NBER WORKING PAPER SERIES

TEENAGE UNEMPLOYMENT: PERMANENT SCARS OR TEMPORARY BLEMISHES

David T. Ellwood

Working Paper No. 399

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge MA 02138

October 1979

This paper was prepared for the NBER Conference on Youth Unemployment, May, 1979, and will be included in a conference volume. I am extremely grateful for discussions with Richard Freeman, Gary Chamberlain, Robert Meyer, Lawrence Summers, and David Wise. I received helpful insights from participants in the Columbia University Labor Economic Seminar. Research for this paper was funded in part by a grant from the Department of Labor to the National Bureau of Economic Research. Any opinions expressed are those of the author and not those of the National Bureau of Economic Research.

NBER Working Paper 399
September 1979

Teenage Unemployment: Permanent Scars or Temporary Blemishes

ABSTRACT

This paper examines the persistence and long-term impacts of early labor force experiences. The paper reports a rise in employment rates for a cohort of young men as they age, but points out that those persons with poor employment records early have comparatively poor records later. In order to asses the extent to which differences in later employment and wages are causally related to these earlier employment experiences, the methodologies of Heckman, Chamberlain, and others are extended to account for Markov type persistence and a straight forward estimation technique results. In addition, a Sims type causality test is used to measure the true impact of work experience on wages.

The paper concludes 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 be reflected in considerably lower wages. At the same time, the data provide no evidence that early unemployment sets off a vicious cycle of recurrent unemployment. The reduced employment effects die off very quickly. What appears to persist are effects of lost work experience on wages.

David T. Ellwood
National Bureau of Economic Research
1050 Massachusetts Avenue
Cambridge, Massachusetts 02138

617/868-3913

Teenage unemployment poses a puzzle for economists. Its causes and consequences are not well understood because 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 paper 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 which are 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 paper.

This paper 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 which trace the experience of a complete cohort over four years. In most other work the high rates of attrition and re-entrance into the sample over the period at least open the possibility of distorting the underlying pattern.

The second section extends the 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 appears 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 more common labor theories. Perhaps most prominent in its prediction of long-term effects is human capital theory. While the theory isn't 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 permanently 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 which 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 counter-cyclical. 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 attachment 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 sizeable 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.

It is the conclusion of this paper 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 paper, I will concentrate only on those persons out of school. I see much 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 8 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 14 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 percent, an unemployment rate of 7.1 percent and an employment rate of 78.2 percent. Persons who remained in the sample had rates of 86.1 percent, 5.0 percent, and 81.8 percent 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.* 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 (1979b). The sample selection and CPS comparison suggests 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 under-represent 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

---

* Meyer & Wise (1979) report similar results for the National Longitudinal Survey of the High School Class of 1972.

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 dropout.

I. THE EARLY LABOR MARKET EXPERIENCE

The labor market position of young men improves dramatically during the first four years out of school. Table 1 shows that while an average of nearly 20 percent are without work in the first year, only 10 percent are not working 3 years later. Labor force participation rates rise precipitously, from 86 percent to 95 percent. 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 occured. Almost immediately, however, the unemployment rate shows up as a questionable indicator fo 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.

Table 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 13 Years of Schooling

	Unemployment Rate*	Employment Rate**	Labor Force Participation Rate***
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

* average weeks unemployed/average weeks in labor force

** average weeks employed/52

*** average week in labor force/52

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 percent of all young men in this sample have four-year employment histories unmarred by a spell out of work. Table 2 shows that nearly 40 percent of all young men spend time out of the labor force in their first year, while just over one quarter report unemployment. Overall, 57 percent 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 percent, almost 40 percent spend some time not employed. And while the labor force participation rate is hovering at 95 percent 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 percent of the sample self report time spent neither working nor looking in the first years. These 40 percent report average spells of 18 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

Table 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

unemployed at all during that first full 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 percent unemployment rate. The sample selection rules, which appear to discriminate against the non-employed, 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 career young men spend a great deal of time without work. By their forth 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.

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 1, 2, and 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 1 indicates that unemployment or non-employment was experienced in the period, a 0 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 parenthesis 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 1, 53.1 percent of persons who had been unemployed in their first year were unemployed in their second year. 14.3 percent 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 (non-employment) in the second period conditional on first period spells is .531 (.631), while those who escaped early problems have only a .180 (.354) probability

Figure 1: Probability Tree of

Weeks Unemployed in First Four Full Years Out of School

N = 364

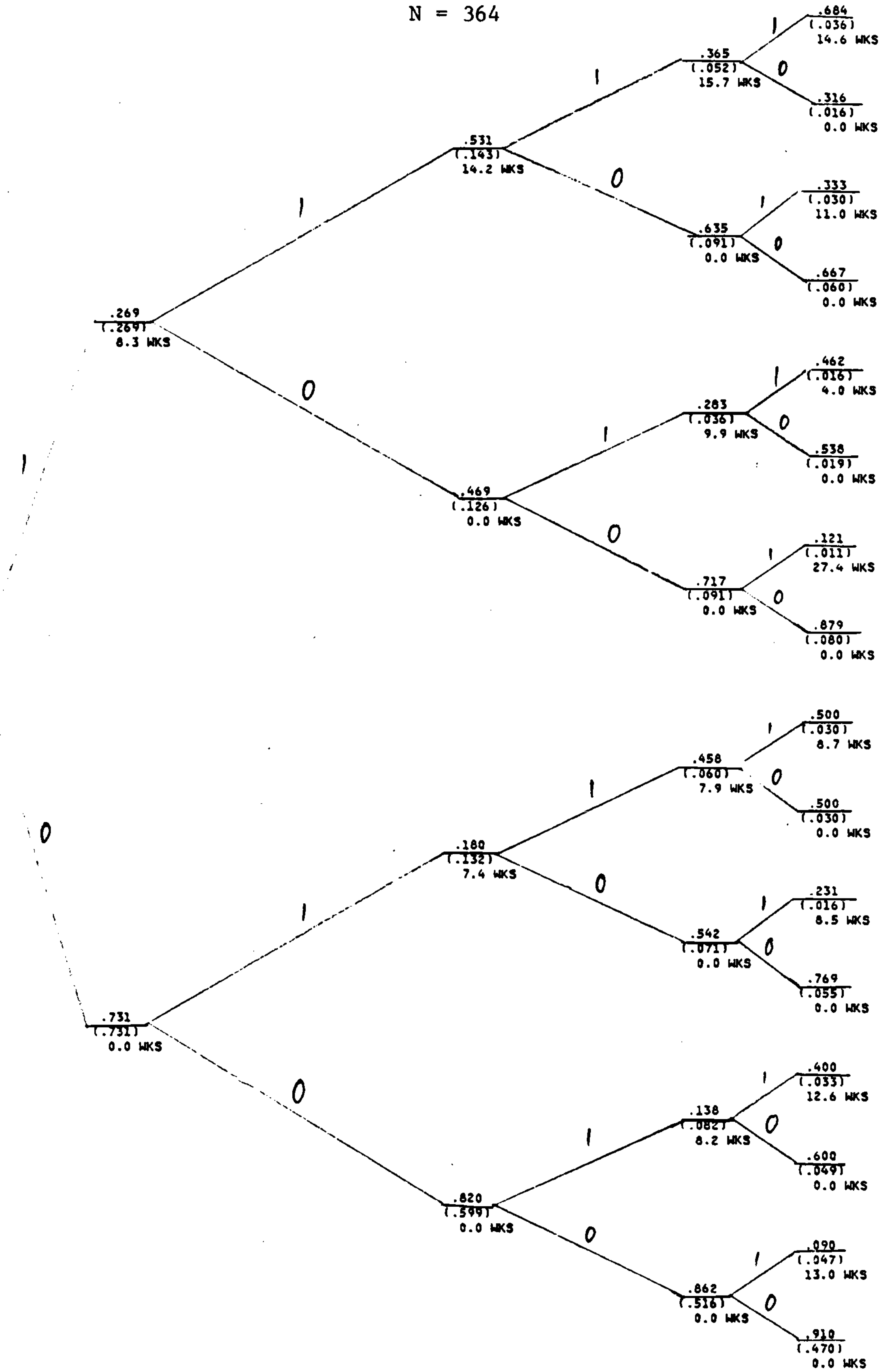


Figure 2: Probability Tree of

Weeks Out of the Labor Force in First Four Full Years Out of School

N = 364

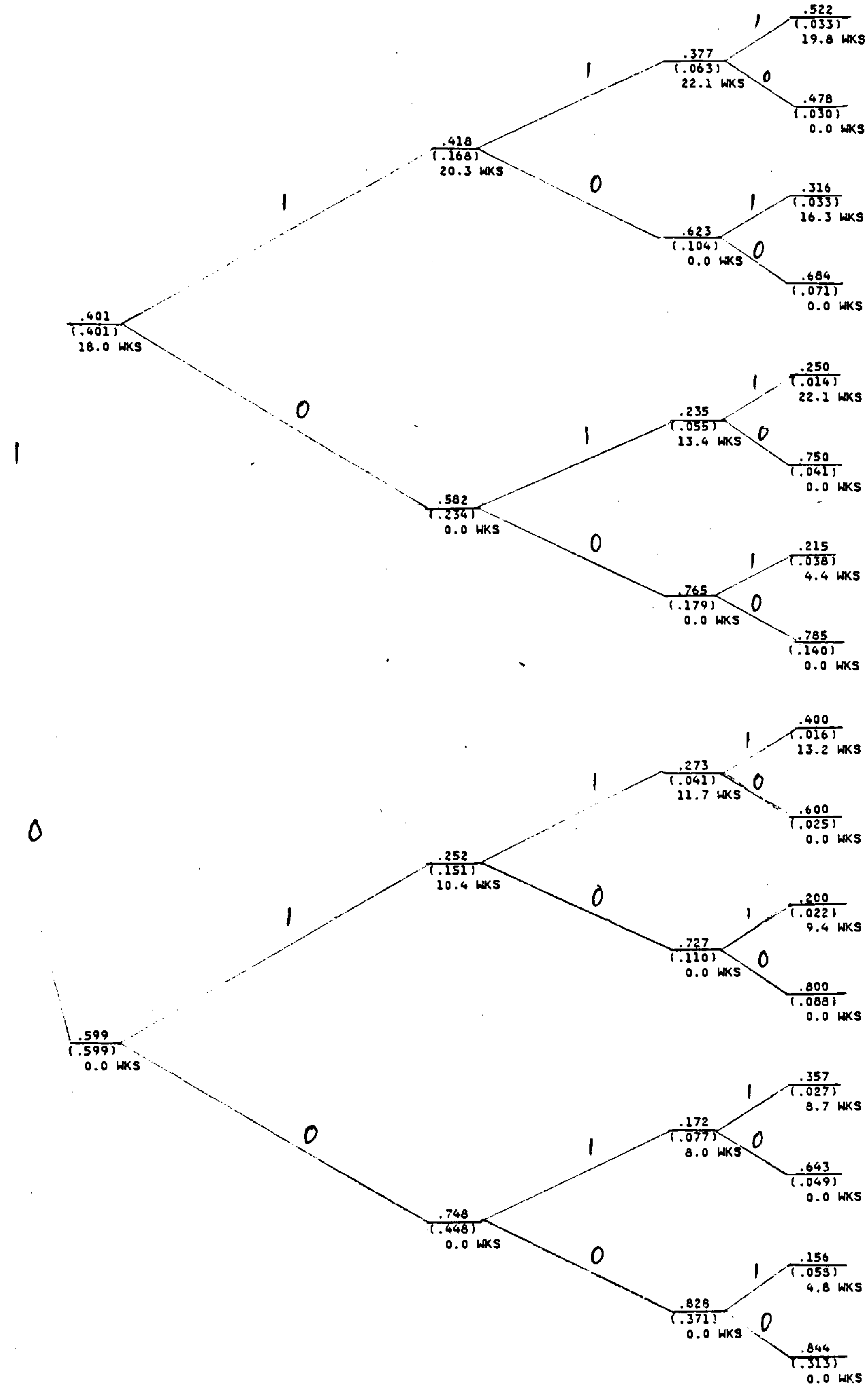
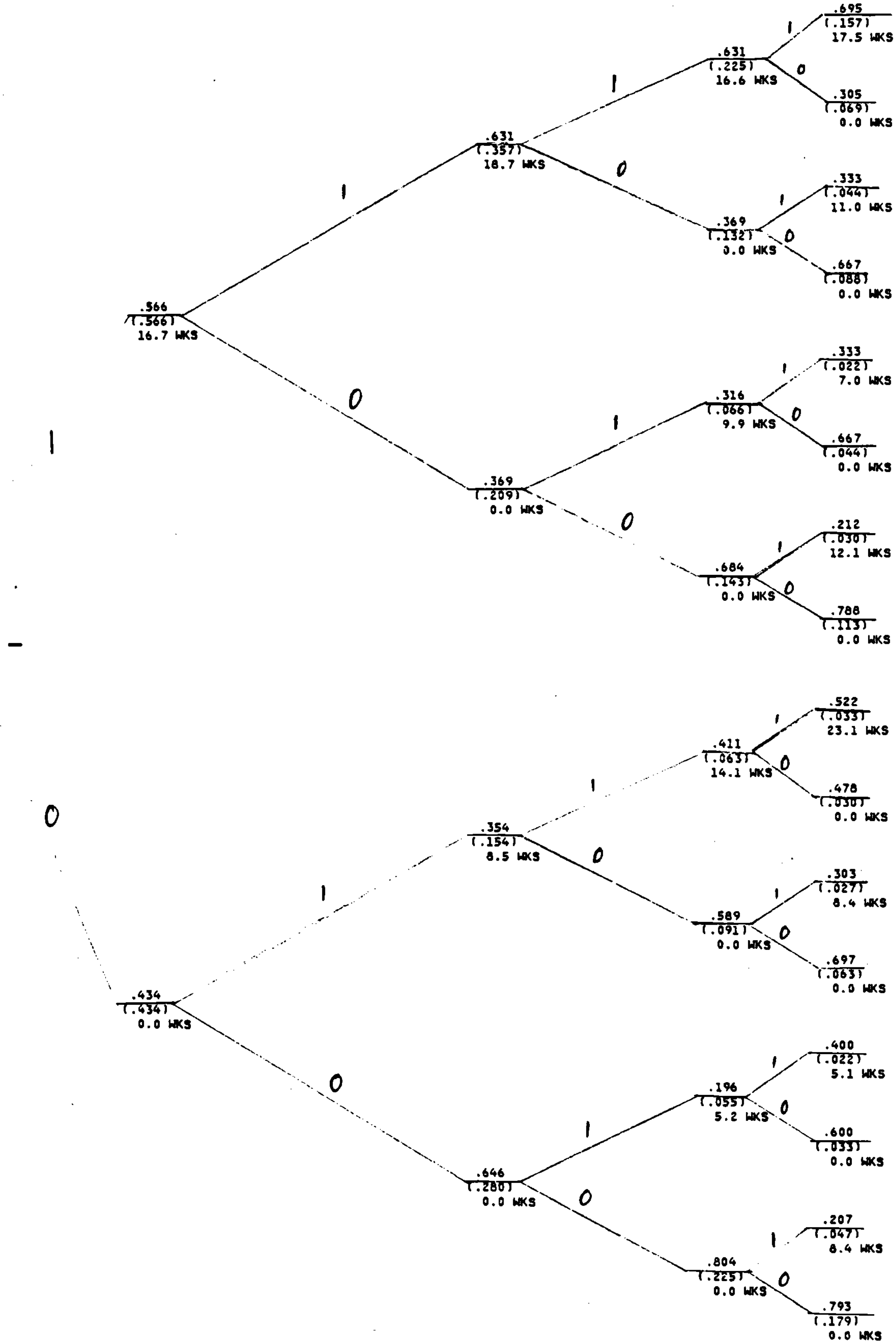


Figure 3: Probability Tree of

Weeks Not Employed in First Four Full Years Out of School

N = 364



of unemployment (non-employment). By the fourth period, boys with three straight years with unemployment are 7 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 10 weeks, and if spells are distributed randomly throughout the year, then 20 percent 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 3 reveals 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 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

Table 3

Probabliity 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 0)	.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 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

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 year (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 reinforces once again the earlier concerns about the quality of unemployment measure (at least this retrospective measure) for this age group.

Both the unemployment and non-employment 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 non-employment are not events randomly distributed over this population of young men. If early unemployment or non-employment is nothing more than search and matching of workers and jobs, then for some at least, the process is quite protracted. Since adverse employment patterns are a problem of a sub-class of youngsters, programs to aid them ought to be targeted to those with early problems.

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

II. 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 in 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 X's.

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 provides 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 1's, persons who do not receive 0's. Thus to be in a state, one need experience only one week of non-employment, but to be out of the state one need experience 52 weeks of employment. If we simply re-define state 1 as 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 20-25% 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 just lost efficiency. They imply peculiar results. The problem of overlapping spells is particularly troubling in the current treatment. If spells last an average of 13 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 asymetry 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 non-technical discussion in an attempt to clarify some of the concepts. For a more technical treatment see Ellwood and Summers (in preparation) and 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 <u>which may be quite independent of his past history of spells or states</u>.

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. Macro-economic 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 <u>entering</u> the 1 state; ψ_i, the probability of <u>remaining</u> 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 the current state alter the probability of entering or remaining in a state. Thus coefficients on d_{it-2}, d_{it-3}... are non-zero. The γ_{t-j} here capture this experience dependence.*

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 have over-estimated 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,

---

* 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.

the γ_{t-j} also capture 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. Last year's weeks worked helps predict the person's state at the end of that year and therefore at the start of the current year. Anyone who worked 52 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 is 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 <u>beginning</u> 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.* 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 $\text{Cov}(U_{it}, U_{it-1}) = 0$. If any of these restrictions are false, spurious

---

* 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 $b_{it-1} = b_{it} = b_{it+1}$.

state dependence can be generated. Probably the most serious concern for this group is non-stationarity 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

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

Substituting into the equation for Y_{i4}

$$Y_{i4} = \lambda_4 \psi_i \, (b_{i4} - b_{i3}) + (\gamma_4 + \frac{\lambda 4}{\lambda 3}) \, Y_{i3} - \frac{\lambda 4}{\lambda 3} \gamma_3 Y_{i2} +$$

$$X_{i4} \, \beta_4 - \frac{\lambda 4}{\lambda 3} X_{i3} \beta_3 + U_{i4} - \frac{\lambda 4}{\lambda_3} U_{i3}$$

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 $\frac{\lambda 4}{\lambda_3}$ although we cannot tell which is which since they enter the equation symetrically.

The restriction $\mathrm{Cov}(U_{it}, U_{it-1}) = 0$ helps to highlight an important distinction between state dependence and serial correlation. In the absence of strong X's which change over time, there is no meaningful empirical distinction between serial correlation and state dependence. However, in the presence of X's 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 non-stationarity of individual specific constant. The only reasonable approach I can see is to assume that both serial correlation and non-stationarity are captured using a time specific coefficient on the individual effect. These models then were used to estimate the long run effects of unemployment.

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 12-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 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 macro policy.*

---

* For one analysis of the long-run performance of cohorts entering weak labor markets see Plantes (1968).

The techniques described in the previous section were applied to weeks worked and to weeks unemployed. Weeks worked was chosen over weeks not worked only because it seems conceptually easier to deal with. Obviously since weeks not worked is simply 52 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 52 nor fall below zero. The importance of the problem is most evident in the case of weeks worked. As weeks worked approach 52 the estimate of state dependence will approach zero if controls are made for heterogeneity. Statistically the limited variable will induce an artificially negative correlation between once lagged weeks and the error term. The result 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 approaches 52, regardless of the true strength of state dependence, the next years' weeks cannot be pushed above 52. This problem is of greater concern in later years when more and more of the young men approach 52 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 52 weeks. Actually, persons who remain at 52 weeks in all three years impart no bias at all in the absence of exogenous variables. They simply provide no information since $y_{it} - y_{it-1} = 0$.*

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 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.

---

* See Myer & Wise (1979) for a treatment of the 52-week truncation problem in the absence of heterogeneity. These authors do not use difference equations.

Table 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 = non-white)
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 6

Means & Standard Deviations for Variables Used in Regressions

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

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

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 26-week spell of unemployment would induce less 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 8 and 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 respectively. 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.

Table 8

Difference Equation Results for Weeks Unemployed

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

Method and dependent variable as column headings.

Standard errors in parentheses.

All equations include year dummies, AGE2, BLACK, and SCHOOL.

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

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

Table 9

Difference Equation Results for Weeks Worked

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

Standard errors in parentheses.

All equations include year dummies, AGE2, BLACK, and SCHOOL.

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

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

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 non-stationarity or serial correlation then, persistence of unemployment -- as distinguished from non-employment -- can be entirely attributed to heterogeneity not 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 30 weeks one year will work an additional 4 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 an additional 5 weeks more in that year than if they had been out of 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 constrast 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. Non-employment begets non-employment. 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 as 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 non-stationarity, 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 which don't control for heterogeneity (Table 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 2 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.

Non-stationarity 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 non-stationarity requires four years of data. Thus, non-stationarity can only be tested between the third and fourth year.

Table 10 presents the results for weeks unemployed and weeks worked designed to isolate the effects of non-stationarity 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 non-stationarity parameter (ratio of the individual effects in year 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.

Table 10

Instrumental Variable Equations Allowing

Non-Stationarity of Individual Component

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

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.

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 30 weeks this year will perhaps work an extra 4 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 30 weeks out of work has 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 non-employment exhibits short-term state dependence quite compelling. There are, however, three important caveats. First, this evidence is from 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 has 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 non-employed may have been excluded from the sample. These persons may gain and lose more from being in or out or 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 non-employment has real but short lived adverse effects on teenage employment prospects.

III. 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 multi-equation system. Let LW_{it} be the natural log of wages of individual i <u>at the start of</u> 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:

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

$$(2) \quad WW_{it} = X_{it}\beta_t + \gamma_t WW_{it-1} + W_t LW_{it} + \delta_{it} + \Psi_{it} b_{it} + U_{it}$$

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 straight-forward 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 52 weeks 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 <u>all</u> X's 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 persons may select different shaped profiles. Persons with early unemployment and non-employment 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 non-employment 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,

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

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

$$\alpha_{tt-1}\, WW_{it-1} + \varepsilon_{it} - \varepsilon_{it-1}$$

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 $Cov(WW_{t-1}, \varepsilon_{it-1}) > 0$. 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, non-zero. If causality is uni-directional, 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 non-zero 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 non-zero 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, Sim's is a test for true causality as opposed to spurious correlation due to endogeneity or omitted variables.

If, as seems likely, the Sim's 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.

<u>Empirical Results in Wage Equations</u>

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 reflected the true effect of early non-employment on future wages, the impact is staggering. Youngsters missing out on 26 weeks employment experience in their first year out of school are left with 12 percent lower wages even ten years later! Cumulated 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 non-employment 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 early experience could be used as a signal of "quality" or "ability" by employers. This is not to say that employers in 1975 look at what happened in 1966, but employers in 1967 or 1968 could. And employers in the next year can look 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 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 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 sizeable. 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 10-20 percent 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

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

All equations estimated as seemingly unrelated equations.

beginning of year t or end of year t-1). Table 12 displays wage equations for years 2,3, and 4, when weeks worked in year 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 $\chi^2(6)$ 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. 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

TABLE 12 : Wage Equations with Weeks Worked

in First Four Years Included in All Regressions*

	- Dependent Variables -		
	$LWAGE_2$ (t = 2)	$LWAGE_3$ (t = 3)	$LWAGE_4$ (t = 4)
WW1	.0031 (.0021)	.0036 (.0018)	.0034 (.0019)
WW2	-.0005 (.0026)	.0025 (.0022)	.0032 (.0023)
WW3	.0014 (.0031)	.0014 (.0026)	.0047 (.0028)
WW4	-.0019 (.0026)	-.0015 (.0022)	.0009 (.0024)

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

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 causing a further fall.

Since the coefficients in year 4 showed that multi-collinearity may be excessive, I will concentrate on the first three years' wage equations. (The results for year four are quite similar.) Table 14 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 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

TABLE 13 : Differenced Wage Equations*

	- 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 (.0019)	-.0001 (.0016)	.0002 (.0020)
WW_2	.0035 (.0022)	.0006 (.0020)	.0040 (.0025)
WW_3	--	.0041 (.0021)	.0040 (.0021)

* 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.

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 was performed using the change in wages on the left hand side, future weeks worked would 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 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 downward bias the results.

The results presented here strongly suggest that in the first few years out of school, experience increases wages by as much as 10-20 percent 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 10-20 percent. 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 other analyses. Ultimately, the final conclusion awaits the availability of a good area unemployment rate measure so that WW_1 can be properly instrumented.

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 arise.

- 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 this retrospective data. Unemployment rates behave erratically over time for this group. All of the results in this paper 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 3 to 4 weeks out of work one year later. There is no evidence in this data that time out of work sets off a long term cycle of recurring "non-employment."

Finally, the effects 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 sizeable impact on wages. Controlling for individual effects, experience in the second, third, or fourth years out of school tends to be associated with wage increases of between 10 and 20 percent a year.

The data did not allow testing for the possibility of catch-up, nor to test 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 in people within the labor market. There are many reasons to expect unobserved differences in 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 paper 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 is 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 unemployemnt 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 does 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 than 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 employer of last resort.

REFERENCES

Becker, E. and S. Hills, 1979, "Teenage Unemployment: Some Evidence of the Long Run Effects", Journal of Human Resources, forthcoming.

Chamberlain, G., 1978, "Omitted Varible Bias in Panel Data: Estimating the Returns to Schooling", Annales de l'INSEE, No. 30-31, April - September 1978.

__________, 1979, "Heterogeneity, Omitted Variable Bias and Duration Dependence", HIER Discussion Paper No. 691.

Clark, K. and L. Summers, 1979, "The Dynamics of Youth Unemployment", NBER Working Paper no. 274.

Ellwood, D. and L. Summers, "Markov Persistence vs. Experience Dependence - A Continuous Time Framework", in preparation.

Grilliches, Z. and W. Mason, 1972, "Education Income and Ability", Journal of Political Economy Vol. 80, No. 3.

Heckman, J., 1978a, "Dummy Endogenous Variables in a Simultaneous Equation System", Econometrica No. 46.

__________, 1978b, "Sample Statistical Models for Discrete Panel Data Developed and Applied to Test the Hypothesis of True State Dependence Against the Hypothesis of Spurious State Dependence", Annales de l'INSEE No. 30-31.

Heckman, J. and R. Willis, 1977, "A Beta-logistic Model for the Analysis of Sequential Labor Force Participation by Married Women", Journal of Political Economy No. 85.

Lillard, L. and R. Willis, 1977, "Dynamic Aspects of Earning Mobility", NBER Working Paper No. 150 (revised).

Meyer, R. and D. Wise, "High School Preparation and Early Labor Force Experience", [this volume].

Plantes, M.K., 1978, Work Experience, Economic Activity and Lifetime Earnings: An Intercohort Analysis, unpublished PhD dissertation, Massachusetts Institute of Technology.

Sims, C., 1972, "Money, Income and Casualty", American Economic Review Vol. 62.

Stevenson, W., 1978, "The Relationship Between Early Work Experience and Future Employability", in Adams, A. and G. Mangum - The Lingering Crisis of Youth Unemployment, Upjohn Institute for Empirical Research.