunately, we have already seen that the area rate performed poorly in weeks-worked equations. Thus it is an unlikely instrument.

Although isolation of the full long-term impact of nonemployment in this data set is infeasible then, a more modest attempt can be made to isolate the impact of heterogeneity. Let us concentrate solely on equation (1), the regression of log wages on an individual constant and weeks worked in previous years. If we treat weeks worked in each year as exogenous, then simple differencing eliminates the nuisance parameter and leaves the last weeks-worked parameter intact. Thus

$$(1) \qquad LW_{it} = X_{it}\beta'_t + \sum_{j=1}^{t-1} \alpha_{t_{i}-j} WW_{i_{t}-j} + \lambda_i + \varepsilon_{it}$$

$$(1') \qquad LW_{it} - LW_{it-1} = X_{it}\beta'_t - X_{it-1}\beta'_{t-1} + \sum_{j=2}^{t-1} (\alpha_{ti-j} - \alpha_{t-1t-j})\, WW_{it-j} + \alpha_{tt-1}\, WW_{it-1} + \varepsilon_{it} - \varepsilon_{it-1}$$

As long as the weeks worked are strictly exogenous, α_{tt-1}, the coefficient on the weeks worked in year $t-1$ represents its impact in that year. One can also difference wages separated by two years. In that case, the coefficients on the last two years of experience could be captured.

The exogeneity assumption, however, is highly suspect. Even if we assume that WW_{t-1} was uncorrelated with ε_{it}, the presence of LW_{t-1} in the labor supply equation determining WW_{t-1} guarantees that $Cov(WW_{it-1}, \varepsilon_{it-1}) > 0$. OLS estimates of the difference equation will then understate the true impact of WW_t on wages. In the previous labor supply results the coefficient on LW_{t-1} was often small, occasionally of wrong sign, and invariably insignificant. Still, without stronger evidence of exogeneity, we must be concerned that OLS estimates will be biased.

There are two reasonable approaches to this problem. First, Sims (1972) has suggested a very simple methodology to test for exogeneity: simply regress the dependent variable on all past and future values of the independent variable. Strict exogeneity in the absence of heterogeneity implies that the coefficient on future values will be zero; those on past values, nonzero. If causality is unidirectional, past values of the independent variable will influence the dependent variable, but the current dependent variable will not influence future values of the independent variables. Unfortunately, even if the independent variable is strictly exogenous, in the presence of heterogeneity the expectation of the future coefficients will be nonzero if the future values are correlated with any part of the heterogeneity not captured by other variables in the equation (see Chamberlain [1979]). The common-sense notion is that any variable partially correlated with an omitted stationary heterogeneity term will have a nonzero coefficient even in equations where the variable would otherwise have a zero coefficient, because it will be serving as a proxy for the omitted variable. If weeks worked in year 2 is capturing heterogeneity in the year 2 wage equation, it ought to capture the same heterogeneity in year 1. Essentially, Sims's is a test for true casuality as opposed to spurious correlation due to endogeneity or omitted variables.

If, as seems likely, the Sims test fails, we are forced to seek an instrument for WW_{it-1} in equation (1′). If we assume that impact work experience in some year j raises wages in years $t-1$ and t by a equal amount, $\alpha_{tj} = \alpha_{t-1j}$ and we can withdraw WW_{ij} from the equation and use it to instrument WW_{it}. WW_{it-2} for instance, might serve as an effective instrument.

Many authors have previously sought to remove heterogeneity or "ability" bias from wage equations (see, for example, Chamberlain [1978a], Griliches and Mason [1972]). These efforts typically were not aimed at deriving the coefficient on work experience as distinct from age, nor did they focus particularly on the very early years of experience. Nonetheless it would be surprising in light of all the previous efforts if we did not find a substantial effect of work experience on wages.

10.3.1 Empirical Results in Wage Equations

To roughly replicate previous studies of the effects of unemployment on wages, wage equations were first estimated for 1975 and 1973 with no experience variables included other than weeks worked in the first year. The data base was the same sample of young men who left high school in 1965 to 1967. The results were similar to those reported by other authors. The coefficient on WW_1 was .00452 on 1975 and .00478 in 1973. Both coefficients were quite significant. If the values actually reflect the true effect of early nonemployment on future wages, the impact is staggering. Youngsters missing out on twenty-six weeks of employment experience in their first year out of school are left with 12% lower wages even ten years later. Accumulated over a lifetime, the cost could be enormous. These results are not purged of heterogeneity, of course. The large size of the possible losses thus makes the separation of the true impact quite important.

At the very least, the results do show dramatic persistence in wages for persons with early time not employed. Even if nonemployment had no important impact of its own, early unemployment can be used to single out persons who will do poorly in the future. They could be the recipients of special aid. The result is also important because it suggests that early experience could be used as a signal of "quality" or "ability" by employers. This is not to say that employers in 1975 would have looked at what happened in 1966, but employers in 1967 or 1968 might have, and employers in 1969 might have looked back to 1968, and so forth. In a market with great uncertainty, those persons who genuinely tried but failed to get work may be inadvertently classed as poor workers. It may take these workers some real time to recover from this early adverse signal.

The issue at hand, however, is whether this early experience or lack thereof actually has ill effects. The previous section described why the only possible hope of capturing the very long-term effects was with an effective instrument on WW_1. The area unemployment rate in year 1 was suggested. As expected, however, instrumental variable equations behaved poorly. The results were erratic; standard errors very high. Thus I chose to focus more narrowly on the effects of experience in the first four years of experience.

Table 10.11 presents regression results of wages at the end of each of the first four full years out of school as a function of weeks worked in previous years. These were estimated as seemingly unrelated equations

Table 10.11 Wage Equations for the First Four Years out of School

	Dependent variables			
	$LWAGE_2$ $(t=2)$	$LWAGE_3$ $(t=3)$	$LWAGE_4$ $(t=4)$	$LWAGE_5$ $(t=5)$
SCHOOL	.040	.051	.046	.060
	(.017)	(.014)	(.015)	(.014)
AGE_2	.040	.038	.018	.027
	(.017)	(.011)	(.012)	(.011)
BLACK	−.114	−.125	−.124	−.070
	(.053)	(.045)	(.048)	(.045)
$SMSA_t$	.135	.145	.171	.138
	(.048)	(.039)	(.041)	(.038)
$SOUTH_t$	−.275	−.218	−.197	−.264
	(.055)	(.045)	(.047)	(.044)
MAR_t	.078	.105	.078	.085
	(.046)	(.033)	(.035)	(.034)
$AREA_t$	.010	.005	−.003	−.012
	(.013)	(.009)	(.008)	(.007)
WW_1	.0030	.0036	.0034	.0049
	(.0019)	(.0017)	(.0019)	(.0017)
WW_2	—	.0028	.0035	.0010
		(.0018)	(.0021)	(.0020)
WW_3	—	—	.0043	.0019
			(.0020)	(.0022)
WW_4	—	—	—	.0017
				(.0017)
INTERCEPT	−.675	−.742	−.433	−.487
	(.258)	(.221)	(.237)	(.226)

NOTE: All equations estimated as seemingly unrelated equations.

since the error terms will almost certainly be correlated. With only 271 observations, the results are plagued by rather high standard errors. Nonetheless, the coefficients on past weeks worked are quite sizable. Furthermore, the results seem quite stable until year 4 when collinearity seems to be excessive. The numbers suggest that each year of experience is associated with a 10 to 20% wage increase in these first four years. Although reserving some concern for the low significance of some estimates, I shall concentrate on determining whether these high point estimates appear to be the result of heterogeneity or state dependence.

The Sims test for true causality is to include future work experience in current wage equations. Strict exogeneity implies zero coefficients on future variables so that the coefficients on WW_2, WW_3, and WW_4 would be zero in the LW_2 regression; WW_3 and WW_4 in the LW_3 regression, and so forth. (Recall that LW_t is wage at the beginning of year t or end of year $t-1$). Table 10.12 displays wage equations for years 2, 3, and 4, when weeks worked in years 1 to 4 are included in each regression. The results are striking. In spite of a high degree of multicollinearity, in each of the equations the coefficients on past experiences remain strongly positive. The coefficients on future experience tend to be small or of incorrect sign. Incredibly, neither endogeneity nor heterogeneity may seriously bias the coefficients on WW_2, WW_3 or WW_4. A likelihood ratio test that the coefficients on future values are zero is not rejected. Twice the natural log of the likelihood ratio is 7.7, while the critical value of χ^2 (6) is 12.6. A similar test that the coefficients on past values are zero is overwhelmingly rejected (likelihood ratio = 126.3).

This evidence for the one-way causality of weeks worked on wages is quite surprising, although the very weak performance of the wage variables in the labor supply equation portended this exogeneity. The minimal bias resulting from heterogeneity is perhaps even more remarkable.

Table 10.12 Wage Equations with Weeks Worked in First Four Years Included in All Regressions[a]

	Dependent variables		
	$LWAGE_2$ ($t=2$)	$LWAGE_3$ ($t=3$)	$LWAGE_4$ ($t=4$)
*WW*1	.0031	.0036	.0034
	(.0021)	(.0018)	(.0019)
*WW*2	−.0005	.0025	.0032
	(.0026)	(.0022)	(.0023)
*WW*3	.0014	.0014	.0047
	(.0031)	(.0026)	(.0028)
*WW*4	−.0019	−.0015	.0009
	(.0026)	(.0022)	(.0024)

[a]All equations include $SCHOOL$, AGE_t, $BLACK$, $SMSA_t$, $SOUTH_t$, MAR_t, $AREA_t$. All equations estimated as seemingly unrelated equations.

It should be remembered though, that these results in no way indicate that heterogeneity is absent. They show instead that the portion of heterogeneity correlated with WW_2, WW_3 and WW_4 is fully captured by WW_1, *SCHOOL, AGE,* and the other controls. The coefficients on these latter variables are presumably biased by the presence of heterogeneity.

The very powerful conclusion from this exercise is that, at least in these four years, the coefficients are a good reflection of the causal relationship between experience and wages. Not surprisingly, the difference results confirm these findings. Differencing eliminates any stationary effects correlated with weeks worked. If heterogeneity were a serious problem, we should expect the coefficients on work experience accumulated between the differenced years' wages to fall. At the same time, endogeneity would induce a negative correlation between this experience and the error term, thus causing a further fall.

Since the coefficients in year 4 showed that multicollinearity may be excessive, I will concentrate on the first three years' wage equations. (The results for year 4 are quite similar.) Table 10.13 presents the estimated coefficients in three difference equations. In the first column, first-year wages are subtracted from those of the second year. The second column presents results of the regressions on the difference in wages between years 2 and 3. The final column provides differences between years 3 and 1. Once again, the data strongly suggest that heterogeneity and endogeneity are relatively small parts of the measured association between experience and wages in the second and third years. The impact of weeks worked in year 1 is neutralized in all of the difference equations, as would be predicted, since the coefficient represents the difference in the effects of experience on wages in two future years. The coefficient on weeks worked in the second year is effectively zero in the second equation, again as predicted. However, the coefficients on weeks worked in the second and third years in equations where those effects were not differenced out remain quite large. The coefficients are much more stable across equations than they were in table 10.11. Their magnitude is, if anything, greater and their significance is increased. The results are thus highly supportive of a causal relationship between experience and wages. The increase in the significance is reassuring that the effects of experience are not purely spurious.

One possible problem may be that we have tested the wrong model. Jobs with the highest *wage growth* may have very stable employment requirements. This model would imply that if a Sims-type test were performed using the change in wages on the left hand side, future weeks worked enter significantly since workers would presumably remain with their jobs. Note also that past weeks worked would likely enter significantly since there is a good chance that persons with good jobs now, as measured by wage growth, had them in the previous year. Neither result

Table 10.13 Differenced Wage Equations[a]

	Dependent variables $LWAGE_3-LWAGE_2$ $(t_1=3, t_2=2)$	$LWAGE_4-LWAGE_3$ $(t_1=4, t_2=3)$	$LWAGE_4-LWAGE_2$ $(t_1=4, t_2=2)$
WW_1	.0002	−.0001	.0002
	(.0019)	(.0016)	(.0020)
WW_2	.0035	.0006	.0040
	(.0022)	(.0020)	(.0025)
WW_3	—	.0041	.0040
		(.0021)	(.0021)

[a]All equations include $SCHOOL$, AGE_2, $BLACK$, $SMSA_{t1}$, $SMSA_{t2}$, $SOUTH_{t1}$, $SOUTH_{t2}$, MAR_{t1}, MAR_{t2}, $AREA_{t1}$, $AREA_{t2}$. All equations estimated as seemingly unrelated equations.

was prominent in the data. Moreover, it is quite possible that the largest single year wage changes will be associated with job changes. Presumably, some young men find new jobs offering better pay. The movers probably have fewer weeks worked than the stayers. These persons bias the results downward.

The results presented here strongly suggest that in the first few years out of school, experience increases wages by as much as 10 to 20% per year. The biggest cost of being out of work therefore may well be the wages. These data do not reveal whether this is the result of the accumulation of general or specific human capital or even if they merely reflect signaling. Nor do they reveal what skills might be gained from early experience. They do reveal, however, that lost work experience really can be quite costly.

These data do not allow good tests for a catch-up effect. It is possible that the loss in wages due to previously lost experience is compensated for when the individual finally gets a steady job. Interaction terms simply make the results unstable. This is an important possibility which merits attention in future work.

The results here imply that early experience increases wages by 10 to 20%. I regard these wage equations as preliminary results requiring verification from other sources. Still, they provide surprisingly strong evidence that, at least in the short run, work experience really does make a difference. Just how long the effect persists requires another analyses. Ultimately, the final conclusion awaits the availability of a good area unemployment rate measure so that WW_1 can be properly instrumented.

10.4 Conclusion: Permanent Scars or Temporary Blemishes?

The first part of this paper examined the early pattern of labor market performance of young men. Several important conclusions were made.

- The early years of labor market experience are times of substantial change. Employment rates rise, as do participation rates. There is considerable evidence of weak labor force attachment early in many young men's careers.

- Although the distinction between time out of the labor force and time unemployed is conceptually appealing, the division is not accurately captured in these retrospective data. Unemployment rates behave erratically over time for this group. All of the results in this chapter suggest that time not employed is a far better measure of the labor market performance of young men.

- Even though there is a general improvement in employment rates for these young men over time, early labor market patterns persist. Young men with poor records early will typically have comparatively poor records later.

The next section revealed that much of the persistence in employment patterns could be directly attributed to heterogeneity.

- Controls for heterogeneity eliminate at least two-thirds of the observed persistence in employment, but evidence of experience dependence remains. That is, even controlling for individual differences in the propensity to work, experience dependence remains. However, the absolute magnitude of the effect is small. Even a six-month spell out of work tends to generate only an additional three to four weeks out of work one year later. There is no evidence in these data that time out of work sets off a long term cycle of recurring "nonemployment."

Finally, the effect of work experience on wages was examined. Apparently, neither heterogeneity nor endogeneity induce important biases in the estimated impact of work experience in the second, third, and fourth years out of school on the wages of youngsters in the first few years afterward. The impact of early experience on wages is quite large.

- Early work experience has a sizable impact on wages. Controlling for individual effects, experience in the second, third, or fourth year out of school tends to be associated with wage increases of between 10 and 20% a year.

The data did not allow testing for the possibility of catch-up, nor for testing how long these wage differentials persist.

There is a strong asymmetry in the problem of isolating the real effects of early labor market experience on future employment and wages from the differences in wages and employment that are the natural result of differences among people within the labor market. There are many reasons for expecting that unobserved differences among people will be correlated both with employment and wages. Thus a finding suggesting that early experience has real impact is always suspect. On the other hand, a finding of no impact is considered quite convincing since the deck was stacked against such a conclusion. The results in this chapter lead me to the former more suspect finding. Early experience really does seem to make a difference, particularly on wages. Even after rather elaborate controls for heterogeneity, both wages and labor supply seem to be directly related to past work experience in the short run, although the effects on labor supply are quite small.

As with all research, many caveats remain. This research was conducted on a small select sample in a period of tight labor markets, quite unlike the present situation. It may be that these findings are peculiar to this group or this era. No separate analysis has been done for the central city poor. The cleanest experiment—testing whether past unemployment rates predict future wages and employment—could not be performed. The ultimate answer to the question of the long-term impact must await these results. Until such time as high quality local unemployment data are available, we will have to rely on statistical methods of removing heterogeneity.

In this group of young men the heavy cost of time out of work was the impact of the lost work experience on wages. The data do not show whether working generates better work habits, or instills general or firm specific skills, or even just creates positive signals. Policy makers should keep in mind, however, that many forms of public employment may not generate the desirable human capital or worker quality signals. Employers may regard public employment quite differently from private employment. The challenge for public policy is to design aid programs which help young people accumulate the important labor experience, rather than simply provide programs which makes the government the last-resort employer.

Notes

1. Meyer and Wise (chapter 9 of this volume) report similar results for the National Longitudinal Survey of the High School Class of 1972.

2. Actually, ψ_i captures both the experience dependence from period $t-1$ plus the Markov type probability of remaining in state 1. This is of no serious concern if the periods are short. If periods are long, asymmetric definition of periods implies a serious loss of efficiency.

3. Actually, it can be proven that if we assume complete stationarity (exclude all Xs), we can legitimately test the null hypothesis of no state dependence by conditioning on $b_{it-1} = b_{it} = b_{it+1}$.

4. For one analysis of the long-run performance of cohorts entering weak labor markets, see Plantes (1978).

5. See Meyer and Wise (chapter 9) for a treatment of the fifty-two-week truncation problem in the absence of heterogeneity. These authors do not use difference quations.

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Comment Robert J. Willis

Is teenage unemployment a serious social problem? Despite high and rising levels of measured unemployment among teenagers, especially black teenagers, economists appear to have quite varied opinions about the seriousness of the problem. To a considerable degree, I suspect that those economists who are least alarmed about teenage unemployment tend to concentrate on short-run costs that are borne by the teenager. Here the magnitude of the welfare loss of the typical unemployed teenager appears to be modest because of the dominance of short spells, the likelihood of income protection from his family or from welfare programs, and the presumably positive value he attaches to his nonmarket time. Indeed, those who emphasize the supply-side determinants of unemployment might argue that teenage unemployment is high precisely because the costs to the teenager of unemployment are so low.

Of course, it is possible that the short-run social costs of teenage unemployment are much higher than those costs because of moral hazard caused by family or social insurance of the teenager's consumption against the risk of unemployment, or because the high private value of his nonmarket time is due to the gains from illegal activity. Even in these instances, however, it may be argued that teenage unemployment is more a symptom than a cause of social problems whose roots lie in the decline of the family as an agent of social control and the failure of other institutions to replace this function of the family as rapidly as they have replaced its protective functions. Unfortunately, there are as yet insufficient theory and data to judge how important these social welfare losses may be or whether the major part of the explanation for trends and levels of teenage unemployment should be sought in the functioning of the labor market or in changes in the broader social, economic, and institutional context of the society.

Even if teenage unemployment does not impose substantial short-run costs, it has been argued that it may have severe long-term consequences for an individual's labor market success. It is this "scar hypothesis" which is the subject of David T. Ellwood's interesting and important chapter. Previous researchers have found a fairly strong correlation between various measures of an individual's current labor force activity (e.g., unemployment, employment, or wage rate) and his teenage unemployment experience. Ellwood rightly argues that the major statistical issue to be resolved is to determine whether such a relationship represents the *causal* effect of early unemployment on the subsequent labor market success of a given individual or whether it simply reflects a correlation

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induced by persistent unmeasured differences (i.e., population heterogeneity) in labor supply parameters across individuals.

The distinction between correlation and causation is always difficult to establish in nonexperimental data, but is especially difficult in the current context because the dependent variable is the occupancy or duration of stay in a discrete state (e.g., unemployment) and the causal variable of interest is a lagged dependent variable (e.g., unemployment as a teenager). The econometric methodology to deal with such problems is just now being developed. It is to Ellwood's credit that he has not only provided a lucid description and critique of this literature but has also made an imaginative and thoughtful attempt to extend and apply this methodology to deal with an important issue.

In my judgment, Ellwood's work represents a significant advance over previous studies of the length of teenage unemployment, and his major finding that previous studies have substantially overstated these effects by neglecting unmeasured heterogeneity is quite plausible. Unfortunately, Ellwood is forced to use data which do not describe an individual's complete employment history (i.e., data on the dates and durations of each spell of employment, unemployment, and nonemployment since leaving school). In attempting to make his econometric model conform to data limitations, I believe that he may have introduced some confusion on two important issues—the definition of "true" state dependence and the distinction between continuous and discrete time models. I suspect that the model he employs is misspecified (relative to a model using ideal complete event history data), but it is beyond my competence to suggest what the best model specification might be given the available data or to guess what bias may inhere in the model he uses.

The causal effect Ellwood seeks to measure is the change in current employment behavior induced by a previous employment event such as the occurrence or nonoccurrence of teenage unemployment. Following the terminology in the emerging econometrics literature, he uses the term "state dependence" to denote such an effect. However, the two types of state dependence which Ellwood distinguishes in the paper, i.e., "simple Markovian dependence" and "true experience dependence" do not necessarily correspond to the type of state dependence implied by the scar hypothesis.

Simple Markovian dependence is designed to control for the inertia which leads an individual who is currently employed to be more likely than an unemployed individual to be employed a short time later. This control is necessary because, while the dependent variable is continuous (e.g., number of weeks worked during the year), the observation period is in discrete time units of a year. Thus it is misleading for Ellwood to characterize his model as continuous; in fact, I believe it suffers from many of the problems he raises concerning discrete time models applied to a continuous time process such as employment. True experience dependence (i.e., the γ parameter) measures the effect of, for example, the number of weeks worked last year on the number of weeks worked this year, holding constant an unmeasured person-specific labor supply parameter. It is this form of state dependence that Ellwood hopes to measure in order to assess the importance of the scar effect.

I suspect that γ measures a mixture of several "pure" forms of state dependence in the underlying continuous time process, not all of which correspond to the causal effects implied by the scar effect. One pure form of state dependence which has received considerable attention recently in the work of Chamberlain, Mincer, and Heckman is duration dependence. Roughly, duration dependence is present if the conditional probability of leaving a state (e.g., employment or unemployment) varies with the length of time spent in the state (i.e., the hazard function varies with duration). In the employment context, for example, Mincer suggests that the acquisition of firm-specific human capital would lead to a decrease in the probability of leaving a given employer as the length of job tenure increases. In this case, the hazard function for a given employment or job spell is said to be decreasing. If duration dependence is the only form of true state dependence, an individual's probability of exiting from a given spell of employment or unemployment is independent of his employment history prior to entering his current state. Clearly, the scar hypothesis suggests dependence on past history so that it is necessary to consider additional forms of true state dependence beyond pure duration dependence. For example, it may be reasonable to postulate that current probabilities of leaving employment (or unemployment) at given duration depend on the number and/or duration of previous jobs on unemployment spells as is suggested in a recent paper by Heckman and Borjas (1979) that was written after the Ellwood chapter.

The scar hypothesis should probably be elaborated in order to specify more precisely what types of state dependence one seeks to measure. For example, a past history of unemployment might be expected to influence both the level and slope of the hazard function pertaining to a given spell of employment because employers may be reluctant to make job-specific investments in "unreliable" individuals. If this is the case, past unemployment reduces the degree of duration dependence but increases the likelihood of turnover in employment at given duration. Because of data limitations which preclude the identification of separate spells of employment or unemployment, Ellwood's model is unable to distinguish between effects of duration in the current spell and effects of events that took place before the current spell began. His concept of true experience dependence probably captures a mixture of these effects. Despite these drawbacks, due largely to data limitations, Ellwood has written an important chapter, the findings of which deserve to be taken seriously.

Reference

Heckman, J. J. and G. J. Borjas. August 1979. Does unemployment cause future unemployment? Definitions, questions and answers from a continuous time model of heterogeneity and state dependence. Mimeo.

Comment Burt S. Barnow

David Ellwood's chapter provides a significant contribution to the literature on the long-term effects of teenage employment problems. Although the previous papers on this subject cited by Ellwood do a good job of documenting the persistence of employment problems, none does as thorough a job of exploring the causality of the persistence. My comments follow the organization of Ellwood's chapter, covering the data, the descriptive results, theory, and the analytical results.

The National Longitudinal Survey (NLS) of young men is useful for Ellwood's purpose, but it has several limitations. Because there are only 298 observations, small effects may not be detected with statistical significance; this problem is increased by the use of instrumental variable techniques. A more important limitation of the small sample size is that greatly different groups are constrained to have identical coefficients—it is questionable whether the same relationship can be expected to be appropriate for middle-class high school graduates and poor dropouts.

Although Ellwood is careful to point out the limitations of the sample, the reader should keep in mind the fact that the sample used is not representative of all youths. In addition to the omission of youths who attended college and other post-secondary schools, many youths of average intelligence and health were drafted. Thus Ellwood's sample overrepresents those with low intelligence or poor health.

The presentation of the descriptive results, especially the probability trees, is very helpful in tracing the labor force experience of the sample. For example, although only 18% of the sample worked throughout the four years and only 39% were always employed in three or four of the years, we also find that 47% of the sample experienced no unemployment over the four years and 70% were unemployed in one or none of the four years. This suggests that unemployment and nonemployment are different phenomena. At several points Ellwood notes that unemployment appears to be a less stable variation than nonemployment. But it is worth noting from table 10.3 that unemployment status in year 1 is a better

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predictor of unemployment status in year 4 than nonemployment status in year 1 is of nonemployment status in year 4.

Ellwood's theoretical section is excellent in its consideration of the concept of state dependence. The distinctions between what he refers to as Markov-type dependence and experience dependence are important from both a theoretical perspective and a policy perspective. What is troubling in the theory section is the lack of economic theory for identifying the variables and the functional forms of the models. For example, Ellwood uses the variable δ_{it} to represent unobservable individual variables. If δ_{it} represents I.Q., then the assumption $\delta_{it} = \delta_i$ is fine, but if δ_{it} represents motivation, then such an assumption is inappropriate. A possible extension of the model is to include interactions of the state dependence and X or δ' variables.

The emprical section of the paper contains some very interesting findings. The finding that an extended period of nonemployment has a small effect on future employment but a relatively large effect on wages is consistent with economic theory, but as Ellwood points out one cannot tell if the wage effects are due to human capital accumulation or signaling. However, it is important to note Ellwood's point that weeks worked is a truncated variable while the wage variable is not, and failure to correct for this will tend to understate the effects of the independent variables. Because of the complexity of Ellwood's models, a mechanism for correcting this potential bias would be extremely difficult to develop, but it is possible that bias in the estimates of the effects on weeks worked is present.

A second problem is that Ellwood's models require the use of many instrumental variables. Some of the instruments are lagged dependent variables, and if there is serial correlation of the error terms, the estimates of the coefficients will be inconsistent; note that in the Myer and Wise chapter in this volume (9), significant serial correlation for a similar data base was detected.

None of these comments is intended to detract from this fine chapter. Ellwood has taken a topic of great importance and applied some highly sophisticated techniques to isolate the parameters of interest. I am sure that this chapter will stimulate additional work in this area.