Volume Title: The Youth Labor Market Problem: Its Nature, Causes, and Consequences

Volume Author/Editor: Richard B. Freeman and David A. Wise, eds.

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-26161-1

Volume URL: http://www.nber.org/books/free82-1

Publication Date: 1982

Chapter Title: The Employment and Wage Consequences of Teenage Women's Nonemployment

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Chapter URL: http://www.nber.org/chapters/c7891

Chapter pages in book: (p. 391 - 426)

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Introduction

Teenage unemployment has risen dramatically in the last few years and has become an increasingly visible national problem, causing widespread concern. However, little is known about its causes and consequences. Some argue that being without work in what should be the early years of one's career does permanent harm by typing an individual as an unreliable worker, thus weakening labor force attachment and depriving him or her of valuable opportunities to invest in work skills. Search theory, on the other hand, suggests that teenage unemployment may be a necessary consequence of the process by which young workers look for the jobs most appropriate to their skills. Others argue that lack of employment in the teenage years is the result either of weak work attachment or (in the case of women) of rational and voluntary decisions to trade off wages and employment for family work.

I propose to examine the ways in which lack of employment during the teenage years affects the employment and wages of women in later years. I will concentrate on perhaps the most serious problem in capturing the consequences of not working, that of separating the differences in employment and wages which are causally related to teenage nonemployment from the differences due to unobserved personal characteristics correlated with it.[1]

Most analyses in this chapter do not separate unemployment time from time out of the labor force. This has the advantage of being comparable with Ellwood's analysis (chapter 10 of this volume). More importantly, while teenage women who are actively seeking work may differ in impor-

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tant ways from those who are not, it is not clear that either retrospective reports of unemployment or the standard CPS unemployment questions allow one to distinguish between these groups. Results reported by Clark and Summers (chapter 7 of this volume) and Ashenfelter (1978) suggest that unemployment statistics fail to capture the labor market activity of young workers adequately, and do not appear to distinguish nonworkers who are seeking work from those who are not. Moreover, unemployment forms a small part of teenage women's reported nonwork time. Understanding how nonwork in the teenage years affects women's later life changes is crucial, whether or not such nonwork is voluntary.

This chapter has three sections. First I describe teenage women's work activity in the years following high school completion. Next I investigate whether early nonemployment reduces women's chances of later employment once we adjust for individual differences that are stable over time and affect employment. Because of data restrictions, this section concentrates on short-run employment effects. In the last section, I estimate the long-run wages costs associated with early nonwork.

This chapter puts forward the following conclusions:

1. Young women's early labor market behavior is quite dynamic. Six out of seven women spent some time working and some time out of work in the four years following school completion.

2. In the four years following school completion, young women's employment rates and participation rates dropped, the duration of nonemployment increased, and the probability that a woman did not work at all doubled. This is the reverse of the pattern observed by Ellwood for young men.

3. Early labor market experiences persisted. Young women with poor early records typically had relatively poor records later. Heterogeneity accounted for a great deal of this persistence. The odds that a woman works given she worked last year were 14.8 times higher than if she did not work last year. Adjustments for heterogeneity halve these odds. Thus, even after controlling for individual differences in women's propensity to work, not working in one year is associated with a much lower probability of working in the next year.

4. There is also evidence that employment effects persist beyond adjacent years. Even given her current work status, a woman's past work history is significantly related to her future work. Because of data restrictions. I could not estimate the magnitude of this relationship or how soon it dies out.

5. Early nonwork involved considerable opportunity costs in the form of lower wages. Ten years after school completion, a woman who spent two years out of work in the years following school completion earned 3 to 5% less per hour than did an otherwise similar woman who had worked

continuously since leaving school. Moreover, for white women, the losses associated with nonwork are greater if that nonwork occurs at the beginning of their careers. This finding must be qualified in two ways. First, at least part of the cost of teenage nonemployment may result from individual differences which are correlated with early nonemployment and later wages. However, controls for differences in women's labor force attachment did not reduce the long-run wage costs associated with teenage nonemployment. Second, some women may be voluntarily electing these costs in order to pursue other goals. But whether voluntary or not, a prolonged period of nonemployment early in one's career is associated with considerably lower wages, even twenty years later. This pattern of long-term reductions in earnings potential is consistent with the results for young men reported by Ellwood and Meyer and Wise.

Nonemployment in women's teenage years is an important policy issue. A sizable proportion of young women reported extended periods of nonemployment in the years following school. Whether voluntary or not, this nonemployment had considerable opportunity costs. It was associated with a lower probability of employment in the short run and with lower wages throughout a woman's work career. Choices made about work and nonwork in the teenage years were clearly important to women's life chances.

The Consequences of Early Nonwork: Theoretical Predictions

Patterns of teenage labor market activity in the years following school completion are quite dynamic, with the majority spending some time unemployed, out-of-the labor force, or both. This continual movement in and out of paid work is more striking for women than for men. Also, women's employment rates, unlike those of men, declined rather than grew in the years following school completion.

Opinions about the consequences of early nonwork vary considerably, with some claiming that it may seriously harm an individual's long-term economic prospects while others argue either that long-run effects are minor or that both any negative consequences associated with early nonwork and early nonwork itself merely reflect unobserved differences in worker quality or in workers' tastes for work. This latter argument has been applied to women in particular since some argue that many young women voluntarily decide to drop out of the labor force because of a preference for home versus market work. Others argue that such "preferences" may be conditioned or encouraged by sex discrimination, either perceived or actual, combined with a shortage of decent jobs.

Both human capital and crowding theories of women's labor market behavior lead to the conclusion that the long-term effects of an early lack of work may be less serious, on average, for women than for men. Human capital theorists stress the importance of early investments in on-the-job-training for men, but argue that a woman's optimal investment strategy may differ (see Mincer and Polachek 1974). Underlying this argument are two assumptions. The first is that many women will choose at some time to withdraw from the work force to meet family responsibilities; the second is that work skills depreciate during such withdrawals. If these assumptions hold, women might choose to defer investments in their future—such as on-the-job training—until after all their expected withdrawals have been completed. This might make the first few years of work less crucial for women than for men.

"Crowding" theorists argue that women tend to be segregated into "female" jobs and that these jobs provide few opportunities for on-the-job training (Bergmann 1971; Stevenson 1973). This job segregation need not be involuntary. Polachek (1975) argues that some women will choose jobs in which future movement into and out of the labor force will not be penalized. If women are disproportionately concentrated in jobs with few training opportunities, then delays in entering the labor market should have few permanent effects on women's careers since most women will not be missing out on valuable investment opportunities.

Even if we accept this reasoning, a number of factors may be operating to increase the harmful effects for women of not working in their early years. As women's participation in the labor force increases and fertility rates decrease, incentives to defer investment and/or to enter occupations which do not penalize them for frequent entries and exits should drop. This should also occur if younger women's perceptions of appropriate sex roles in the labor force and at home are less sex-stereotyped than those of previous generations. Similarly, if equal opportunity and affirmative action policies are widening the range of jobs available to women, then early work behavior might become a more important determinant of women's economic life chances.

Using Panel data, empirical researchers have estimated measures of persistence in the work participation of married women aged 30 to 50, which allow for individual differences (Heckman and Willis 1977; Heckman 1978b, d; Chamberlain 1978c). Cross-sectional analyses of wage determination suggest that periods of nonwork are associated with lower wages for married women aged 30 to 50 (Mincer and Polachek 1974; Corcoran 1979). But analysts have not made such a thorough investigation of the determinants and consequences of teenage nonemployment, particularly among teenage women. Yet economic theories suggest that the teenage years may be important decision years and that decisions about family and work are interrelated. Suppose, for instance, young women respond to a lack of decent jobs by getting married, bearing a child, or by staying home to raise a child; these decisions will in turn shape future work decisions.

Data and Samples

I examined the employment consequences of early nonwork for teenage girls using subsamples of the National Longitudinal Survey (NLS) of Young Women. This is a national sample of 5159 young women between the ages of 14 and 24 who were interviewed annually from 1968 to 1973, and then were interviewed again in 1975. Each year women reported on the past year's labor market experiences. I used this data to track employment experiences for a cohort of young women who left school in 1966, 1967, 1968 and remained out of school for at least four consecutive years. Women still in school were eliminated from analysis since nonwork during school may be less likely to have permanent effects on later employment or wages. Analysis was restricted to women with less than 14 years of schooling in order to avoid confounding the effects of age and education. The group of women aged 15 to 19 who were out of school included dropouts and high school graduates, while a group of women aged 20 to 24 and out of school included college graduates with little experience and high school dropouts with as much as eight years of experience. There is a potential selection bias here since education is a powerful predictor of women's labor supply. Finally, by including only women who reported their work behavior in each of the four years following school, the sample was reduced from 829 to 634 women. This may cause some selection bias problem since the 634 women who reported on their work behavior were apparently more likely to be employed and less likely to be out of the labor force (see table 11.1).

Thus my major sample consists of those 634 young women in the NLS who left school permanently in 1966, 1967, or 1968 with less than 14 years of education and who reported on their work behavior in each of the next four full years. Parallel analyses are also reported for those 401 women in this group who reported work behavior over five full years.

The NLS data show higher employment rates and lower unemployment rates than do the CPS data (see Ellwood and Meyer and Wise). Part of this difference probably reflects differences in respondents; in the NLS, the teenage reports on her work status; in the CPS, a parent reports on the child's work status. Freeman and Medoff (chapter 4 of this volume) show that if we compare reports of parents and teenagers in the same family, the teenagers' reports show more employment and less unemployment than do the parents' reports. However, part of the difference in NLS and CPS statistics could occur because the long-term unemployed are less likely to participate in or remain in longitudinal surveys.[2]

Both the sample selection procedures and the CPS comparison suggest that some women with long-term records of nonwork may be omitted from our sample of 634 women. Analysis performed on this sample may underestimate the long-run costs associated with nonemployment in the teenage years.

Table 11.1 Effects of Adjustments for Missing Data for Women Who Left School in 1966, 1967, or 1968 Who Had Less than 14 Years of Schooling

	Respondents who reported their labor force status in years 1 to 4 at the time of the interview[a]			Respondents who reported their past year's work history for years 1 to 4 after school completion		
Year	Percentage employed	Percentage unemployed	Percentage out of the labor force	Percentage employed	Percentage unemployed	Percentage out of the labor force
1	58.5	8.3	33.2	65.0	8.2	26.8
2	56.6	8.2	35.3	63.1	7.7	29.1
3	56.6	6.5	36.9	63.1	6.1	30.8
4	52.4	6.1	41.5	57.7	6.1	36.2
N	770	770	770	634	634	634

[a]There are 829 women in the Parnes who left school in 1966, 1967, or 1968 and who had less than 14 years of school; 770 of these women reported their labor force status at the time of the interview in the next four years.

11.1 Women's Early Employment Patterns

Women's employment and labor force participation declined steadily in the first four years out of school. Women's participation and employment rates dropped from 68 and 64% in the first year to 60 and 57% by the fourth year (table 11.2). This is in marked contrast to young men whose employment and participation rates rose steadily over the same period to 90 and 95% by the fourth year (see Ellwood, table 10.1). The decreases in women's participation and employment rates were the result of increases in the amount of time women stayed out when they were employed. The average time spent out of the labor force by those with any such time increased from 27 to 34 weeks over the period, and the proportion of women who did not work at all in a given year almost doubled from 12% in the first year to 24% in the fourth year.

The diversity and change apparent in teenage women's labor force patterns is striking. Women move continually between work and nonwork in the four years following school. Almost all women spend some time not employed (90%) and some time employed (96%) over this period. Even in a single year, at least three out of four women reported some work and six out of ten reported a period of nonwork (table 11.3).

Tracking the unemployment rate over time provides little information about changes in women's labor force activities. Although employment and participation rates decreased in the four years following school completion, there was no clear time trend in unemployment rates. Moreover, time unemployed formed less than one-seventh of women's total nonemployment time.

Women clearly spend a great deal of time not working in the years following school completion. Almost one-quarter of all women did not work at all in the fourth year following school and, in any given year, more than two-thirds reported some nonwork time. In addition, nonwork time per person out of work increased from an average of 27.4 weeks in the first year after school to an average of 32.3 in the fourth year. Given that the procedures for dealing with missing data and that NLS/CPS comparisons suggest that these data are likely to underrepresent nonwork time, these results are quite dramatic. Understanding the extent to which this nonwork time hinders women's future economic life chances is the central issue of this chapter.

11.1.1 The Persistence of Labor Market Experiences

Early labor market experiences predict later ones. This section documents the extent of this persistence in work experience. The next section will investigate whether or not this persistence is due to personal differences in worker characteristics or to a causal link between past and current employment.

Figures 11.1, 11.2, 11.3, and 11.4 are probability trees for unemployment, time out of the labor force, time not employed, and whether never employed over the four-year period. Each branch corresponds to one year. A one indicated that a woman was unemployed, spent time out of the labor force, was not employed, or was never employed in that year; a zero indicates the opposite. Above the line at a branch is the estimated

Table 11.2 Unemployment Rate, Employment Rate, and Labor Force Participation Rate for Young Women during First Five Years[a] after Leaving School in 1966, 1967, or 1968 with Less than 14 Years of Schooling

	Unemployment rate[d]	Employment rate[e]	Labor force participation rate[f]
Year 1[b]	5.8	63.9	67.8
Year 2[b]	4.8	62.9	66.0
Year 3[b]	5.6	60.5	64.1
Year 4[b]	5.4	56.8	60.0
Year 5[c]	3.6	58.1	60.3

[a]A year here is not a single calendar year. Instead, the n^{th} year represents the n^{th} full year following school completion. Thus for women who left school in 1966, year 1 is 1967; while for women who left school in 1968, year 1 is 1969.

[b]$N=634$.

[c]$N=401$.

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

[e]Average weeks employed/52.

[f]Average weeks in labor force/52.

probability of being in that state given one was at the previous branch. Below the line in parentheses is the proportion of all people who are found on that branch. The bottom number under a branch is the length of time spent in a particular state. Thus, in figure 11.1, 34.4% of women who were unemployed in their first year were also unemployed in the second year; 8.8% of all women were unemployed in both years, and these women averaged 9.3 weeks out of work.

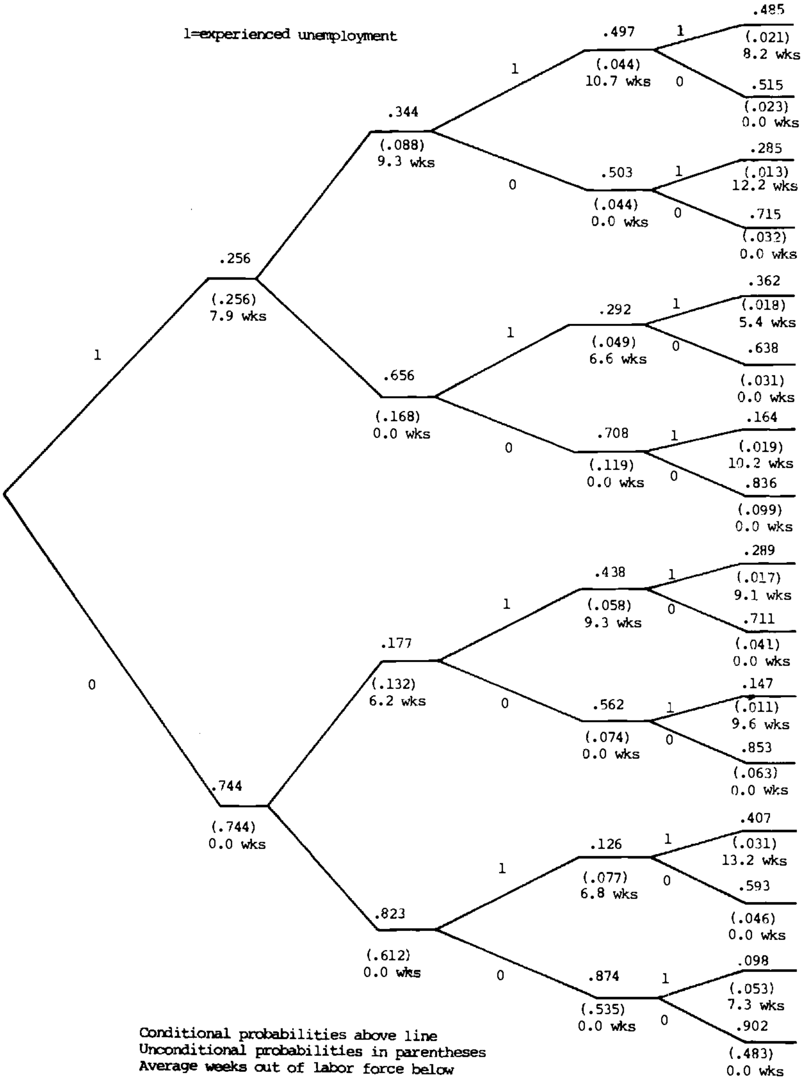

Fig. 11.1 Probability Tree of Weeks Unemployed in First Four Full Years out of School ($N = 634$)

There is considerable persistence in young women's labor market experiences. For instance, the estimated probability that a woman did not work at all in year 2 is .608 if she did not work in the previous year and .116 if she worked (see figure 11.4). By the fourth year after school completion, women who had never worked were eight times as likely not to work as were women who had worked in each of the previous years (.767 to .089).

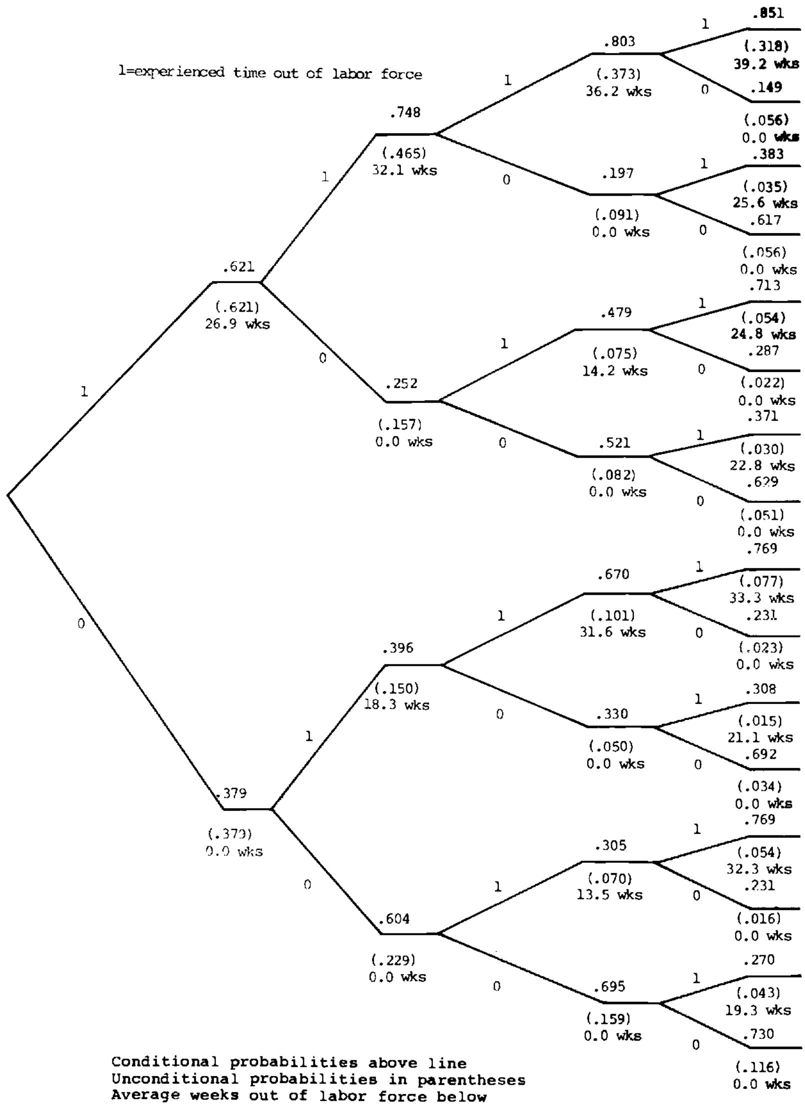

Fig. 11.2 Probability Tree of Weeks out of the Labor Force in First Four Full Years out of School ($N = 634$)

But such patterns can be misleading (see Ellwood). If spells of unemployment are long, say thirteen weeks, and are distributed randomly throughout the year, then one-quarter of all the unemployed in one year would have spells which overlap into the next one. Table 11.4 gives the estimated probabilities of being unemployed, out of the labor force, not employed, or never working in the third, fourth, and fifth years, given one's work experiences in year 1. Again, labor market behavior persists.

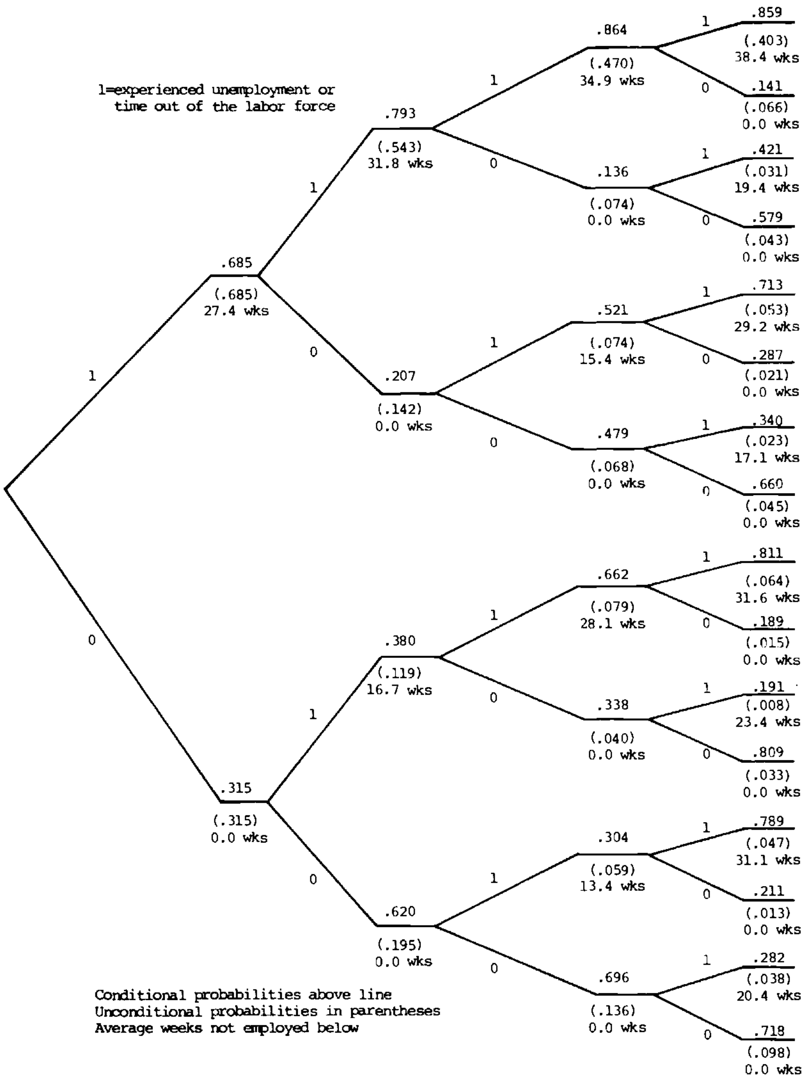

Fig. 11.3 Probability Tree of Weeks Not Employed in First Four Full Years out of School ($N = 634$)

Women who spent some time not employed in the first year were one and a half times as likely to miss some work in the fifth year as were women who had worked continuously in the first year. And women who did not work at all in year one were 1.8 times as likely not to work at all in the fifth year as were women who had worked in the first year.

Cross-year correlations for weeks not employed in the first four and five years following school completion also indicate that employment

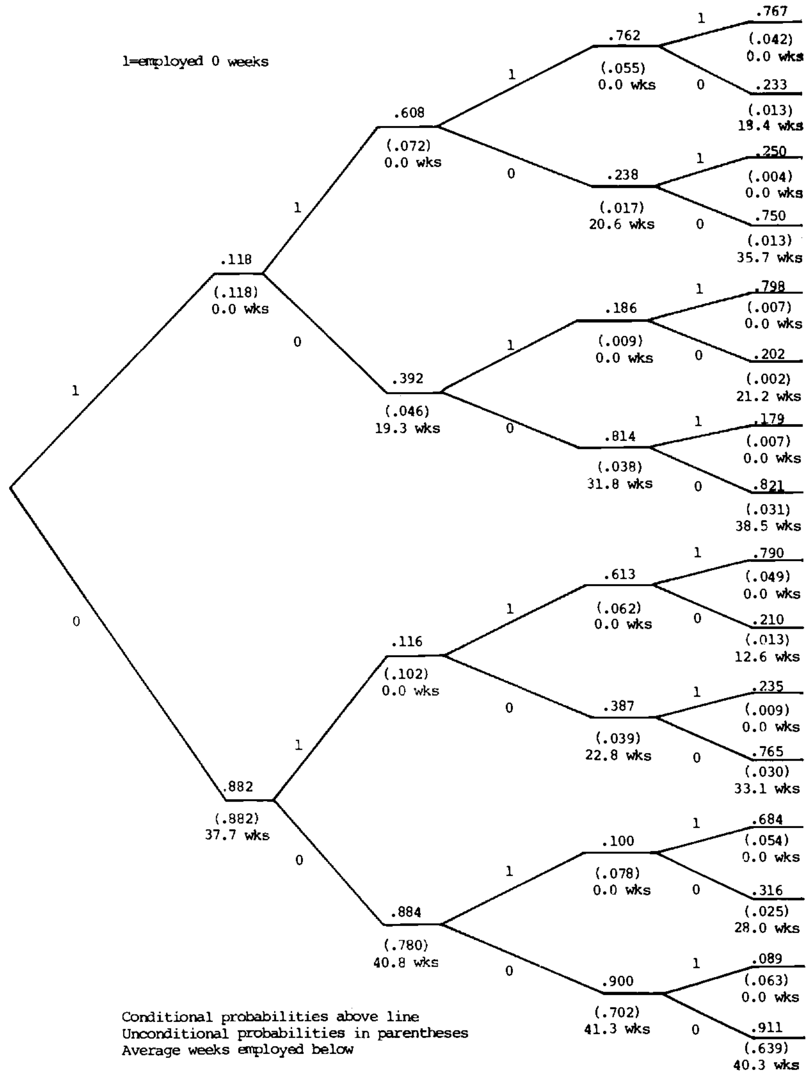

Fig. 11.4 Probability Tree of Zero Weeks Employed in First Four Full Years out of School ($N = 634$)

Table 11.3 **Percentage of Women with Unemployment Time, Time out of Labor Force, and Time not Employed, and Who Never Worked during the First Four Years after Leaving School**

	Percentage unemployed	Percentage with time out of labor force	Percentage with time not employed	Percentage who never worked in year t
Year 1	25.6	62.1	68.5	11.8
Year 2	22.0	61.5	66.3	17.2
Year 3	22.8	61.8	68.2	20.4
Year 4	18.2	62.6	66.7	23.9

NOTE: $N=634$.

behavior persists (See table 11.5). Estimates of the one-year correlation range from .6 to .7 and the correlation between the first and fifth years is .26. Persistence in weeks not employed is stronger for young women than for young men (Ellwood). There is also a slight tendency for correlations to increase over time.

Women's weeks unemployed show far less persistence. Adjacent year correlations range from .12 to .35 and drop quickly. The correlations between weeks unemployed one year removed range from .05 to .21.

The cross-year correlations for weeks not employed are more stable than would be generated by a first-order Markov process. Individual differences could be an important part of the underlying process. The next section of this paper investigates whether persistence in employment merely reflects differences in workers' traits or whether future employment is causally related to past experience.

Table 11.4 **Probability of Unemployment, Time out of Labor Force, Time Not Employed, and Never Working in Later Years Conditional on First Full Year out of School**

	Unemployment	Time off	Time not employed	Never working
P(1 in year 2/1 in year 1)[a]	.344	.748	.786	.608
P(1 in year 2/0 in year 1)[a]	.177	.396	.370	.116
P(1 in year 3/1 in year 1)[a]	.362	.721	.793	.542
P(1 in year 3/0 in year 1)[a]	.182	.449	.440	.159
P(1 in year 4/1 in year 1)[a]	.278	.702	.744	.508
P(1 in year 4/0 in year 1)[a]	.150	.499	.499	.198
P(1 in year 5/1 in year 1)[b]	.242	.719	.758	.408
P(1 in year 5/0 in year 1)[b]	.187	.519	.485	.221

[a]$N=634$.
[b]$N=401$.

Table 11.5 Correlation Matrix for Weeks Not Working during First Five Years out of School ($N = 401$)

Weeks not working	Year 1	Year 2	Weeks not working Year 3	Year 4	Year 5
Year 1	1.00	.57	.46	.32	.26
Year 2		1.00	.67	.49	.45
Year 3			1.00	.67	.54
Year 4				1.00	.74
Year 5					1.00
Weeks unemployed	**Year 1**	**Year 2**	**Weeks unemployed Year 3**	**Year 4**	**Year 5**
Year 1	1.00	.35	.05	.11	.00
Year 2		1.00	.15	.15	.03
Year 3			1.00	.12	.21
Year 4				1.00	.29
Year 5					1.00

11.2 Sources of Persistence in Young Women's Employment Decisions

Past and current employment decisions show a positive and strong association, but several quite different processes could generate this association. It could be seen as a "mover-stayer" problem; unobserved differences in women's talents, motivations, or preferences—"heterogeneity"—might be correlated with their past and present employment behavior (Heckman and Willis 1977). A second possibility is that women's past and present work behavior is affected by unobserved variables which are serially correlated over time. For instance, a woman may not work in two adjacent years because the local market is depressed both years. Finally, early work (or nonwork) may have a "real" effect on later work behavior—"state dependence." This could arise for several reasons. Women's preferences for market or home work may be altered as a result of early employment (or home production) activities; i.e., working may reinforce the desire to work. Similarly, women's work skills and hence their ability to find employment and/or demand high wages may grow as a result of employment (through investment in on-the-job training, accumulation of seniority, etc.) and depreciate during periods of nonwork (see Mincer and Polachek). Finally, even if worker skills and motivations are unaltered by early nonwork or employment experiences, employers may use past behavior as an indicator of future behavior when hiring.

Distinguishing heterogeneity from serial correlation and state dependence is not straightforward. Economists have routinely dealt with heterogeneity by assuming it away, i.e., by assuming that unmeasured tastes, preferences, and/or talents are uncorrelated with included independent variables (in this case with past work behavior). The ways in which panel data are collected further complicate this task. Apparent persistence in employment behavior over time could occur simply because a single employment spell spans two data collection periods.

11.2.1 Heterogeneity vs. State Dependence: Some Econometric Models

Heckman (1978b, 1978d) and Chamberlain (1978c) have developed models to explore persistence in behavior over time. I will use Chamberlain's autoregressive logistic model to investigate the extent to which a woman's work history influences her current work behavior. This model eliminates effects of unobserved person factors by comparing the likelihood that a woman works given she worked in the previous year to the likelihood that the *same* woman works given she did not work in the previous year.

The model is:

$$\mathrm{Prob}(y_{it} = 1 \mid y_{i,t-1}) = \frac{\exp(\alpha_i + \gamma y_{i,t-1})}{1 + \exp(\alpha_i + \gamma y_{i,t-1})} \qquad \begin{array}{l} i = 1, \ldots, N \\ t = 1, \ldots, T \end{array}$$

with $y_{it} = 1$ if person i is employed anytime in period t, $= 0$ otherwise and α_i = unobserved personal characteristics which raise the ith person's propensity to work $i = 1 \ldots N$. In this model, the conditional probability that the ith woman works this year, given her last year's employment status depends on an individual-specific constant (α_i) and on her employment status in the previous year. This model has $N + 1$ unknowns: N individual-specific constants (α_i) and γ, the coefficient on last year's employment status. If unmeasured person effects (α_i) completely accounted for the observed association between past and present employment behavior then γ should equal zero. We can test this by calculating a confidence interval for γ.

Note three characteristics of this model. First, the individual traits that influence a woman's probability of work (α_i) do not vary over the time period considered (in this case over the five years following school completion). Second, this model assumes that γ is constant over the time period considered; that is, the relationship between employment in two adjacent years does not change over time. Taken together, these two assumptions imply that the distributions of transitional employment probabilities should be similar across time. We can check this by seeing whether the estimated conditional employment probabilities change much over the five-year period. The estimated mean values of $p(1 \mid 0)$, the probability of working this year given one did not work last year, were quite similar across the five-year period, ranging from .106 to .120. But estimated values of $P(1 \mid 1)$, the conditional probability of employment this year given employment last year, increase over the five-year period from .549 in years 1 and 2 to .680 in years 4 and 5. This suggests that there may be a time trend in employment behavior. Third, there are no Xs (exogenous predictors) in this model. Constant Xs will be captured in α_i, but the model cannot capture effects of changing Xs. This means that we cannot differentiate serial correlation from state dependence with this model. In practice, this may not be such a serious limitation. I attempted to predict women's employment decisions using a number of demographic and demand variables which changed over time.[3] Only one of these consistently and significantly influenced women's probability of employment, "number of dependents." But if lack of early work reduces the probability of later work by increasing women's incentives to bear and raise children, then we should not control for family size when estimating state dependence.

If we allow each individual to have her own individual specific parameter (α_i), maximizing the joint likelihood function over α_i and γ will not in general provide a consistent estimator of γ. But Chamberlain shows that we can get a consistent estimator of γ if we use a conditional likelihood function. The basic idea is that the number of years a women was employed over the period, $s_i = \sum_{t=1}^{T} (y_{it})$ and her employment status in the last year (y_{iT}) provide sufficient statistics for the omitted person factor, (α_i). Holding fixed s_i and y_{iT}, α_i drops from the likelihood function. Initial conditions are dealt with by conditioning a woman's employment status in the first full year following school completion (y_{i1}).[4] This gives:

$$\text{Prob}(y_{i1}, \ldots, y_{iT} \mid y_{i1}, \sum_{t=1}^{T} y_{it}, y_{iT}) = \frac{\exp\left(\gamma \sum_{t=2}^{T} y_{it} y_{it-1}\right)}{\sum_{d \in B_i} \exp\left(\gamma \sum_{t=2}^{T} d_t\, d_{t-1}\right)}$$

where $B_i = \{\underset{\sim}{d} = (d_1, \ldots, d_T) \mid d_t = 0 \text{ or } 1,$

$$d_1 = y_{i1}, \sum_t d_t = \sum_t y_{it}, d_T = y_{iT}\}$$

Here, $S_{i11} = \sum_{t=2}^{T} y_{it} y_{i}, t-1$ is a sufficient statistic for γ.

For $T \geq 4$, there are conditional probabilities that depend upon γ. Since not all conditional probabilities will depend upon γ, this procedure uses only a subset of any given sample to estimate γ. For instance, when $T = 5$, 18 of the 31 possible sequences depend upon γ (see Chamberlain 1978c for a more detailed discussion of this model).

Even if γ were significantly different from zero we still cannot conclude that a woman's past work behavior is causally related to her current work behavior. Such persistence could also be due to unmeasured factors which influenced her chances of working and which were serially correlated over time (e.g., local demand conditions).[5] In addition, the measure γ depends upon the period of observation. Even if women's past and current behavior were not causally related, we would expect γ to be nonzero simply because a nomemployment spell may span two years. To see this, suppose our period of observation were one day; the probability that a person who worked yesterday would work today would be very close to one.

To describe completely a woman's work history we would want to know the length and timing of all spells of work and nonwork. If her past work history does not help us to predict her future, given her current state, then this is a Markov process. Chamberlain (1978c) calls deviations from the Markov property "duration dependence." He points out that duration independence would imply that a woman's employment history prior to the current spell should not affect the distribution of the length of the current spell, and the amount of time spent in the current spell should not affect the distribution of remaining time in that spell. This implies that the durations of the spells should be independent of each other and the distribution of time in a state should be exponential. If we assumed that all spells of employment have the same distribution, that all spells of nonwork have the same distribution, and that the exponential rate parameter for each of the states is the same for all spells, then we would have an alternating Poisson process. In this case, the stationary heterogeneity model implies that each woman is characterized by the two parameters of an alternating Poisson process. Departure from this model would be evidence of duration dependence at the individual level; i.e., even given her current state, a woman's past history helps predict her future.[6]

Chamberlain has developed tests for duration dependence based on binary employment sequences generated by questions such as "Did you work last year?" The basic idea underlying these tests is that stationary heterogeneity implies that a woman's probability of working in period t depends upon the number of consecutive periods immediately preceding period t in which she worked. The reasoning is as follows. If a woman is following an alternating Poisson process, then only her state at the end of the past year is relevant. If $y_{t-1} = 1$, we know only that she worked sometime last year. We do not know whether she worked at the end of the past year, and y_{t-2} will affect the probability that she worked early in the year rather than late in the year. But if $y_{t-1} = 0$, then the woman *never* worked last year. Thus we know her state at the end of that year and y_{t-2}, y_{t-3}, etc. are irrelevant.

This gives

$$\text{Prob}(y_t = 1 \mid y_{t-1}, y_{t-2}, \ldots) = \text{Prob}(y_t = 1 \mid y_{t-1} = \ldots = y_{t-J} = 1, y_{t,J-1} = 0) = \text{Prob}(y_t = 1 \mid J)$$

where J = the number of consecutive preceding years that the woman was employed. That is, the probability that a woman works in year t depends only on how many consecutive years she worked immediately preceding year t. This would give the following logistic model:

$$\text{Prob}(y_{it} = 1 \mid y_{i,t-1}, y_{i,t-2}, \ldots) = \frac{e^{A_i}}{1 + e^{A_i}}$$

where $A_i = \alpha_i + \sum_{k=1}^{\infty} \psi_{ik} \prod_{j=1}^{k} y_{i,t-j}$

Here, each woman has her own set of parameters α_i and Ψ_{ik}. Chamberlain extends this model to test for duration dependence as follows.

$$\text{(2)} \quad \text{Prob}(y_{it} = 1 \mid y_{i,t-1}, y_{i,t-2}, \ldots) = \frac{\exp(A_i + \gamma_2 y_{i,t-2})}{1 + \exp(A_i + \gamma_2 y_{i,t-2})}$$

For $T \geq 6$ and large N, we can consistently estimate γ_2 using a conditional likelihood function. For $T = 5$, Chamberlain's model has some equality predictions for particular sets of conditional probabilities which enable us to tell whether or not γ_2 differs significantly from zero.

11.2.2 Empirical Results: Employment Effects of Early Nonwork

The techniques described in the previous section were used to analyze the persistence of employment. I looked at employment behavior rather than participation behavior because results of other studies (Ashenfelter, Summers and Clark) suggest that for young women time unemployed and time out of the labor force are not conceptually distinct.

I began by obtaining an estimate of first-order dependence that is based on Chamberlain's autoregressive logistic model, where each woman is assigned her own employment probability (equation 1). Chamberlain's conditional likelihood function on five-year employment sequences gives $\hat{\gamma} = 2.05$ with a standard error of .33. This estimate is based on the employment sequences of 80 women (19.9% of the five-year sample).[7]

A woman's employment behavior in one year is a good predictor of her employment behavior the next year—even after we allow each woman to have her own employment probability. The odds that the *same* woman works, given she worked in the previous year are $e^{2.05} = 7.8$ times higher than if she did not work last year. While high, these odds are only about half as large as the odds we would get if we ignored heterogeneity. Not allowing for unobserved person factors would increase these odds to 14.8.[8]

This estimate of adjacent year persistence in work behavior ($\hat{\gamma}$) assumes stationary heterogeneity, invariance of γ over time, and does not control for changing Xs. Yet the estimates of $P(1 \mid 1)$, the conditional probability of a woman being employed this year if she was employed last year, rise considerably in the five-year period following school from .549 for years 1 and 2 to .680 for years 4 and 5. This suggests employment transition probabilities may be increasing over time. Our estimate of persistence may pick up some of this time trend.

The following model suggested by Chamberlain (1979) allows us to include a time trend:

$$p(y_{it} = 1 \mid y_{i,\,t-1}) = \frac{\exp(\alpha_i + \beta X_t)}{1 + \exp(\alpha_i + \beta X_t)} + \gamma y_{i,\,t-1}$$

$$\text{where } X_t = t - 1 \quad t = 1, \ldots, T$$

Here the term βX_t allows us to pick up a time trend. Consistent estimates of $\hat{\beta}$ can be obtained using a conditional likelihood function. By comparing the value of γ that obtains when we set $\beta = 0$ to the value of γ that obtains when β is estimated, we can get a rough idea of how much our estimate of adjacent-year persistence will drop if we allow for a time trend.

Not allowing for a time trend (i.e., setting $\beta = 0$) gives $\hat{\gamma} = .47$. This estimate is significant ($p < .05$). Not allowing for state dependence (setting $\gamma = 0$), gives $\hat{\beta} = .30$. This estimate is not very significant ($p = .25$). If we estimate both β and γ, the estimate of γ is still large, $\hat{\gamma} = .31$, but is not very significant ($p = .25$); the estimate of β is quite small, $\hat{\beta} = .09$ and is quite insignificant ($p = .86$). Thus allowing for a time trend reduces estimates of adjacent year persistence ($\hat{\gamma}$) by about one-third.[9]

The NLS data did not provide strong predictors of the work decision, which also changed over time and which were not proxies for expectations. I tried area demand variables (whether South, whether lived in a city, and the local unemployment rate) and a measure of husband's income as predictors of the decision to work in the second, third, fourth, and fifth years following school completion. None of these consistently and significantly predicted the decision to work. Given this, I did not attempt to differentiate between serial correlation and state dependence on these analyses.

Recall that we would expect to observe some persistence in women's work behavior simply because of the way in which the NLS employment information is recorded. That is, past and current employment will be associated simply because a single work (or nonwork) spell may span two years. Instead of asking whether a woman's previous year's employment status helps to predict her current year's employment status we may want to ask whether, given her current employment status, her employment history enables us to predict her future. That is, does employment behavior follow a Markov process? Departures from this Markov property are evidence of duration dependence, i.e., evidence that given a woman's current employment status, her past employment history is informative about her future.

I tested for departures from this Markov property using Chamberlain's second-order autoregressive logistic model (equation 2). Chamberlain has shown that when $T = 5$, probabilities of certain binary employment sequences should be equally likely whenever there is no duration dependence. A likelihood ratio test comparing those probabilities which obtain under the assumption of no duration dependence ($\gamma_2 = 0$) to the probabilities in the data (unrestricted model) gives $\chi^2(5) = 10.9$, based on 30 women. This is significant at the .05 level, suggesting a departure from an alternating Poisson process. That is, we can not conclude that given a woman's current state, her past work history will not influence her future.

Taken together, these results strongly reject the notion that unexplained personal differences account entirely for the strong link between women's present and past employment behavior. The odds that a woman works given she worked last year are 7.8 times higher than if she did not work last year—even if we allow each woman to have her own employment probability. Furthermore, even after we adjust for data collection procedures, there is still a link between past and current employment. Given a woman's current employment status, her past work history still predicts her future. This finding was significant even though based on only a small number of cases.

Given the small number of cases, I could not estimate the magnitude of this duration dependence, nor could I estimate how quickly the predictive power of early employment dies out over time. Finally, it may be that women who do not work early in their careers "catch up" by working more later. Because of data restrictions I did not investigate this possibility.

These findings are not inconsistent with those reported by Ellwood and by Meyer and Wise. We all find employment behavior persists in the short run. Ellwood and Meyer and Wise further show that these employment effects diminish after several years; because of data restrictions I did not investigate this for women.

11.3 Long-Run Wage Losses Associated with Early Nonwork

The previous section documented and explored the persistence of employment in the short run. In the long run, lost wages may be a more serious cost of early nonemployment. Losing early work experience may impose costs in addition to delaying the start of a career. If employers evaluate a worker's potential by her past work behavior, women who spent considerable time out of work in the years following school completion may be permanently tracked into less desirable career ladders. If working (or not working) reinforces the tendency to work (or not to work) and/or if human capital depreciates during periods of nonwork, then women's expected lifetime earnings may be permanently lowered by extended periods of nonwork in their early careers. Even if much of the wage loss were "voluntary" (in the sense that women trade off wages for flexibility and/or time to engage in home work), it would still be useful to know the "opportunity costs" associated with early nonemployment.

I explore the long-run opportunity costs of early nonwork using a sample of 2,067 employed women aged 18 to 64 (1,326 whites and 741 blacks) from the 1976 wave of the Panel Study of Income Dynamics (PSID). In 1975, these women reported about their wages and current jobs and gave retrospective reports of their employment histories. Using these data, I constructed the experience and nonwork measures. (Elsewhere [Corcoran 1979] I describe in detail how these measures were constructed.) Note that these measures distinguish nonwork which occurs early in a woman's career and "number of years not employed in the period following school completion" from nonwork which occurs later in a woman's career and "other nonwork time." A large percentage of these women (29% of the whites and 42% of the blacks) experienced a year or more of nonwork early in their careers. And this nonwork was often quite extensive; the average duration was 9.6 years for white women and 7.2 years for black women.

This analysis involves seven pairs of equations. First I regress the natural logarithm of wages on experience and nonwork measures with controls for education, city size, and region for employed black and white women (table 11.6, columns 1 and 2). Some economists (Heckman 1974; Gronau 1974) have argued that restricting analysis to employed women could lead to selection bias if the independent variables in the wage equation influence a woman's market wage relative to her reservation wage. So next I reestimate the wage equation using a procedure described by Heckman (1977) which corrects parameter estimates for selection bias (table 11.6, columns 3 and 4).[10] This is followed by a modest attempt to control for heterogeneity by adjusting for individual differences in women' labor force attachments. The PSID provides four indicators of labor force attachment: absenteeism due to own illness; absenteeism due to others' illness; self-imposed restrictions on work hours and/or job location; and whether the respondent plans to leave work in the near future for reasons other than training. If women with less experience earn less than other women because low attachment to the labor force both decreases wages and leads to less work,[11] then controlling for attachment should reduce the observed effect of work and nonwork measures on wages. Columns 5 and 6 of table 11.6 present the results when these four indicators of attachment are added to the regression of experience and nonwork measures on wages. But to the extent that these measures of labor force attachment are subject to random measurement errors, use of OLS may still understate the influence of attachment on wages (Griliches and Mason 1973) and hence overstate the influence of experience and nonwork. To correct for this problem I use a two-stage procedure to get predicted values of the labor force attachment measures (table 11.6, colums 7 and 8).

Finally, work experience is not obviously an exogenous variable since a woman's expected market wage will presumably influence her decision to work. Table 11.7 compares results of three sets of equations. In the first (columns 1 and 2), experience is assumed exogenous; in the second (columns 3 and 4), experience is assumed to be endogenous; and in the third, experience is assumed endogenous and corrections are made for selection bias.[12]

Results were consistent across all sets of equations. Both black and white women's wages increased with experience; this increase was large for the first few years and then dropped off over time. In addition, not working for prolonged periods early in one's career lowered white women's expected wages by .7% for each of the nonworking years in addition to lowering wages indirectly by lowering total experience.[13] None of the observed influences of early nonwork and experience on wages was reduced when I adjusted for selection bias. This is consistent with other research (Heckman 1977a, Corcoran 1979). Similarly, treating experience as endogenous did not reduce the estimated effects of experience or early nonwork on wages. The magnitude of these influences also remained unchanged when controls were added for labor force attachment. But, of course, these procedures, which provide very crude adjustments for unmeasured personal traits, influence early experience and wages, so we may be overestimating the long-term wage costs that are causally associated with early nonwork.

Two kinds of opportunity are associated with early nonwork: foregone earnings and the reduction in later earnings that is associated with lower experience and extended nonwork. I estimated this latter cost by comparing the expected 1975 earnings of women who have worked continuously to those of otherwise similar women (in terms of education, age, race, residence, and on our measures of labor force attachment) who did not work for a year or more in the period immediately following school completion (see table 11.8). Note that these estimated costs are for nonwork which occurs in the years following school completion.[14] These costs are quite large even many years later. Ten years after school completion a two-year period of nonwork lowers white women's ex-

Table 11.6 **Work Experience, Early Nonwork. and Wages for Employed Women Aged 18–64 Who Were Wives or Heads of Households in 1975 ($N = 1326$ Whites[a], 741 Blacks)**

Predictor Variables[b]	Dependent variable = ln (1975 hourly wage)							
	White	Black	White	Black	White	Black	White	Black
Education	.0855**	.0921**	.0859**	.0902**	.0842**	.0920**	.0831**	.0730**
	(.0053)	(.0070)	(.0055)	(.0073)	(.0054)	(.0073)	(.0057)	(.0091)
Total work experience	.0290*	.0251*	.0300*	.0284*	.0296*	.0242*	.0363*	.0264*
	(.0045)	(.0053)	(.0054)	(.0054)	(.0046)	(.0052)	(.0055)	(.0069)
Total work experience squared	−.0005*	−.0005*	−.0006*	−.0006*	−.0006*	−.0005*	−.0007*	−.0006*
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Number of years not employed following school completion[c]	−.0067*	.0016	−.0073*	−.0018	−.0066*	.0027	−.0072*	.0018
	(.0023)	(.0029)	(.0030)	(.0030)	(.0023)	(.0030)	(.0025)	(.0032)
Controls for labor force attachment measures[d]					—	—	—	—
Corrections for censoring[e]			—	—				
Labor force attachment measures are instrumented[f]							—	—

(*Notes appear on following page*)

Table 11.7 Work Experiences and Wages: Corrections for Endogeneity for Employed Women Aged 18–64 Who Were Wives or Heads of Household in 1975 ($N = 1326$ Whites,[a] 741 Blacks)

	Dependent variable = ln (1975 hourly wage)					
Work experience measures[b]	White	Black	White	Black	White	Black
Total work experience	.0271[e]	.0236[e]	.0292[e]	.0217[e]	.0301[e]	.0360[e]
	(.0047)	(.0052)	(.0056)	(.0073)	(.0056)	(.0085)
Total work experience squared	−.0005[e]	−.0055[e]	−.0007[e]	−.0006[e]	−.0008[e]	−.0010[e]
	(.0001)	(.0001)	(.0002)	(.0002)	(.0002)	(.0003)
Corrections for endogeneity of experiences[c]	—	—			—	—
Corrections for censoring[d]			—	—	—	—

NOTE: A dash indicates the procedure was followed.

[a]A white is defined as a nonblack.

[b]Controls were included for education, city size, and region.

[c]Experience as a fraction of time since leaving school was estimated as a function of education, family income exclusive of respondent's own earnings, marital status, and number and ages of children. Expected experience was estimated to be the product of this fraction and time since leaving school.

[d]This was done by estimating a probit analysis of the decision to work for all PSID wives and female heads using instrumented experience, education, city size, region, marital status, number and ages of children, and family income exclusive of the respondent's own earnings. I used this equation to construct the Mills ratio and included it in the regression.

[e]Significant at .05 level.

Table 11.6 notes

A dash indicates this variable was included.

[a]A white is defined as a nonblack.

[b]In all regressions, other nonwork time, city size, south and percentage of work experience that was full time were controlled. See Corcoran (1979) for details on how these variables were constructed.

[c]See Corcoran (1979) for details on how these measures were constructed.

[d]These measures include: days absent in 1975 because of own illness; days absent in 1975 for care for others; self-imposed restrictions on job hours or location; expect to leave work in the near future for nontraining reasons. See Coe (1979) for details on the absenteeism measure; Hill (1979) for details on the self-imposed restrictions measure, and Corcoran (1979) for details on the expect-to-leave-work measures.

[e]This was done by estimating a probit analysis of the decision to work for all PSID women using the work experience, and nonwork measures, education, city size, region, family income exclusive of the respondent's earnings, marital status, and the number of children less than 3 years, 3–6 years, 7–11 years, and 12–17 years. I used this equation to construct the Mills ratio and included it in my regression. See Heckman (1977) for a detailed description of this procedure.

[f]I instrumented the labor force attachment variables using a two-stage least squares routine. Instruments included the number and ages of children, marital status, family income exclusive of respondent's earnings, whether expect more children, own health problems, whether anyone in the family needed extra care, and fertility plans.

*Significant at .05 level.

Table 11.8 Expected Percentage Wage Differences between Women Who Worked Continuously Since School Completion and Women Who Experienced a Spell of Nonwork in the Period Immediately Following School (White Women[a])

Length of nonwork spell (in years)	Number of years since leaving school (maximum potential experience)				
	5	10	15	20	25
1	3.1	2.5	1.9	1.3	.7
2	6.3	5.1	3.9	2.7	1.5
4	13.0	10.6	8.2	5.8	3.4
8	—	23.2	18.4	13.6	8.8
12	—	—	30.5	23.3	16.1
16	—	—	—	34.9	25.3

[a]These figures are estimated using the coefficients in table 11.6, column 5. This gives the effects of experience when education, city size, region, part-time work, self-imposed limits on jobs hours or location, days absent to care for oneself or others, and expectations about leaving work are held fixed.

pected wages by 5% and black women's expected wages by 3%. Even twenty years later, a four-year spell of nonwork lowers white women's wages by 5.8% and lowers black women's expected wages by 2.5%.

It might be more useful to estimate the expected wage reductions associated with an early spell of 9.6 years of nonwork for white women and a spell of 7.2 years of nonwork for black women (these are the average durations of early nonwork time for women with any such time). Twenty years after school completion, these expected wage reductions are 16.8% for white women and 5.6% for black women.

Given differences in sample populations and in methodology, it is difficult to compare these wage costs with those estimated by Ellwood and by Meyer and Wise. Nonetheless, the consistency across studies is quite remarkable. These authors also found that early nonwork was associated with significantly lower wages later on. Their estimates of men's wage losses were larger than those estimated for this sample of women.[15]

The wage losses associated with not working in the years following school completion are large and persist over time. I suspect, however, that a part of the wage costs associated with lower experience and early nonwork could be due to unobserved factors (e.g., "ability"; propensity to work) which differ across women and which are correlated with both employment behavior and wages, and which are inadequately captured by the included labor force attachment measures. In addition, even if all the observed wage losses were causally related to early nonwork, women may be voluntarily electing to trade off these wage gains for other desired goals. Nonetheless, it is evident from these data that women miss a great deal of nonwork early in their careers and that this loss of work is associated with lower lifetime earnings. Women forego earnings by not working, and early nonwork is associated with lower hourly wages throughout most of a woman's career.

11.3.1 Summary and Conclusions

Young women moved continuously in and out of employment in the four years following school. Almost all the young women in this sample spent some time not employed over this period, and it appears that these women's labor force attachment weakened somewhat over this period.

Many of these young women were not employed for a prolonged period. Descriptive results showed evidence of considerable persistence in women's employment behavior. Further analysis suggested that a part of this persistence was due to unmeasured individual differences which influenced a woman's propensity to work. Nonetheless, even allowing each woman to have her own employment probability, the odds that she worked given that she worked last year were 7.8 times higher than if she did not work in the previous year. Since the NLS data did not provide good exogenous predictors of employment behavior, I could not test whether this persistence was due to a causal relationship between past and present employment or to exogenous variables which were serially correlated over time.

Part of this persistence could, of course, be caused by the way the NLS collects and records employment behavior. That is, we would observe

Table 11.9 **Expected Percentage Wage Differences between Black Women Who Worked Continuously Since School Completion and Black Women Who Experienced a Spell of Nonwork in the Period Immediately Following School[a]**

Length of nonwork spell (in years)	Number of years since leaving school				
	5	10	15	20	25
1	2.0	1.5	1.0	.5	0
2	4.0	3.0	2.0	1.0	0
4	8.0	6.5	4.5	2.5	.5
8		14.6	10.6	6.6	2.6
12			18.2	12.2	6.2
16				18.7	11.5

[a]These figures are estimated using the coefficients in table 11.6, column 6. This gives the effects of experience when education, city size, region, part-time work, self-imposed limits on jobs hours or location, days absent to care for oneself or others, and expectations about leaving work are held fixed.

some first-order serial correlation simply because a single employment spell may span two years. However, even given a woman's current employment status, her past work history is informative about her future. Because of the small sample size, I could not estimate the magnitude of this association.

Evidence also suggests that early nonemployment is associated with lower future wages—even as much as twenty years later. Moreover, for white women, the wage losses associated with prolonged nonwork are greatest when it occurs at the beginning of their careers. While part of these wage losses may result from individual differences which are correlated both with early nonwork and with wages, controls for a number of behavioral indicators of labor force attachment did not decrease the estimated long-run wage losses associated with early nonwork. Whether or not is is voluntary, not working for a prolonged period during the teenage years is associated with considerably lower wages later on.

Nonemployment is pervasive and prolonged among teenage women with less than fourteen years of schooling. It is associated with a lower probability of employment in the short run and with lower wages throughout women's work careers. Whether voluntary or not, early employment behavior apparently has lasting implications for women's future economic careers.

Notes

1. This paper uses a set of techniques developed by Gary Chamberlain to examine employment persistence. Chamberlain has been extremely generous with both time and advice, and helped me plan an analysis strategy for examining employment persistence as well as providing useful assistance at every stage of the analysis. I am also grateful for discussions with Joan Brinser, Greg Duncan, David T. Ellwood, Elizabeth Phillips, and David A. Wise.

2. This does not appear to be the case for the NLS 74 (See Meyer and Wise).

3. I did this by estimating whether a woman worked in the i^{th} year following school completion as a function of region, urban residence, local unemployment rate, husband's income, marital status, and number of dependents with controls for age, race, and schooling.

4. This affects the distribution of $\underset{\sim}{\alpha}$, but since we are conditioning on $\underset{\sim}{\alpha}$, no problems arise.

5. Theoretically, we can distinguish serial correlation from "true" state dependence when we have stong predictors (Xs) of employment behavior which change over time. If all the remaining persistence in women's work behavior (after adjusting for heterogeneity) were due to exogenous factors that were serially correlated over time, then a change in X should have its full effect on work behavior immediately with no damped response into the future. On the other hand, if past and present work behavior were causally related, then a change in X should affect the probability of working now and should alter the probability of working in the future because the initial change in work behavior will induce future changes. To test this we need to introduce current and lagged values of predictors (Xs) into a model which predicts the current decision to work, which omits the lagged decision to work, and which adjusts for heterogeneity.

6. This paragraph is a brief summary of a more elaborate argument developed by Chamberlain (1978b, 1978c).

7. Applying Chamberlain's model to four-year employment sequences gives much the same results. The estimate of first order dependence is large and significant ($\hat{\gamma} = 1.81$ with a standard error of .41), again suggesting that past employment is associated with significantly higher changes of future employment, even after adjustments for heterogeneity.

8. We may calculate these odds as follows. For each year after the first, calculate the probability that a woman works, given she worked in the previous year. The average of these probabilities over years 2 . . T is equal to the average value of $P(1 \mid 1)$. Similarly calculate the average value of $P(1 \mid 0)$. For five years, these average values are .890 and .354. The odds are: $e^{\gamma} = (.890 \mid .110)\ (.646 \mid .354) = 14.8$. Here γ would equal 2.84.

9. Chamberlain (1979), in response to a question about time trends, very generously developed this procedure and used the NLS four year sample as an example.

10. This procedure involves estimating a probit function of the decision to work for all women, employed and unemployed, calculating the Mills ratio for each employed woman using the probit estimates, and including this Mills ratio in the regression equation.

11. Note that this test will not allow us to distinguish between two quite different hypotheses. The first is that both work behavior and wages are causally related to attachment, but not to one another. The second is that work behavior alters attachment which, in turn, influences wages.

12. In order to treat work experience as endogenous, I assumed that fertility was exogenous. As Cain (1976) points out, this assumption is difficult to justify.

13. It should be noted that this result is consistent with many different hypotheses. This penalty associated with early nonwork could be due to the depreciation of human capital, to stereotyping by employers on the basis of early work behavior, or to reinforcement of "good" work attitudes by work and "bad" work attitudes by nonwork. I will not attempt to differentiate among these competing hypotheses, although each has quite different implications for our understanding of the wage determination process for women. Instead my purpose is in a more limited—to assess the opportunity costs associated with early nonwork.

14. It might also be useful to ask the question, Are labor force withdrawals which occur in the period following school completion more costly than labor force withdrawals which occur after beginning a career? This is true for white women but not for black women (see Corcoran 1979).

15. Ellwood attempted to remove heterogeneity from the relationship between experience and wages by differencing using the NLS data. To do this, he assumed that wages and experience were not simultaneously determined and that problems of selection bias could be ignored since almost all young men worked in both years 3 and 4. These assumptions become much more suspect if applied to women. Research on women's labor force participation typically assume that wages influence time worked. In the NLS four-year subsample of women, about 30% of the women did not work either in year 3 or in year 4 and so did not have wage measures for one or both years. Given problems of simultaneity and selection bias, I chose not to examine heterogeneity with the NLS data.

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Comment Solomon William Polachek

According to some schools of existential philosophy,[1] decisions may be interpreted as being based on the moment, and hence independent of one's past and future. Economists no longer adhere to such a philosophy concerning the decision process, and are hard at work integrating decisions made at a given time with aspects of one's past and future. The chapter by Mary Corcoran is an example of such an analysis. This chapter seeks to assess the impact of early teenage women's nonemployment on two aspects of future life: the first concerns the immediate future, and the second pertains to a long-run time horizon. With respect to the short run, Corcoran looks at the impact of nonemployment on a subsequent year's work prospects. That is, do teenage women (alike in observed and unobserved characteristics) have a higher probability of working in any year given that they were employed in the previous year? With respect to the long-run impact of nonwork, the chapter seeks to ascertain whether wages, even 20 to 25 years in the future, are affected by nonwork spells upon school completion.

Basically it is found (1) that past (non-) employment raises the probability of current (non-) employment (even when adjusting for individual differences in initial work probabilities), and (2) that future wages are affected greatly by spells of teenage unemployment. Thus, regardless of individual characteristics, the past, the present, and the future *are* related for all. This need not imply a heterogeneous population composed of workers and nonworkers, but instead that "state dependence" is important. In fact, even after adjusting for population heterogeneity, the odds of a woman working, given that she worked last year, are about seven times higher than if she did not work the previous year.

Qualitatively (with exceptions to be noted), these results appear intuititively reasonable and similar to the Elwood and Meyer-Wise chapters in this volume. Yet I see at least two problems of major concern to policy makers. First, policy prescriptions are not derived, and second (as the author readily admits) computed statistics are subject to biases inher-

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ent in restrictions innately embedded in the estimation techniques. My comments address this latter problem first and then move on to the former by proposing alternative estimation schemes.

Persistence is determined by a nonzero γ parameter of Gary Chamberlain's "autoregressive logit model."[2] As Mary Corcoran aptly points out, three shortcomings exist: (1) that individual traits which influence work behavior are assumed not to vary over time; (2) that exogenous predictors are omitted; and (3) that the transitional probability (γ) is assumed to be stationary. It cannot be overemphasized that these restrictions can lead to serious biases. For example, if marital status or number of children (assumed constant in the model) in fact change, and if these variables determine work status (as is commonly found), then changes in labor force behavior will be observed, thereby appearing to lower the importance of state dependence when in fact marital status and children *could be* the sole determinants of work status, and state dependence could be absolute (true). Suffice it to say the other assumptions lead to more biases, but I think neglecting changing exogenous variables to be the most serious. Empirically, an illustration of this bias appears in different state dependencies observed by Mincer and Ofek (1978) and Heckman and Willis (1979) in their recent exchange.[3]

This bias and others can be corrected by applying slightly more sophisticated yet theoretically estimatable models. If one insists on a logit framework, the Chamberlain fixed effect logit augmented with an autoregressive structure could be used.[4] This too can be generalized by modifying the autoregressive structure so as to eliminate the necessity of a constant γ.

Even the long-term effects computed in tables 11.6 to 11.9 are subject to bias. These effects are computed essentially by OLS regressions of home-time on wages. Two-stage least-squares and adjustments for selectivity with respect to who is in the labor force at a given time do *not* alter results. Population heterogeneity (as adjusted for in computations on employment persistence) is not accounted for. Thus overestimates likely result since those with lower wages are most likely to face the most teenage nonemployment. The longitudinal nature of the data could be further exploited to alleviate the possibility of this potential bias. Two approaches are possible. One (that eliminates heterogeneity in a manner much like the autoregressive logit technique just used) would be to regress first differences of earnings with first differences in experience variables.[5] Another approach would be to apply pooled cross-section time-series techniques such as the variance of error components technique.[6]

Even if these possible biases prove to be inconsequential, one could ask how the estimates can be used by policy makers. It is to this end that I see some difficulty. Thus I wish to devote the remainder of my comments to this issue.

The major point of the chapter is that teenage unemployment is costly and therefore should be reduced. Yet no implication is given concerning what instruments are appropriate in achieving lower teenage unemployment rates. The reason for this omission stems from the fact that nonemployment is analyzed in a reduced form framework and hence taken as a "basic" measurement. Nonemployment causes more nonemployment, but the structural equations analyzing the real factors that cause nonemployment are treated as unobserved variables, and hence are omitted from the analysis. Thus, as a policy maker, I would not know whether decreases in nonemployment were more sensitive to unemployment insurance changes, antinatalist fertility policies, job training programs, minimum wage legislation, etc.[7] Furthermore, the decision to work may be innately tied to the wage equation, which is treated independently by Corcoran.

I thus propose an analysis of the structural equations of the system in a simultaneous equations context. As is well known, employment is related to the relationship between one's shadow value of time (a latent variable) and one's market earnings power. Fertility, too, may at least affect the value of one's home time. In short, both parts of the paper could be synthesized and studied within a unified framework.

Two approaches seem plausible. If one insists on treating employment as dichotomous, simultaneous equation probit techniques could be used. If one were to utilize more fully the per-period duration of employment status data, simultaneous tobit techniques could be adopted to perform panel estimates.

As an example of the former approach, let $d(i, t)$ depict individual i's work status in period t. Generally, $d(i, t) = 1$ if one works and $d(i, t) = 0$ if one does not. Following the logic of Heckman (1974, 1978), work status is determined by whether one's market wage $w(i, t)$ exceeds one's shadow price of time $w^*(i, t)$. Thus

$$d(i, t) = 1 \text{ if } w(i, t) > w^*(i, t)$$

where

$$w(i, t) = Z_1\beta_1 + \varepsilon_1$$
$$w^*(i, t) = Z_2\beta_2 + \sum_{j=1}^{T} \gamma(i, t-j)\, d(i, t-j) + \varepsilon_2$$

Appropriate specifications of Z_1 and Z_2 (exogenous time dependent factors, both individual and market oriented) enable one to determine the individual and market characteristics most influential in determining labor force behavior. Distinguishing β_1 and β_2 disentangles which of these factors affects more greatly the shodow prices of time in the home as contrasted to the market wage level. Similarly, changes in exogenous factors that affect work status and are important to policy makers would be accounted for directly. Although I have neither written the likelihood function nor estimated this system, others have moved in this direction.[9] Thus such estimation is feasible.

To summarize, I believe Corcoran's chapter represents an excellent and careful piece of research which addresses a most important topic. Using the latest computer software, she finds strong short-run and long-run impacts of female teenage nonemployment on their future well-being. My comments are designed not to critize but instead to prod Corcoran and others into extending already existing panel data models to achieving innately less biased and more policy oriented parameters.

Notes

1. For example, see Watts (1951) and Marcuse (1964).
2. See Chamberlain (October 1978), pp. 32–38.
3. Contrast the results of tables 2–3 of Mincer and Ofek (1979) with tables 1–4 of Heckman and Willis.
4. In this case,

$$\text{Prob}\ (y_{it} = 1 | \underset{\sim}{x}_i, y_{i,\ t-1}) = \frac{e^{\alpha_i} + \gamma y_{i,\ t-1}}{e^{\alpha_i} + e^{-\underset{\sim}{\beta}\, \underset{\sim}{x}_{it}}}$$

where

$\underset{\sim}{x}_i \equiv$ a vector of exogenous variables and the other variables are defined in the Corcoran chapter.

5. An example of this is Mincer and Polachek (1978).
6. An example of this technique is used in Lillard and Willis (1978) and Kniesner, Padilla, and Polachek (1980).
7. Also following Clark and Summers (chapter 7 of this volume), no distinction is made between being out of the labor force and in the labor force but not at work.
8. Dummy exogenous regressors could be used to indicate single work spells spanning two-year segments in an attempt to avoid possible first-order serial correlation.
9. For example see Waldman, "The Time Allocation of Young Men" (1979). In that application the joint decision of school and work status is modeled on the basis of wage and shadow wage equations.

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Comment Isabel V. Sawhill

Mary Corcoran's chapter is the twin sister of David T. Ellwood's chapter (10). Both consider the so-called scarring effects of a lack of work experience in the years immediately following school, one for young women and the other for young men. In addition, both are first cousins of the chapter by Meyer and Wise (9) which examines the effects of high school work experience, vocational education, and academic achievement on later success in the labor market and post-secondary school attendance. It would have been helpful if these chapters could have been considered and discussed together. We should be trying to compare and contrast the findings to see if they vary because (1) different groups are being examined, (2) a slightly different question is being asked, or (3) different data sources or methodologies are being used. For example, it may be that scarring effects are different for men and women because the returns to female experience are smaller for the usual reasons identified in the human capital literature. Alternatively, these differences may reflect variations in data sources and methodologies across the two studies.

As an aid to those who are only interested in the bottom line, let me attempt to summarize the major findings of each of these papers without attempting to disentangle fully the reasons for any differences noted.

Meyer and Wise find that the relationship between work experience in high school and later employment and wages is large and permanent. However, the interpretaion of this finding is clouded, as the authors point out, because of the heterogeneity problem. As Chamberlain puts it, we

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don't know whether early labor market experience is doing something to a person or telling us something about the person. On the other hand, the authors find that after controlling for heterogeneity, work experience after high school is only weakly related to later employment, a conclusion that is consistent with both Ellwood and Corcoran.

Ellwood and Corcoran explain in detail the scarring hypothesis. Inability to control for individual heterogeneity has generated considerable skepticism about almost all past research which has purported to show a causal relationship between teenage joblessness and later success in the labor market.

Both Ellwood and Corcoran have adopted sophisticated methodological detours around this problem. As a result, their work represents a giant step forward in enabling us to document the longer-term consequences of early labor market experiences. To those who argue that youth unemployment is a nonproblem because it disappears with age and has no lingering effects, we can now say with some confidence, "You are wrong."

Specifically, Ellwood finds that the effects of early experience on the later employment of young men are not very significant but that the effects on wages are quite large and clear-cut. Corcoran finds that the employment effects for young women are larger than in the case of young men but that the wage effects are somewhat smaller. This latter finding is not too surprising when one realizes that it is based on historical data covering a period when women were tracked into jobs with flat age-earnings profiles. In fact, I was surprised that the wage effects were as significant and persistent as they were and am inclined to believe this is because heterogeneity was only crudely controlled for in these particular regressions.

I would now like to make some comments about remaining research gaps. First, we need to know why the relationship between early experience and later wages exists. It could be a simple human capital building effect or it could be a signalling or credentialling effect (reducing the power of age- or sex-based statistical discrimination). The policy implications of these different explanations vary. If human capital building is the key, then providing learning experiences or on-the-job training becomes critical. On the other hand, if labeling is what matters, then on-the-job training is not necessarily important, but providing private as opposed to subsidized public sector experience may be. In my own work, I am running into a lot of ancedotal evidence suggesting that employers discount the value of government work experience programs. Participants in these programs wear a "damaged goods" label.

A second research need is for more information on *noneconomic* consequences. Here there are some clear-cut sex differences in the predicted outcomes of teenage unemployment. We hypothesize that adolescent females who lack job opportunities will overinvest in *early* childbearing, which is known to have various adverse effects, including some which are intergenerational. For males, the expected outcome is a higher incidence of crime. In both cases, a relationship is known to exist, but the direction and extent of causation, if any, are not established. Yet documenting these effects is more important than in the case of economic outcomes because they involve greater externalities. This is precisely why they loom so large in popular discussions of youth unemployment.

As a final and related comment, I want to note that the justification for special intervention efforts on behalf of youths rests somewhat uneasily on the economic consequences alone. If the long-term opportunity costs (lower wages) are voluntarily paid because of young people's preferences for leisure or nonmarket work, then society should not intervene except perhaps to inform young people what the costs are so they can make more informed judgments. Unfortunately, there is still a lot of debate and uncertainty about how to distinguish between voluntary and involuntary joblessness, especially in the case of youths.

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