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9 High School Preparation and Early Labor Force Experience

Robert H. Meyer and David A. Wise

Many kinds of preparation and experience are presumed to prepare youths to find jobs, to perform them, and to keep them. At least three are often mentioned. One is general academic education: reading, writing, arithmetic. A second is vocational training intended to develop the skills necessary to perform particular tasks. A third is work experience itself, emphasized as the way to learn what it is like to work, to acquire the habits and attitudes that persons who work have, that draw one to want to work, and that those who hire want to find in those they pay. Motivated by these common hopes, we have investigated the relationships between early labor force experience and the three kinds of high school preparation that emphasize them. This chapter analyzes the relationship between high school curriculum, work experience, and academic achievement on the one hand and early labor force employment and wage rates on the other. We find that work experience acquired while in high school is strongly related to later employment. Academic performance in high school is also related to successful labor market experience. But we find no significant effect of current forms of high school vocational training on early labor force experience. Thus the weight of our evidence implies that programs that emphasize work experience for youths, together with general academic education, have the greatest chance of enhancing their subsequent labor force experiences.

Our analysis is based on male youths who graduated from high school. A large number of young persons enter the labor force immediately upon graduation from high school. Many receive no further formal education.

Robert H. Meyer is a graduate student at Harvard University. Elizabeth Philipp put together many summary tabulations for this chapter. The authors have benefited from the comments of Gary Chamberlain, David T. Ellwood, Richard B. Freeman, Zvi Griliches, Steven Venti, and other members of the NBER youth unemployment group.

For these youths, as well as those who continue their education, high school preparation is a potentially important determinant of early labor force experience. Because our study is limited to high school graduates, its implications for high school dropouts must be indirect. Among all groups of youths, high school dropouts, and in particular black school dropouts, have the poorest labor force experiences. Nonetheless, labor force statistics suggest a high youth unemployment rate, even among high school graduates. And our results for high school graduates, we think, have strong implications for future generations of persons who might contemplate dropping out of high school.

Our analysis is based on data collected by the National Center for Educational Statistics through the National Longitudinal Study of 1972 High School Seniors. The study collected a wide range of school, family background, and attitude and aspiration information from approximately 23,000 high school seniors in the spring of 1972. The 1972 base survey was based on a nationwide sample of high schools, stratified in such a way that schools in lower socioeconomic areas were somewhat oversampled. In addition to the base survey, the study included three follow-up surveys in 1973, 1974, and 1976. The follow-up surveys were used to obtain information on post-secondary school and work choices as well as labor force experiences.[1] Unlike most other data sources, this one allows us to follow a single cohort in their transition from school to work.

Most male youths in the years immediately after high school are either in the labor force or are attending a post-secondary school; some are in the labor force and going to school. Because the labor force aspirations of persons in school, their labor force behavior, their access to the labor market, and thus their realized experiences are likely to differ substantially from persons who are not in school, we have sought to obtain estimates that represent the experience that we would expect to find among persons not in school. To obtain such estimates, however, we must consider simultaneously both the decision to enter the labor force rather than go to school, and the expected experience of those who enter the labor force. In a strictly statistical sense, this may be thought of as correcting for sample selection bias. But in our case, the determinants of school attendance, as well as the determinants of labor force experience, are of considerable substantive interest. In fact, the decision to attend school may be expected to be determined in part by expected labor force experience. Although our primary emphasis will be on labor force experience, we will give some attention to the determinants of school attendance as well. The outline of the chapter is preceded by a summary of our major findings.

We have found a strong relationship between hours of work while in high school and weeks worked per year upon graduation. Persons who work while in high school also receive higher hourly wage rates than those who don't. The combined effect on earnings is very substantial. For example, with other individual characteristics equal to the average in the sample, persons who worked 16 to 20 hours per week in high school are estimated to earn annually about 12% more than those who didn't work at all in high school. Depending upon the amount of work in high school and estimated weeks worked based on other characteristics, the estimated "effect" on annual earnings of high school work could be as high as 30 or 35%. On the other hand, we find almost no relationship between any measure of high school vocational training and later weeks worked or wage rates. This has led us to raise the possibility that programs that emphasize work experience in high school may well have a greater impact on later labor market experience than programs that emphasize job skill training without work experience. Our evidence, however, establishes only a strong correspondence between work while in high school and later employment; it cannot be used to infer a cause-and-effect relationship of the same magnitude.

Traditional measures of academic achievement are also positively related to early success in the labor market. In particular, class rank is related to both weeks worked after graduation and to wage rates, after controlling for test scores reflecting a combination of aptitude and achievement. Combined with the results on hours worked in high school, this implies to us a substantial carry-over to the labor market of individual attributes associated with or developed through work effort in and out of school. Class rank may also measure general academic knowledge, and together with the positive estimated effect of test scores on both weeks worked and wage rates implies a significant effect of traditional measures of academic aptitude and achievement on labor market performance upon leaving school. Thus both high school academic performance and work experience seem to dominate specific vocational training as preparation for successful early experience in the labor market.

In contrast to the lasting relationship between high school work experience on weeks worked and wage rates over the next four years, there is little relationship between random—as distinct from individual specific—determinants of weeks worked in the first year after graduation and weeks worked four years later, and little relationship between random determinants of wage rates upon graduation and wage rates four years later. After controlling for individual specific characteristics, we find little lasting effect of unusually few weeks worked in the first year or two on weeks worked three or four years later. Similarly, after controlling for individual specific terms, we find little lasting effect of random fluctuations in initial wage rates on wages four years later. Whatever the determinants of wages and weeks worked, other than individual specific attributes, they do not lead to long-run persistence of initial experience. (On the other hand, wage rates increase with job experience so that weeks not working contribute to lower wage rates in the future.) And much if not most of work while in high school, which has a substantial positive relation to later labor market experience, must have been on jobs with limited direct relation to future job ladders, although our data do not provide any indication of the quality of high school jobs. Thus our findings suggest that the frequently expressed worry that poor initial jobs and initial jobs without a future should be avoided for fear that they will contribute to lasting poor labor force experience, may be misplaced. Our evidence on persons graduating from high school suggests, albeit indirectly, that this worry is unfounded and in fact should give way to policies to encourage early work experience, possibly without exaggerated initial concern for its relationship to a well-defined hierarchy of future jobs. We find no evidence that persons on average were hindered by the work experience they had in high school; on the contrary, the evidence suggests that they may well have been helped. And our evidence shows that low-wage jobs after graduation do not in themselves increase the likelihood of low-wage jobs a few years later.

We have distinguished weeks worked in the four years following high school by the year in which the experience was had. Thus after four years, for example, we know how much an individual worked in each of the three preceding years. As expected, we find that wage rates at any given date are determined in part by previous experience. Thus although there is no lasting effect of nonemployment in one year on employment in subsequent years, there is a cost associated with early nonemployment, namely, lower wages in future years. The effect of early labor force experience on subsequent wages is not obviously different in magnitude from the effect of work experience while in high school. But the effect of work experience while in high school does not decline over the first five years in the labor force, whereas there is some evidence that the effect of early labor force experience on subsequent wages may decline over time. Thus high school work experience may be capturing attributes that are in part at least distinct from those associated with later labor market experience. The pattern of the relationships between work while in high school and weeks worked in subsequent years in the labor force provides further evidence of this. Indeed, the latter finding suggests strongly that high school work experience is associated with individual attributes that persist over time.

The average wage rates of whites and nonwhites in the labor market are quite close, with whites earning a bit more per hour after the first year. But after controlling for other variables, nonwhites seem to earn a bit more per hour than whites. On the other hand, nonwhites work fewer weeks per year than whites on the average, but we find little difference between the two groups after controlling for other variables. After controlling for other variables, the probability that nonwhites are in school in each of the four years after high school is about 0.10 higher than the corresponding probability for whites.

In general, summary statistics based on the National Longitudinal Study do not suggest severe employment problems for these high school graduates. On the contrary, they suggest a group of persons moving rather smoothly into the labor force.

Finally, employment ratios of both white and nonwhite high school graduates based on these data are considerably higher than those calculated from Current Population Survey data, and unemployment rates much lower. Although employment ratios of nonwhites are lower than those of whites, and unemployment rates higher, four years after high school graduation they are close. The October 1976 white employment ratio was .909 and the nonwhite ratio .875; unemployment rates were .065 and .081 respectively. Very few persons in the sample were chronically out of school and unemployed.

The general outline of the chapter is as follows: section 9.1 contains some general descriptive statistics on the transition from school to work. Empirical estimates of weeks worked and wage equations are presented in section 9.2, and are accompanied by nonschool attendance equations. Section 9.3 is an analysis of the extent of persistence of individual experience over time. Concluding remarks are contained in section 9.4.

9.1 Some Descriptive Statistics on the Transition from School to Work

Through the National Longitudinal Study of 1972 High School Seniors, data were obtained on almost 23,000 persons from over 1,300 high schools. The high schools were a stratified sample of all public, private, and church-affiliated schools in the country. To increase the number of "disadvantaged" students in the sample, high schools located in low-income areas and schools with a high proportion of minority enrollment were sampled at approximately twice the sampling rate used for the other schools. The summary statistics reported below have not been adjusted to reflect population proportions. They are reported, however, separately for whites and nonwhites. Both groups probably reflect more persons from low-income families than would be found in a random sample of the population.

We will present summary statistics in three groups: the first on work and school status by year; the second on the likelihood of selected sequences of school and work status over time; and the third on weekly earnings and hours worked and annual employment and by year.

9.1.1 School and Work Status by Year

The distribution of white and nonwhite males in the survey sample by school and work status, together with some summary labor force statistics, is shown in table 9.1. (More detailed distributions by nine school and five work classifications are presented for five consecutive Octobers beginning in 1972 in tables available from the authors.) We will point out first some general findings based on an examination of table 9.1 and then indicate the kind of detail that can be found in the more detailed tables without presenting an extensive discussion of it.

The most striking statistics in table 9.1 are the comparatively low unemployment rates and high employment ratios, as compared with those based on Current Population Survey data (see Freeman and Medoff, chapter 3 of this volume). Although we cannot provide a direct comparison for each October, we can for 1972. In October of 1972, the Census Bureau conducted a special survey of spring 1972 high school graduates (see Bureau of Labor Statistics [1973], p. 27). A comparison of unemployment and other labor force statistics based on the two data sources is presented in the tabulation below (for persons not in school).

Statistic	National Longitudinal Study		Current Population Survey, October 1972	
	White	Nonwhite	White	Nonwhite
Employment ratio	.880	.784	.815	.680
Labor force participation	.929	.902	.916	.880
Unemployment rate	.054	.130	.110	.227

An investigation of the definitions used in the two surveys does not reveal any differences that would suggest such apparently contradictory results, although the survey questions are not identical. Although the NLS survey is weighted to oversample low income youths, this should tend to raise implied unemployment rates, not to lower them. The survey respondent, however, in the NLS survey is the individual youth, but is likely to be the mother or father of the youth in the CPS survey. The NLS data is collected through a mailed questionnaire (together with some mail and telephone reminders), while the CPS data is obtained by interview with a household member, often the female head. Freeman and Medoff find that a large portion of the difference between the CPS numbers and those based on the Parnes National Longitudinal Survey can be attributed to the different respondents.

The summary statistics also reveal several differences between white and nonwhite youths. The percentage of white youths in school full time is approximately 12 percentage points higher than the percentage of nonwhites until 1976, when many youths would have finished four years of college. Of those not in school, the percentage working full time is about 8 to 10 points higher for whites than for nonwhites. By 1976, the percentages were about 80 and 72 respectively. The proportions working

Table 9.1 Percentage of Male Youths in School and Work Categories and Labor Force Statistics, by Year and Race, October of Each Year

	White					Nonwhite				
	1972	1973	1974	1975	1976	1972	1973	1974	1975	1976
In school full time[a]	53.6	43.3	38.2	35.2	22.1	42.3	30.3	26.5	23.3	17.7
In school part time	4.6	7.2	6.2	6.6	7.7	4.4	7.4	6.5	7.4	7.0
Not in school, total	42.4	49.5	55.7	58.2	70.2	53.3	62.3	67.1	69.4	75.3
Working full time	71.9	76.2	74.1	77.5	80.1	60.1	67.7	63.3	68.7	71.9
Working part time	9.2	5.1	4.9	4.2	4.1	11.4	6.2	5.1	5.2	5.1
Military	7.7	11.2	11.9	10.8	7.4	8.8	14.2	16.1	15.7	12.1
Out of labor force	6.6	4.5	2.3	2.7	2.7	9.0	6.2	2.9	3.1	4.1
Looking for work	4.6	2.8	6.7	4.7	5.7	10.7	5.8	12.6	7.3	6.9
Labor force statistics:[b]										
Employment ratio	.880	.914	.898	.916	.909	.784	.860	.840	.877	.875
Labor force participation	.929	.946	.974	.969	.972	.902	.928	.965	.964	.953
Unemployment ratio	.053	.035	.079	.053	.065	.130	.073	.155	.090	.081

[a]Includes a small number of persons in graduate school in 1975 and 1976.

[b]For persons not in school and not in the military.

part time do not differ substantially in any of the years, although in each year the percentage for nonwhites is somewhat higher than for whites. It declines between 1972 and 1976 from 9.2 to 4.1 for whites and from 11.4 to 5.1 for nonwhites. A larger proportion of nonwhites than whites is in the military. In 1974, the year of highest military participation for both groups, about 12% of whites and 16% of nonwhites were in the armed forces.

More blacks than whites are out of the labor force, but the differences are not large. The proportion "looking for work," however, is about twice as high for nonwhites as for whites in 1972 through 1974. The differences decline in 1975 and 1976. The percentage of whites looking for work in 1976 was 5.7 versus 6.9 for nonwhites.

The labor force participation rates are high for both groups and do not differ substantially. Between 1972 and 1976 they moved from .93 to .97 for whites and .90 to .95 for nonwhites. The employment ratio is higher for whites than nonwhites in 1972, .88 versus .78; but by 1976 the two ratios were much closer, .91 versus .88. This closing of the gap between the two groups is reflected in the unemployment rate which was more than twice as high for nonwhites as for whites in 1972; but by 1976, the two rates were rather close, .065 versus .081.

In short, these numbers suggest a cohort of youths moving rather smoothly into the labor force. Although there are differences between the statistics for whites and nonwhites, they do not seem to us to be striking. In particular, the unemployment rates, although higher for nonwhites than whites, are not shocking to us for either group in any year. By 1976, somewhat more than four years after graduation from high school, labor force participation and employment ratios are high for both groups and the unemployment rates are modest for both groups. Youth unemployment does not appear from these data to be a severe problem for this group of high school graduates.

From the statistics in the tables available from the authors, one can find more detail within this more general picture. For example, it can be seen that most youths who are working part time are also in school full time, although the proportion is lower for nonwhites than for whites. Also, many persons looking for work are full-time students. They were not included in the unemployment statistics reported above.

9.1.2 Sequences of School and Work Status

The average statistics reported above do not reveal extremely high unemployment rates. But it could be that there are some youths who are often unemployed. As a worst case, we have lumped together the persons out of the labor force with those who are unemployed. Table 9.2 reports the percentage of persons not in school and not working (in either civilian or military jobs) for each possible number and sequence of time periods. For example, the sequence 10101 indicates not in school and not working

Table 9.2 **Percent of Male Youths Not in School and Not Working, October 1972–76, by Sequence and Race[a]**

	Percentage of total					
Sequence	All males		White		Nonwhite	
11111	0.1	0.1	0.1	0.1	0.1	0.1
11110		0.1		0.0		0.3
11101		0.0		0.0		0.1
11011	0.3	0.0	0.1	0.0	0.5	0.0
10111		0.0		0.0		0.1
01111		0.1		0.1		0.4
11100		0.2		0.1		0.6
01110		0.1		0.1		0.1
00111		0.2		0.2		0.5
11010		0.0		0.0		0.0
11001	1.0	0.1	0.8	0.1	2.0	0.3
01101		0.0		0.0		0.0
10110		0.1		0.0		0.2
10011		0.1		0.1		0.3
01011		0.1		0.1		0.1
10101		0.0		0.1		0.0
11000		0.6		0.6		1.0
01100		0.5		0.0		1.0
00110		0.5		0.4		0.9
00011		0.5		0.0		0.7
10100		0.3		0.3		0.4
01010	3.1	0.1	3.1	0.1	6.3	0.1
00101		0.4		0.3		0.9
10010		0.2		0.1		0.5
01001		0.2		0.2		0.4
10001		0.2		0.2		0.3
10000		3.1		2.3		5.4
01000		1.9		1.8		2.5
00100		2.7		2.5		3.9
00010	14.1	2.5	13.2	2.3	18.6	3.2
00001		3.9		3.9		3.7
00000	81.0	81.0	82.7	82.7	72.2	72.2
Total		9115		7639		1475
Missing		2052		1448		522

[a]The percentages have been rounded to the nearest tenth. Differences between the sum of the numbers in the groups and the group totals reported to the left in each column are due to rounding. A "1" indicates not in school and not working. The left digit pertains to October 1972.

in October 1972, October 1974, and October 1976, but not in this category in October 1973 or in October 1975. The left digit pertains to 1972.

Examination of table 9.2 reveals that 81% of the sample were *not* in this category in *any* of the five October periods (the data pertain to the first full week in October of each year). Only one-tenth of one percent were out of school and not working in all of the periods.[2] For whites and nonwhites together, this represents five persons out of 9115. Three-tenths of one percent were in this category four out of the five periods, and one-tenth of one percent in three out of the five. Only 14% were so classified in one of the five periods. We do not find a large group of chronically not in school and not working youths. More nonwhites than whites were in this status for one, two, three, and four periods; but over 72% of nonwhites were never out of school and without work in these October periods. These data do suggest, however, that some youths are much more likely to be in this category than others; there is heterogeneity among the group. For example, according to table 9.2 about 5 % of white youngsters are in this category in any year. If a person had a .05 probability of being in this category in any period and the probabilities were independent over time, the likelihood of being in this status three out of the five periods, for example, would be only .001, much less than the observed proportion of .008 for all white males.

Similarly defined sequences and associated percentages for full-time school and full-time work are reported in tables 9.3 and 9.4 respectively. Table 9.3 figures reveal that 36% of the sample were never in school full time, 35% for whites and 44% for nonwhites. (While these numbers suggest that whites are more often in school than nonwhites, the estimates below of the probability of attending school suggest a higher probability for nonwhites than whites after controlling for other relevant variables such as test scores and family background.)

Although there is some movement into and out of school, it is not the norm. Of persons who go to school at all, 69% begin in the first year after high school and attend only in consecutive years. Eighty-four percent of those who attend at all attend during the year immediately after high school. The in and out possibility that is sometimes emphasized, possibly more often for older persons, is not the norm among this group.

While 36% of the sample were never in school full time, only 24% worked full time in each of the five periods, as can be seen in table 9.4. As could be inferred from the school attendance figures, we see in table 9.4 that a relatively large number of persons work the last four, the last three, the last two, or the last year, but none of the prior years.

9.1.3 Weekly Earnings and Hours, Annual Employment and Unemployment, and Number of Employers

Average hourly wage rates, weekly earnings, and weekly hours worked for persons not in school and for those in school are shown in table 9.5.

Table 9.3 Percentage of Male Youths in School Full Time, October 1972–76, by Sequence and Race[a]

	Percentage of total		
Sequence	All males	White	Nonwhite
11111	11.7	12.4	8.1
11110	12.1	13.2	6.3
11101	1.3	1.3	1.2
11011	1.2	1.1	0.6
10111	1.2	1.2	1.3
01111	1.0	1.0	1.0
11100	3.8	3.9	3.3
01110	0.6	0.6	0.4
00111	0.5	0.5	0.5
11010	0.8	0.8	0.5
11001	0.4	0.5	0.3
01101	0.2	0.2	0.1
10110	1.1	1.1	0.9
10011	0.6	0.6	0.5
01011	0.2	0.2	0.1
10101	0.2	0.2	0.2
11000	7.1	7.0	7.3
01100	0.9	0.9	0.8
00110	0.5	0.5	0.7
00011	0.8	0.7	1.0
10100	1.2	1.1	1.2
01010	0.1	0.1	0.1
00101	0.1	0.1	0.1
10010	0.7	0.7	0.6
01001	0.1	0.1	0.1
10001	0.6	0.6	0.7
10000	9.5	9.2	11.2
01000	1.6	1.6	1.7
00100	1.5	1.4	1.9
00010	1.1	1.0	1.5
00001	1.3	1.2	2.0
00000	36.4	34.9	44.0
Total	9152	7659	1492
Missing	2052	1428	505

[a]The percentages have been rounded to the nearest tenth. Differences between the sum of the numbers in the groups and the group totals reported to the left in each column are due to rounding. A "1" indicates in school full time. The left digit pertains to October 1972.

Table 9.4 Percentage of Male Youths Working Full Time, October 1972–76, by Sequence and Race[a]

	Percentage of total		
Sequence	All males	White	Nonwhite
11111	23.7	23.7	24.1
11110	1.5	1.6	1.3
11101	1.2	1.1	1.5
11011	2.8	2.7	2.9
10111	2.9	2.9	3.2
01111	9.7	9.5	10.7
11100	1.0	0.9	1.5
01110	1.0	1.0	1.3
00111	7.6	7.2	9.7
11010	0.4	0.3	0.6
11001	0.8	0.8	0.9
01101	0.7	0.7	1.0
10110	0.5	0.4	0.9
10011	1.2	1.2	1.3
01011	1.8	1.7	2.1
10101	0.3	0.3	0.5
11000	1.0	0.9	1.2
01100	0.7	0.6	0.7
00110	1.3	1.3	1.2
00011	4.5	4.3	5.5
10100	0.4	0.4	0.3
01010	0.3	0.3	0.4
00101	1.4	1.4	1.4
10010	0.2	0.2	0.3
01001	0.9	0.9	0.9
10001	0.7	0.7	0.7
10000	1.2	1.2	1.3
01000	1.6	1.5	1.8
00100	1.6	1.6	1.4
00010	1.5	1.5	1.6
00001	9.9	10.6	6.5
00000	15.6	16.4	11.4
Total	9208	7689	1518
Missing	1959	1398	479

[a]The percentages have been rounded to the nearest tenth. Differences between the sum of the numbers in the groups and the group totals reported to the left in each column are due to rounding. A "1" indicates working full time. The left digit pertains to October 1972.

Table 9.5 **Average Hourly Wage Rates, Weekly Earnings, and Weekly Hours Worked for Persons Working in October, by School Status, Race, and Year[a]**

	Out of school					In school				
Item and race	1972	1973	1974	1975	1976	1972	1973	1974	1975	1976
Hourly wage rate										
All males	2.67	3.18	3.69	4.14	4.56	2.34	2.66	3.06	3.49	4.04
	(3040)	(3752)	(4199)	(4039)	(4950)	(2153)	(2223)	(1892)	(1725)	(1347)
White	2.68	3.22	3.73	4.17	4.61	2.34	2.65	3.00	3.47	4.06
	(2471)	(3001)	(3403)	(3271)	(4097)	(1866)	(1938)	(1625)	(1473)	(1162)
Nonwhite	2.67	3.02	3.53	3.99	4.35	2.49	2.74	3.42	3.59	3.91
	(550)	(731)	(796)	(753)	(841)	(276)	(273)	(267)	(248)	(181)
Weekly earnings										
All males	108.68	133.61	154.21	173.48	192.15	62.96	74.34	89.25	103.77	130.80
White	110.32	137.04	157.41	176.47	195.52	61.85	73.43	86.94	101.47	129.74
Nonwhite	101.69	119.79	140.53	160.85	176.18	69.29	78.93	103.29	115.99	136.48
Weekly hours worked										
All males	41.50	42.79	42.47	42.53	42.63	26.83	27.47	27.95	28.88	31.45
White	41.90	43.21	42.80	42.86	42.95	26.57	27.24	27.58	28.39	31.06
Nonwhite	39.65	41.14	41.02	41.09	41.09	28.09	28.62	30.20	31.60	33.73

[a]The data pertain to the first full week in October of each year. The numbers reporting figures in each year are in parenthesis under the wage rates. They are the same for weekly earnings and weekly hours worked.

They cover all persons in the sample who were working in the first full week of October of the year indicated. Persons working full time or part time are included. For persons out of school, wage rates for the two groups are virtually identical right after graduation. After four years, whites earn about 6% more per hour than nonwhites, presumably, in part at least, because of the different schooling patterns of the two groups and post-high school work experience. Nonwhites also work about two hours per week less than whites in each of the time periods and thus have lower weekly earnings—about 8% in the first year and 10 or 11% in each of the subsequent years.

On the other hand, nonwhites who are in school work 1.5 to 3 hours per week more than whites, earn somewhat more per hour in all but the last period, and have higher weekly earnings in each of the periods between 5 and 19% depending on the period.

We also calculated the percentage of persons with wage rates below the federal minimum. The results of October of each year are shown in the tabulation below.

Year	Minimum wage rate	Percentage below minimum: Total	White	Nonwhite
1972	$1.60	10.98	11.02	10.76
1973	$1.60	5.89	5.93	5.59
1974	$2.00	8.06	8.35	6.68
1975	$2.10	8.14	8.14	7.99
1976	$2.30	5.76	5.39	7.73

These numbers presumably reflect in large part wages of persons in jobs exempt from minimum wage legislation.

Average annual weeks worked, weeks looking, weeks out of the labor force, and number of employers, by school status, are shown in table 9.6. Among persons out of school, nonwhites work fewer weeks per year than whites, but the difference declines continuously over the four-year period. Nonwhites work 13% less in the first year, 10% in the second, 7% in the third, and 5% in the fourth. The differences are accounted for by both weeks looking for work and weeks out of the labor force. Differences among whites and nonwhites in school are somewhat less in general, although as among persons not in school nonwhites who are in school spend more weeks than whites looking for work.

9.2 High School Training and Labor Force Experience

Our goal is to estimate the effects of personal characteristics, particularly high school preparation, on labor force experiences in the years following high school graduation. The measures of labor force experience we shall use are weeks worked and wage rates. We have annual weeks worked for four years following high school graduation and wage rates for five consecutive October periods, as described above. We have estimated a weeks-worked equation separately for each of the four years and a separate wage equation for each of the five October periods. Jointly with each of the weeks-worked and wage equations we have estimated a "school nonattendance" equation, that is, the probability of being in the sample, and thus having recorded wage or weeks worked measures as defined below. We have followed this procedure in the first instance to correct for possible bias in the parameters of the weeks-worked and wage equations. But the school nonattendance equations have a behavioral interpretation in this case and the associated parameter estimates are of interest apart from their relationship to the weeks-worked and wage equation estimates. In addition, the procedure we have used to estimate weeks worked accounts for the upper limit of fifty-two weeks in a year. A large proportion of respondents report working a full fifty-two-week year. Parameter estimates obtained without recognizing this limit tend to underestimate the effects of explanatory variables on weeks worked. (An analogy would be the effect of knowledge about a subject on an examination score in that subject if the exam is very easy. After some level of knowledge, more doesn't help. You can't score above 100.) Thus we have combined a tobit specification for weeks worked with a probit school nonattendance specification. Finally, in section 9.3 we shall discuss the relationships between weeks worked and wage rates over time.

A more precise description of the approach we have followed to estimate weeks worked is presented in section 9.2.1 below. The variant of this procedure used to estimate wages is described in section 9.2.2. The results are then discussed in turn, beginning with estimates of the probability of school attendance, followed by parameter estimates for the weeks-worked and wage rate equations.

9.2.1 The Weeks Worked Estimation Procedure

Suppose that weeks worked in each of four years are indicated by Y_1 through Y_4. Assume also that in each period there are vectors of "exogenous" variables X_1 through X_4. In practice, these vectors will be composed largely of variables like test scores and family background that do not change over time, although some like schooling and work experience do. Let the relationships between weeks worked and the exogenous variables for individuals in the population, should they decide to work, be described by

$$
\begin{aligned}
Y_{1i} &= X_{1i}\beta_1 + \varepsilon_{1i}, \\
Y_{2i} &= X_{1i}\beta_2 + \varepsilon_{2i}, \qquad (1) \\
&\vdots \\
Y_{4i} &= X_{4i}\beta_4 + \varepsilon_{4i},
\end{aligned}
$$

Table 9.6 Average Annual Weeks Worked, Weeks Looking for Work, Weeks out of the Labor Force, and Number of Employers for Male Youths, by School Status, Race, and Year[a]

Item and race	Out of school 1972–73	1973–74	1974–75	1975–76	In school 1972–73	1973–74	1974–75	1975–76
Weeks worked								
All males	39.91	43.32	43.66	44.08	28.17	30.03	30.82	31.55
	(4374)	(4214)	(5031)	(5470)	(5541)	(4253)	(3923)	(3708)
White	41.05	44.18	44.26	44.52	28.55	30.22	30.86	31.54
	(3433)	(3364)	(4087)	(4424)	(4742)	(3703)	(3432)	(3236)
Nonwhite	35.77	39.91	41.05	42.14	25.70	28.78	30.58	31.60
	(899)	(850)	(944)	(1030)	(779)	(549)	(490)	(463)
Weeks looking for								
All males	3.57	3.00	3.78	3.51	2.21	2.16	2.77	2.90
	(3960)	(3941)	(5061)	(5470)	(5140)	(3987)	(3913)	(3730)
White	3.06	2.77	3.60	3.42	1.98	1.99	2.59	2.75
	(3112)	(3158)	(4091)	(4411)	(4438)	(3462)	(3418)	(3251)
Nonwhite	5.55	3.93	4.54	3.88	3.70	3.25	4.03	3.92
	(807)	(783)	(970)	(1042)	(683)	(524)	(494)	(470)

Weeks out of the labor force								
All males	8.39	5.86	4.47	4.25	21.73	20.04	18.41	17.58
	(3925)	(3899)	(4898)	(5313)	(5093)	(3948)	(3815)	(3646)
White	7.72	5.17	4.02	3.89	21.59	20.00	18.59	17.71
	(3089)	(3131)	(3980)	(4303)	(4408)	(3431)	(3339)	(3186)
Nonwhite	10.79	8.66	6.39	5.84	22.84	20.32	17.09	16.63
	(795)	(768)	(918)	(994)	(276)	(516)	(475)	(451)
Number of employers								
All males	1.85	1.59	1.43	1.41	1.76	1.64	1.43	1.56
	(4342)	(4409)	(5306)	(5732)	(5496)	(4399)	(4019)	(3837)
White	1.89	1.60	1.43	1.41	1.78	1.67	1.43	1.57
	(3418)	(3509)	(4290)	(4611)	(4726)	(3822)	(3503)	(3335)
Nonwhite	1.72	1.55	1.43	1.41	1.66	1.46	1.43	1.47
	(884)	(900)	(1016)	(1105)	(748)	(576)	(515)	(493)

[a]The number of respondents is shown in parentheses under each average. The numbers for nonwhite and white may not add to the total because race is sometimes unknown.

where the ε_{ti} are random terms and the β_t vectors of parameters. It is important in our case that the β_t be allowed to vary. We do not want to restrict the influence of variables like high school work experience to be constant over time. On the contrary, we would like to see if their effects change, and if so, how.

Two groups of individuals are distinguished: those who are in school and those who are not. Persons included in our out-of-school group were not in school in either the October beginning the year nor in the following October. Although one might well consider the determinants of weeks worked for persons in either group, we will concentrate on those not in school. We judged that the labor market behavior of the two groups would be quite different and we did not want estimates that confounded the decisions of both.[3] Each of the equations (1) is presumed to describe the work experience of persons in the population should they decide not to go to school in the year indicated by the subscripts 1 through 4.

Suppose that there are four unobserved variables S_t, one for each of the four time periods. Define them by

$$
\begin{aligned}
S_{1i} &= Z_{1i}\delta_1 + \eta_{1i}, \\
S_{2i} &= Z_{2i}\delta_2 + \eta_{2i}, \\
&\vdots \\
S_{4i} &= Z_{4i}\delta_4 + \eta_{4i},
\end{aligned}
\tag{2}
$$

where the Z_t are vectors of exogenous variables, the δ_t are vectors of parameters, and the η_t are random terms. Let s_{ti} be an indicator variable with $s_{ti} = 1$ if the i^{th} individual is *not* in school in year t, and thus in the sample $s_{ti} = 0$ if he is. Also, let

$$
s_{ti} = \begin{cases} 1 \text{ if } S_{ti} \geq 0 \\ 0 \text{ if } S_{ti} < 0 \end{cases}
\tag{3}
$$

for t equal to 1, 2, 3, or 4. Then the probability that the i^{th} individual is not in school is given by $Pr(s_{ti} = 1) = Pr(S_{ti} = Z_{ti}\delta_t + \eta_{ti} \geq 0)$. And if η_{ti} is assumed to be normally distributed, we have for each period a probit specification of the probability of not being in school:[4]

$$
\begin{aligned}
Pr\ (s_{1i} = 1) &= \Phi[Z_{1i}\delta_1] \\
Pr\ (s_{2i} = 1) &= \Phi[Z_{2i}\delta_2] \\
&\vdots \\
Pr\ (s_{4i} = 1) &= \Phi[Z_{4i}\delta_4]
\end{aligned}
\tag{4}
$$

We know that estimation of any of the Y_{ti} equations in (1), based only on persons not in school in year t, will yield biased coefficient estimates if ε_{ti} and η_{ti} are correlated.[5] We could correct for this potential bias by estimating jointly for each year the weeks-worked equation and the corresponding choice-of-status, or school attendance, equation.[6]

In our case, however, the upper limit on weeks worked has an important effect on the estimates of ß in equation (1) and thus on the interpretation of the relationship between preparation in high school and post-high school labor force experience. The percentage distribution of weeks worked for persons not in school by selected interval is shown for each of the four years in the tabulation below.

Weeks worked interval	Percentage distribution 1973	1974	1975	1976
0 to 10	5.2	5.3	4.6	4.6
11 to 20	4.8	3.1	3.6	3.5
21 to 30	8.0	5.9	5.7	5.5
31 to 40	12.5	12.0	9.9	10.2
41 to 51	30.3	32.3	24.2	24.7
52	39.2	41.4	52.0	51.5

The percentage reporting fifty-two weeks of work ranges from 39% in 1973 to 52% in 1976. It is apparently the case that many persons are prepared to work, and have work opportunities that exceed the time available constraint as measured in weeks.[7]

Thus we have changed the specification in equation (1), interpreting capital Y_i as an unobserved "propensity" to work, with observed weeks worked given by

$$(5)\qquad y_{1i} = \begin{cases} X_{1i}\beta_1 + \varepsilon_{1i} & \text{if } X_{1i}\beta_1 + \varepsilon_{1i} \leq 52, \\ 52 & \text{if } X_{1i}\beta_1 + \varepsilon_{1i} > 52, \end{cases}$$
$$\vdots$$
$$y_{4i} = \begin{cases} X_{4i}\beta_4 + \varepsilon_{4i} & \text{if } X_{4i}\beta_4 + \varepsilon_{4i} \leq 52, \\ 52 & \text{if } X_{4i}\beta_4 + \varepsilon_{4i} > 52. \end{cases}$$

The maximum likelihood procedure we have used estimates ß in (5) jointly with δ in (2), for each of the four years individually. It is explained in more detail in the appendix. The relationship between the expected value of Y given by $X\beta$ and the expected value of weeks worked, $E(y)$, may be seen in figure 9.1, in which one right-hand variable is assumed. At low levels of $X\beta$, the estimated parameter ß represents the approximate effect of a change in X on the expected number of weeks worked. As X increases, its effect on weeks worked approaches zero.[8] For example, our results reported below suggest that ß is about twice as large as the derivative of $E(y)$ with respect to X, evaluated at $\bar{X}$. At the mean of the variables in our sample, the expected number of weeks worked is about forty-four, and the expected value of the unobserved Y is somewhat

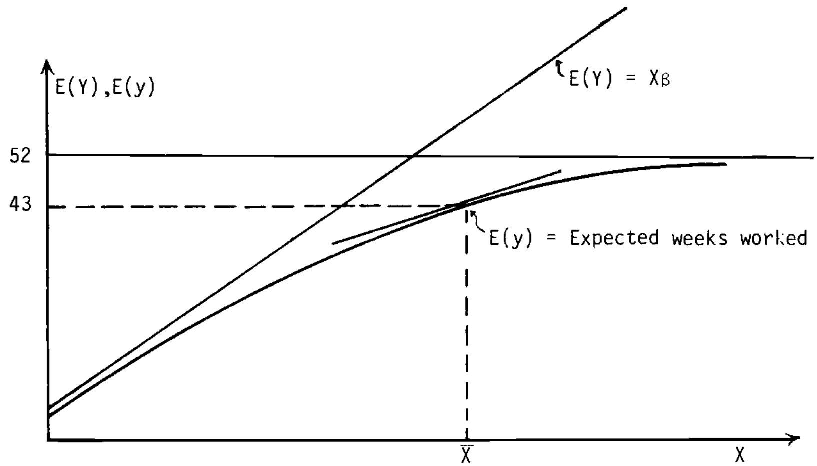

Fig. 9.1

greater than fifty-two. The derivative of $E(y)$ with respect to X at $\bar{X}$ is approximately equal to the estimate of ß from a specification that does not distinguish employment at the limit of fifty-two weeks from observations below fifty-two.

There were also a few persons each year who did not work at all. We obtained some initial estimates that accounted for this by specifying weeks worked to be bounded at zero, as well as at fifty-two. It did not significantly affect our results and we did not incorporate it in the results presented below.

An alternative to separate estimates for each year is to divide the sample into two groups: one composed of persons who were never in school, and the other composed of everyone else. But for our purposes the procedure outlined above has at least two advantages over the alternative. First, it allows us to make use of as much of the data as possible.[9] Examination of table 9.3 shows that the number of persons out of school in all years is much smaller than the number in any single year.[10] Also, even if the group with weeks worked is defined in the alternative way, a sample selection correction must still be made to obtain unbiased estimates of the population parameters in the weeks-worked equations. This presumably would be done by estimating a probit equation pertaining to the probability of never being in school. Such an equation could be used to correct each of the weeks-worked equations for the sample selection bias.[11] But it is difficult to think of a behavioral interpretation for this sample selection equation, since in a given year one group includes persons who are in school as well as some who are not. Our status equations can be interpreted in each year as estimating the determinants of school attendance in that year.

9.2.2 The Wage Rate Estimation Procedure

Wage rate equations were also estimated jointly with school nonattendance equations. There are five wage equations, however, one for each of the October survey periods. But there is no limit problem as with weeks worked. Parameters in equations like (1) and (2) for weeks worked were estimated jointly, with the logarithm of the wage substituted for weeks worked.[12] There is, however, a complication that does not arise in the weeks-worked equation.

Wage rates are presumed to depend on years of schooling, as well as on other variables. For example, persons who were working in the fourth October period may have been in school during some or all of the previous periods, and their wage rates may be expected to depend on the amount of schooling. Suppose that the logarithm of the wage is given by

$$W_{4i} = X_{4i}\alpha_4 + a_1 A_{1i} + a_2 A_{2i} + a_3 A_{3i} + \nu_{4i}$$

where A_1 equals 1 if individual i was in school during period 1 and zero if not, and similarly for A_2 and A_3. The potential bias resulting from the possibility that $E(\nu_{4i} \mid s_{4i} = 1)$ may not be zero is corrected for by estimating the equation jointly with the probability of school nonattendance. But if the ν_t's are correlated with the error in the school nonattendance equations, and the η_t's or the ν_t's are correlated over time, then A_1 through A_3 may be correlated with the error ν_4 in the wage equation. To overcome this problem, we experimented with an instrument for prior schooling.[13] In practice, we found that the use of an instrument for schooling did not substantially alter the character of our conclusions.[14] A similar problem may pertain to work experience that is also assumed to determine the wage rate. We did not attempt to correct for it. (In subsequent work we will estimate a more appropriate model for solving this problem. It will allow joint estimation of weeks worked, wage, schooling, and a sample selection equation.)

Finally, the sample selection equations estimated with the wage rate equations are not precisely school nonattendance equations, although in practice the two are almost interchangeable. The weeks-worked equation for a given year included all persons who were not in school during that year. Thus the status or sample selection equations are equivalent to school attendance equations. Students were also excluded from the wage equations. But we do not have wage rates for all persons who were not students. Some nonstudents were also not employed. To correct the wage equation for sample selection bias we need to consider all persons without a recorded wage rate, whatever the reason.[15]

The variables used in the analysis are defined below.

Weeks worked: Annual weeks worked, October to October.

Wage rate: Earnings divided by hours worked, first full week in October.

Test scores total: Sum of scores on six texts: vocabulary, reading, mathematics, picture-number, letter groups, mosaic comparisons.

Class rank in high school: Percentile ranking relative to other persons in individual's high school.

Job training in high school: One if the individual received in high school "any specialized training intended to prepare [him] for immediate employment upon leaving school (for example, auto mechanics, secretarial skills, or nurses aid)," zero otherwise.

Hours worked during high school: Response to the question, "On the average over the school year, how many hours per week do you work in a paid or unpaid job? (Exclude vacation.)" The response was by interval: 0, 1–5, 6–10, . . . , 26–30, over 30.

Parents' income: Annual income of parents, in thousands.

Education of mother (father) less than high school: One if the youth's mother (father) had less than a high school education and zero otherwise.

Education of mother (father) college degree or more: One if the youth's mother (father) had a college degree or more education, and zero otherwise. The excluded category is a high school degree but less than a college degree.

Race: One if nonwhite, zero otherwise.

Dependents: Number of persons dependent on the individual for income.

School years: Number of Octobers in which the individual said he was in school.

On the job training: Months of on-the-job training.

Experience: Work experience, in years. Excludes work while attending a post-secondary school. Experience is distinguished by the year in which it occurred.

Part-time working: One if the individual is working part time, zero otherwise.

Rural, urban: One if the individual's residence location corresponds to the one indicated, zero otherwise. The excluded category is suburban and town.

West: One if the person lives in the West, zero otherwise.

State wage: Annual average wage in manufacturing.

State unemployment: Average annual unemployment rate.

Missing variable indicators: For test scores, class rank, parents' income, experience. Each is one if the designated variable is missing and zero otherwise. The corresponding variable takes the value zero if it is missing and the recorded value if it is not.

The means and standard deviations of these variables are given in appendix table 9A.2.

9.2.3 School Attendance

Concomitant to the procedure used to estimate both the weeks worked and the wage equations, we estimated school attendance equations, or more precisely, the probability of not attending school. Before presenting results on the central questions of our analysis, we will summarize briefly the implications of the estimated attendance probability parameters.

School nonattendance equations estimated with the weeks worked-equations are presented in table 9.9; those estimated with the wage equations are shown in table 9.12. The two sets of parameter estimates are necessarily very similar. The discussion in this section is based on those estimated with weeks worked. Recall that the parameter estimates in table 9.9 are analogous to the parameters δ in equations (2) and (4). The variables used in the probability equations are easily identifiable by glancing at the table. They need no further explanation. The first two groups of variables pertain to school achievement and family background. All are measured with considerable precision, as shown in the table.

To get a better idea of the importance of the variables, however, we have calculated estimated differences in the probability of attending school for persons who have different values of a specified variable, but the same values for all the others. All other variables were assumed to have values equal to their respective means. The specified differences and the associated differences in estimated school attendance probabilities are shown in table 9.7.

Possibly the most notable finding is that the probability that nonwhites are in school, controlling for other variables, is considerably higher than for whites, at least .10 in each of the five years.[16] (Recall that the summary numbers in table 9.1 show that in each of the first four years following high school the percentage of nonwhites in school fulltime was between 11 and 13 percentage points less than the percentage for whites.) This could result from relatively fewer opportunities in the labor force. But as indicated in the wage equation estimates discussed below, after controlling for other variables, there is little difference between the wage rates of whites and nonwhites in the first three years; in the last two, nonwhites are estimated to earn about 4% more than whites.[17] And the weeks-worked equations indicate that after controlling for other variables nonwhites work about the same number of weeks per year as whites. This could also reflect higher returns to education for nonwhites than for whites, as discussed by Freeman (1976a, 1976b), for example.

The other academic and family background variables are all related in the expected way to school attendance, although the relative magnitudes may not be widely known. Parents' income seems to have much less effect

Table 9.7

Difference in academic or social background	1972–73	Associated differences in the probability of school attendance 1973–74	1974–75	1975–76	1976[a]
Test scores one S.D. above the mean, versus one S.D. below	.242	.254	.247	.230	.181
Class rank one S.D. above the mean, versus one S.D. below	.308	.279	.288	.292	.165
Parents income one S.D. above the mean, versus one S.D. below	.112	.104	.096	.104	.059
Education of Father and Mother college or more, versus less than h.s graduate	.341	.247	.319	.323	.230
Nonwhite versus white	.103	.097	.091	.109	.136

[a]Based on the school nonattendance question estimated with the 1976 wage equation. See table 9.12.

on school attendance than either of the measures of academic achievement. Parents' income may be the least important of all the variables listed. Class rank seems somewhat more important than the test scores, although our comparison is only suggestive. Recall that the tests measure a range of abilities and achievements, some more academically oriented than others. Also, we have made no attempt to distinguish types of schools. The relative importance of academic ability is likely to increase with the quality of school.[18] Finally, there are large differences in expected probability of school attendance associated with extremes in parents' education.

In an alternative specification of the school attendance equations in table 9.9 we also included the number of hours worked per week in high school—measured as one of seven intervals, 1 to 5, 6 to 10, [. . .] over 30—and a variable indicating whether or not the individual had job training during high school. It is questionable whether the job training variable (and possibly hours worked in high school) should be included in a school attendance equation. The question arises because job training in high school may indicate a "noncollege trait" and thus a prior decision not to go to school—it may be more an indicator of post-secondary school attendance rather than a determinant of it. But because the relationship between these choices while in high school and later school attendance may be of interest we have reported the results when they are included. Their inclusion has a negligible effect on the other parameter estimates.[19]

Persons who work more than about 20 hours per week in high school are considerably less likely to be in school in any of the four years than those who work less, according to the estimates of the coefficients on hours worked in high school. The average effect on the probability of school attendance of working 21 to 25, 26 to 30, or more than 30 hours per week is about .10, with the probability evaluated at the means of the other variables. Persons who work less than 20 hours per week are also less likely to be in school during the first year after high school than those who don't work at all, but the relevant coefficients are not measured very precisely.[20] In the remaining three years, the estimates indicate little relationship between post-secondary school attendance and hours worked in high school until hours worked exceeds 20 hours per week approximately.

Recall that these are estimates after controlling for high school achievement and family background. We will show below that, with a few notable exceptions, the number of hours worked during high school is not strongly related to most measures of socioeconomic background nor to school achievement. It is largely an independent personal characteristic. Recall that only for persons who work many hours per week during high school is such work significantly related to later school attendance. There is a tendency for persons who work a lot to be less inclined to continue their formal schooling. Possibly some have made a prior decision to work rather than go to school. We will see, however, that hours worked in high school are strongly related to weeks worked per year after graduation. As expected, persons who get job training in high school are considerably less likely to go to school later than those who don't.

Only two of the other variables in table 9.12 need be mentioned; the others may be thought of simply as controls. One might suppose that school attendance would depend on expected wage, if the individual is not in school, and the ease of finding work; thus the state wage and unemployment variables have been included in the probability equations. They could be considered as rough instruments for individual wage and unemployment rates. Neither is significantly different from zero in most years, although the wage rate in each year is negatively related to school attendance and the unemployment rate positively related. The wage rate is significantly different from zero by standard criteria during the first two years. It could be that labor force opportunities are important determinants of school attendance right after high school, but that in the later years, once in school, persons don't drop out because of changes in the wage rate, and they are less likely to enter school having not attended previously. The marginal lifetime return to an additional year of school probably increases as one nears college graduation.

Finally, persons who go to rural high schools are less likely to be in school after graduation—the difference in probability is about .06 in the first two years and .12 and .13 respectively in the third and fourth years.

Table 9.8 Estimates of Weeks Worked Equation Parameters by Year[a]

Variable	October 1972 to October 1973	October 1973 to October 1974	October 1974 to October 1975	October 1975 to October 1976
Hours worked during high school:				
1 to 5	0.1700	1.1246	0.9006	3.4239
	(1.1629)	(1.6710)	(2.0376)	(1.9529)
6 to 10	2.5027	2.2663	0.4056	3.0490
	(1.1893)	(1.7883)	(1.9750)	(1.7620)
11 to 15	7.3619	1.7668	4.0527	4.4541
	(1.3896)	(1.9820)	(2.2907)	(1.9907)
16 to 20	6.8180	4.2688	5.9215	6.5548
	(1.1109)	(1.5694)	(1.9135)	(1.7884)
21 to 25	7.8500	5.1503	4.2531	7.3057
	(1.2329)	(1.7854)	(1.9096)	(1.7893)
26 to 30	10.9685	6.1313	5.9604	7.1673
	(1.3189)	(1.7165)	(1.9496)	(1.8737)
31 or more	12.5225	7.6769	8.9859	8.1603
	(1.1273)	(1.5174)	(1.8231)	(1.6714)
Class rank in high school	0.2323	0.2120	0.1914	0.2044
	(0.0267)	(0.0276)	(0.0294)	(0.0270)
Test score total	12.1144	10.8146	9.7233	6.7031
	(1.4197)	(1.6811)	(1.7978)	(1.6649)
Job training during high school	−1.4376	1.4486	0.5983	3.0389
	(0.7151)	(1.0539)	(1.2593)	(1.2358)
Race	−1.9184	0.1898	0.3935	−0.9848
	(1.3714)	(1.6311)	(1.7363)	(1.5666)
Parents' income	0.6370	0.4868	0.5015	0.3137
	(0.1168)	(0.1211)	(0.1401)	(0.1218)
Dependents	4.2551	1.6987	1.3027	1.8420
	(0.5997)	(0.7739)	(0.7485)	(0.6702)
On the job training years		0.6600	0.3335	0.3680
		(0.2667)	(0.1885)	(0.1653)
Rural	−2.5912	0.1617	−2.7939	0.0897
	(0.9628)	(1.3651)	(1.4721)	(1.3027)
Urban	−1.4913	−2.2115	−1.2479	0.3683
	(0.8016)	(1.0583)	(1.2458)	(1.1566)
State wage	−2.1755	−1.0422	−1.9233	−1.6065
	(0.9391)	(1.1206)	(1.0218)	(0.8324)
State unemployment	−1.0645	−0.4019	−0.8878	−0.7279
	(0.4069)	(1.1206)	(0.4418)	(0.2826)
Test score missing	33.0777	29.6297	26.4883	19.3229
	(4.3147)	(5.1682)	(5.5833)	(5.1371)
Class rank missing	10.1779	6.8823	6.6354	7.1726
	(1.9191)	(2.2285)	(2.5529)	(2.2480)
Patents' income missing	6.6426	6.8539	3.2212	2.2059
	(1.7567)	(1.9804)	(2.2199)	(2.2028)
Constant	27.2947	23.4604	36.8182	40.9972
	(5.3019)	(6.3969)	(7.0262)	(6.5931)

[a]Standard errors in parentheses.

Table 9.9 **Estimates of School Nonattendance Equation Parameters by Year (Estimated with Weeks-worked Equation)[a,b]**

	October 1972 to October 1973	October 1973 to October 1974	October 1974 to October 1975	October 1975 to October 1976
Test scores total	−0.7411	−0.7704	−0.7180	−0.6714
	(0.0504)	(0.0684)	(0.0694)	(0.0674)
Class rank in high school	−0.0154	−0.0133	−0.0132	−0.0136
	(0.0008)	(0.0010)	(0.0010)	(0.0010)
Race	−0.2792	−0.2509	−0.2297	−0.2726
	(0.0519)	(0.0687)	(0.0696)	(0.0678)
Parents' income	−0.0284	−0.0254	−0.0228	−0.0247
	(0.0039)	(0.0051)	(0.0051)	(0.0049)
Education of mother	0.1358	0.0509	−0.0067	0.0372
less than high school	(0.0275)	(0.0488)	(0.0511)	(0.0511)
Education of mother	−0.2534	−0.1325	−0.1540	−0.1759
college degree or more	(0.0449)	(0.0628)	(0.0703)	(0.0687)
Education of father	0.2503	0.2808	0.2560	0.2295
Less than high school	(0.0277)	(0.0466)	(0.0494)	(0.0504)
Education of father	−0.2890	−0.1772	−0.3991	−0.3675
college degree or more	(0.0377)	(0.0554)	(0.0631)	(0.0607)
Rural	0.1708	0.1542	0.3022	0.3186
	(0.0341)	(0.0529)	(0.0551)	(0.0554)
Urban	−0.0456	−0.0174	−0.0071	−0.0387
	(0.0278)	(0.0439)	(0.0475)	(0.0470)
State wage	0.1444	0.0658	0.0063	−0.0030
	(0.0333)	(0.0437)	(0.0397)	(0.0339)
State unemployment	−0.0191	−0.0156	−0.0195	0.0008
	(0.0141)	(0.0179)	(0.0167)	(0.0112)
Test score missing	−2.1016	−2.1982	−2.115	−2.0005
	(0.1597)	(0.2177)	(0.2237)	(0.2169)
Class rank missing	−0.5491	−0.4709	−0.5977	−0.6305
	(0.0704)	(0.0941)	(0.0980)	(0.0940)
Parents' income missing	−0.2979	−0.3408	−0.2796	−0.3026
	(0.0643)	(0.0845)	(0.0868)	(0.0836)
Constant	2.5045	2.7335	3.0666	3.0571
	(0.1903)	(0.2584)	(0.2675)	(0.2619)
Correlation with	−0.9276	−0.9321	−0.8680	−0.8190
weeks-worked equation	(0.0381)	(0.0248)	(0.0461)	(0.0543)
Standard error weeks-	24.5215	23.3751	25.1452	24.0463
worked equation	(0.7192)	(0.6595)	(0.8119)	(0.7394)
Likelihood value	−6243.7689	−6435.9659	−6598.4836	−7607.7185
Sample size total	4100	3885	3864	4100
Number with weeks worked	1406	1545	1811	2150

[a]These equations pertain to the probability of *not* being in school in *both* the October beginning the year and the following October.

[b]Standard errors in parentheses.

9.2.4 Weeks Worked

Estimates of the parameters in the weeks-worked equations are shown in table 9.8. The most significant finding is that hours worked while in high school bear a substantial relationship to weeks worked per year in the years immediately following high school graduation. The estimated coefficients corresponding to hours-worked intervals in high school are reproduced in table 9.10 below. As can be seen in table 9.8, they are measured with considerable precision. Recall that they represent an upper bound on the effect of high school work. They are slightly larger than the estimated effect of high school hours worked on expected weeks worked, evaluated at $X\beta$ close to zero. As the expected value of Y rises, and thus the expected value of weeks worked, the marginal effect of hours worked in high school falls. Indeed, as the expected number of weeks worked approaches fifty-two, the marginal effect of a change in any variable declines and must ultimately approach zero.

To give an idea of the magnitude of the decline, we have evaluated the estimated effects of high school work at two additional points. One is the expected value of weeks worked evaluated at the mean of X for all persons in the sample, whether they were in fact in the labor force or in school. These values are shown in the second portion of table 9.10. In addition, the expected value of weeks worked is shown for each year, along with an "adjustment factor." The adjustment factor indicates the multiple by which the estimates in the first portion of the table must be multiplied to get the estimates in the second portion.[21] The other evaluation point is the mean of X for persons who were in the labor force and is conditional on knowing that they were. The estimated effect on weeks worked over the four year period is shown in the last column.

Estimates of the β coefficients on hours worked range from zero for hours between 1 and 5 to over 12 for hours greater than 30. Even in the fourth year, the estimated values of β are very large, ranging from 3 for the fewest hours category to over 8 for the largest. It is notable that after four years the relationship of even a little work in high school to work after high school is substantial. Over the four-year period, the sum of the estimates ranges continuously upward from 6 weeks to 37 weeks.

Expected weeks worked per year evaluated at the mean of the right-hand variables averaged over all persons in the sample is about 47 weeks in each of the four years. Even at this level, the estimated relationship to work in high school is very large. The average over the four years of the effect of working between 16 and 20 hours per week is about 1.5 weeks per year. For persons who worked over 30 hours, the average of the estimated effects is almost 3 weeks per year. The sums of the effects for the four years range from 2 to 11 weeks.

Possibly the most intuitively meaningful results pertain to persons who did in fact choose to work rather than go to school. Expected weeks

Table 9.10

Hours worked in high school	Estimated effect on weeks worked 1973	1974	1975	1976	Total
	Effect at zero weeks worked (estimate of β)				
1 to 5	0.17	1.12	0.90	3.42	5.61
6 to 10	2.50	2.27	0.41	3.05	8.23
11 to 15	7.36	1.77	4.05	4.45	17.63
16 to 20	6.82	4.27	5.92	6.55	23.56
21 to 25	7.85	5.15	4.25	7.31	24.56
26 to 30	10.97	6.13	5.96	7.17	30.23
over 30	12.52	7.68	8.99	8.16	37.35
	Effect at mean of X in total sample				
Expected weeks	47.20	47.26	47.36	47.04	—
Adjustment factor	0.270	0.287	0.270	0.297	—
1 to 5	0.06	0.32	0.24	1.02	1.64
6 to 10	0.68	0.65	0.11	0.91	2.35
11 to 15	1.99	0.51	1.09	1.32	4.91
16 to 20	1.84	1.22	1.60	1.95	6.61
21 to 25	2.12	1.48	1.15	2.17	6.92
26 to 30	2.96	1.76	1.61	2.13	8.46
over 30	3.38	2.20	2.43	2.42	10.43
	Effect at mean of X for persons in labor force				
Expected weeks	44.17	43.26	44.42	44.41	—
Adjustment factor	0.484	0.567	0.461	0.458	—
1 to 5	0.08	0.64	0.41	1.57	2.70
6 to 10	1.21	1.29	0.19	1.40	4.09
11 to 15	3.56	1.00	1.87	2.04	8.47
16 to 20	3.30	2.42	2.73	3.00	11.45
21 to 25	3.80	2.92	1.96	3.35	12.03
26 to 30	5.31	3.48	2.75	3.28	14.82
over 30	6.06	4.35	4.14	3.74	18.29

worked evaluated at the mean of X over persons observed to be in the labor force is about 44 weeks in each of the four years. The sums of the effects over the four years range from almost 3 to close to 18 weeks. Sixteen to twenty hours of work in high school is associated with an average of almost 3 weeks per year in weeks worked during the four years after graduation.

Estimates of the marginal relationship between hours worked in high school and weeks worked, evaluated at any other expected value of weeks worked, can be obtained by multiplying the numbers in the top portion of table 9.10 by the appropriate adjustment factor. For example, the appropriate multiple when the expected value of weeks worked [$E(y)$] is thirty is approximately .86 in each of the four years.[22]

How to interpret this finding is open to question. It is possible that persons who work in high school *gain* skills and other attributes, through their work, that give them an advantage in the labor market after graduation. Demand may be greater for them than for persons who do not work. This is consistent with the finding that wage rates are also higher for persons who work in high school, although the relationship is not nearly as strong as that between weeks worked and work in high school. But we might expect such an advantage to diminish over time as post-high school experience becomes an increasingly larger proportion of total experience. And although the estimated effect declines somewhat, it is still very important four years after graduation. This suggests that working in high school may be an indication of personal characteristics not gained through work, but leading to work in high school as well as greater labor force participation following graduation. That is, it is not that the demand is greater for persons who work in high school, but that these persons have a greater propensity to work. That wage rates are not so greatly affected by high school work seems to add to the evidence for this interpretation.

Even this latter interpretation, however, would not rule out the possibility that work experience while in high school, for persons like those in our sample who did not work, would increase their employment after high school. Working may in fact enhance in these persons attributes that were associated with high school work of persons in our sample. And, as we shall see below, work experience while in high school may increase subsequent wage rates in much the same way that work experience upon graduation increases later wage rates in the labor market.

It is informative to consider these findings and the possible interpretations of them in conjunction with the relationship between work in high school and other school and family characteristics. We shall return to that after some discussion of some of the other results shown in table 9.8.

Class rank in high school is strongly related to weeks worked in *each* of the four post-high school years. The estimates indicate that a 50-point increase in class rank is associated with an increase of about 10 in the expected value of Y, or say 3 in the expected value of weeks worked, over the total sample. This result is based on holding constant the test score. The test score appears to measure a combination of aptitude and achievement. No matter what the interpretation of test score, conditional on holding it constant, class rank is likely to reflect effort directed to doing well in high school. Effort in school, like the characteristic reflected in high school work, is related to later labor force participation at least for the next four years. Both hours worked and class rank may capture what is sometimes referred to as the "work ethic." Those who work harder in high school also work more in subsequent years. Or, those who become accustomed to working at a young age maintain the habit. Or, if they have or develop early in life characteristics associated with working, they maintain them.

We also find that high test scores are associated with more employment after graduation, but the effect diminishes over the four years following graduation. An increase of one standard deviation—about .4—in the sum of the test scores is associated with a one-and-a-half-week increase in expected weeks worked in the first year and declines continuously to about one week in the fourth. It may be that persons with greater ability or achievement, as reflected in the test scores, have an advantage in the labor market, but as time goes on the skills that are associated with the test scores are in part compensated for by skills developed on the job or elsewhere. That their effect diminishes over time suggests that the reason is not entirely a permanent underlying individual characteristic. (The wage equation estimates suggest some advantage to higher test scores, but there is no distinct time pattern.)

We could find no measure of high school vocational or industrial training that was significantly related to employment or wage rates after graduation. The variable included in the results in table 9.8 is high school training for a particular job. We assumed that if any high school training mattered, this training should. It doesn't. We experimented with many other measures of job related training—semesters of various vocational courses, academic versus nonacademic tracking, and others. We found none that was related to subsequent employment. It could be that the least able are directed to vocational training courses or are self-selected into them. But our results are conditional on controlling for traditional measures of school performance: class rank and test scores. This cannot be interpreted to mean that no training matters; but it does indicate strongly that none of the training in current high school curriculums, or at least that training systematically measured in the survey, is related to later labor force participation after high school. We were not able to distinguish vocational high schools from others. In subsequent work we will. It is possible that the effect of training in a vocational high school is different from the effect of training received in schools whose curriculums are not primarily directed to job training.

Nonwhites are employed about the same number of weeks per year as whites during the first years following high school graduation. The differences between the expected value of weeks worked for nonwhites and whites, evaluated at the mean of X in the sample, are $-.52$, .05, .11, and $-.29$ respectively in the first four years following high school graduation. None is significantly different from zero by standard criteria. Remember that these results are partial effects after controlling for other variables, unlike the summary statistics in the first section of the chapter. (The simple averages in table 9.6 indicate fewer weeks worked by nonwhites than whites during the first two years after high school, but little difference in the third and almost none in the last.) The wage estimates below indicate that after controlling for other variables, wage rates of whites and nonwhites are quite close in the first three October periods, and that in the last two that nonwhites earn about 4% more per hour than whites.

Variable	Coefficient	Standard error
Test Score Total	−2.81	(0.92)
Race	−5.33	(0.87)
Parents' Income	0.22	(0.07)
Education of Mother Less than High School	0.49	(0.69)
Education of Mother College Degree or More	−0.85	(1.51)
Education of Father Less than High School	0.36	(0.68)
Education of Father College Degree or More	−1.58	(1.28)
Class Rank in High School	0.01	(0.01)
Job Training during High School	0.37	(0.66)
Extracurricular Participation in High School	0.51	(0.29)

The averages in table 9.5 indicate whites and nonwhites who are not in school have very similar weekly earnings and hours worked as well as similar wages.

Parents' income bears a substantial positive relationship to weeks worked during each of the first four years after graduation. In the first year, an increase in parental income of $5,000—about a standard deviation—is associated with an increase in weeks worked of over three weeks. The relevant coefficient declines over time to about half of its original size by the fourth year. If children whose parents have higher-paying jobs have an advantage in finding work, the advantage apparently diminishes as the youth cohort gains labor market experience.

In sum, the most important determinants of weeks worked seemed to be characteristics associated with effort pursuant to succeeding in high school, as measured by class rank after controlling for ability, and to effort devoted to outside work while in high school, this last characteristic being particularly significant. It may be informative therefore to consider the relationship between hours of work while in high school and other personal and family characteristics.

For descriptive purposes, we have obtained coefficient estimates from a least-squares regression of hours worked per week on several variables. The coefficients and standard errors pertaining to the variables of most interest are listed above.[23] There are two groups of variables: one that can be interpreted as composed of predetermined personal and family characteristics, the second composed of measures of the individuals' high school experience other than hours worked while in high school. There does not seem to be a substantial trade-off between any of these latter measures and hours worked. For example, working does not seem to take the place of studying, as reflected in class rank after controlling for test scores. Comparable results were found by Griliches (1977).

Race is the only variable that stands out. Nonwhites work considerably less in high school than whites, given the measures of parents' income and education. This may result either from differences between the two groups in job opportunities, or from differences in work habits, or some combination of the two. Whatever the reason, five hours less work in high school is associated with a maximum of about 1.5 fewer weeks worked in the years following graduation, according to the weeks-worked results. Recall that after controlling for hours worked in high school as well as other variables, nonwhites work about the same number of weeks per year as whites.

In addition, persons with higher test scores work a bit less and those with higher parents' income a bit more. (The standard deviation of test scores is .4 and of parents' income is 5.7 in thousands.) The latter may result from more job possibilities if one's parents have better jobs, or it may reflect cultural differences related to income. Possibly persons with higher test scores, given class rank, foresee a greater probability of going to college and thus are somewhat less inclined to take jobs in high school. This is consistent with the school nonattendance results.

Thus we have in hours worked in high school a personal characteristic that is somewhat related to race, to test scores, and to parents' income.[24] But after controlling for these variables, hours worked in high school are strongly related to weeks worked after graduation. Hours worked capture an individual attribute that is not simply a reflection of other personal and family socioeconomic characteristics; they reflect a largely independent personal attribute that persists over time.

We shall mention briefly the effects of the remaining variables in table 9.9. The estimated effect of on-the-job training is always positive, but it declines over time. This result may be due to training agreements or employment expectations that lead to training for persons who expect to continue in the same job, or whom employers expect to continue. The effect might be expected to die out over time as persons are increasingly likely to have changed jobs.

Persons living in urban areas are employed less than others. According to our imprecise estimates, the maximum negative effect is 2.2 weeks; in the last year when a larger proportion of those working are college graduates the estimated urban effect is in fact positive, although not significantly different from zero. College graduates may have relatively greater work opportunities in urban areas. As expected, state unemployment is negatively related to the employment of youths. Roughly speaking, if the unemployment rate increases by a percentage point, expected weeks worked by these youths fall by about half a week, about one percent of the mean value of weeks worked. The higher the state wage as we have measured it, the lower the number of weeks worked by youths.[25]

Finally, for each year our procedure estimates the correlation between the random term in the weeks-worked equation and the random term in the probability of school nonattendance equation. They are reported for each year in the last section of table 9.9. Recall that a zero correlation coefficient indicates no sample selection bias. Our estimates (and standard errors) for the four consecutive years are − .93 (.03), − .93 (.02), − .87 (.05) and − .82 (.05). That is, in each year unmeasured determinants of college *attendance* bear a very strong positive relationship to unmeasured determinants of weeks worked. Holding constant the variables we have measured, persons who choose to go to school would work more if they are in the labor force than those who choose not to go to school after high school. The relationship is very striking. The results seem to indicate that the motivations or drives that characterize persons who continue their education are also attributes that are related to increased employment given school nonattendance. In practice, correction for sample selection by estimating jointly weeks worked and the probability of school nonattendance increases substantially the estimated coefficients on class rank, test scores, and parents' income, but yields coefficients on the other variables that are close to tobit results. For purposes of comparison, weeks-worked parameter estimates by method of estimation are presented in appendix table 9A.1.

We have not in this specification of weeks worked included a schooling variable. One might suppose, however, that if the probability of school attendance in a given year is positively related to the number of weeks a person would work if he were in the labor force, then the number of years of schooling prior to a given year would also be likely to affect weeks worked in that year if a person were in the labor force. When prior schooling is included in the weeks-worked equations, however, its effect is not significantly different from zero, even with prior schooling also included in the sample selection equation. This suggests that the large correlation between unmeasured determinants of school attendance and measured determinants of weeks worked reflects the difference between persons who attend school for several years after high school graduation—possibly long enough to obtain a degree—and those who don't. Apparently, persons who move in and out of school during the first four years after graduation are in this respect much like persons who don't attend school at all.

9.2.5 Wage Rates

The wage rate parameter estimates are reported in table 9.11. Some have been referred to already. Work experience in high school is positively related to post-high school wage rates as well as to weeks worked. In general, during the last four periods, persons who worked in high school earned roughly 5 to 9% more per hour than those who didn't. Thus not only are additional hours of work in high school associated with additional weeks worked after graduation, but with higher earnings per hour as well. But although there is an increasing relationship between the number of hours worked in high school and weeks worked later, given five or ten hours per week, additional hours in high school are not associated with increments in wage rates until high school hours exceed thirty per week.[26] For these reasons, we have used only three high school work intervals instead of the seven used in the weeks-worked equations.

To evaluate the relationship between hours worked in high school and annual earnings, we need to consider the association between high school work and both weeks worked and the wage rate. (We have not considered the possible effect on hours worked per week.) In addition, according to our specification, the marginal effect of any variable on weeks worked depends on the number of weeks worked at which the marginal effect is evaluated (see pages 295 and 305). Consider, for example, persons in the labor force who otherwise—if not for high school work experience—would have worked forty-four weeks per year. This is approximately the average number of weeks worked by persons who were in the labor force (see page 305). According to our estimates, those who worked between sixteen and twenty hours in high school earned about 12% more than those who didn't work at all in high school. Persons who worked over thirty hours earned about 18% more. The effect could be much greater for persons who would otherwise work less. For example, consider persons who would work only thirty weeks per year. Those who worked sixteen to twenty hours in high school would earn about 25% more than those who didn't work at all. Those who worked more than thirty hours would earn about 35% more. These latter figures should be considered only as suggestive because the estimates do not allow interactions among the variables and therefore imply substantial extrapolation based on estimated coefficients. Nonetheless, the relationship between earnings and work in high school is certainly large even for persons who are working most of the time and is probably much larger for persons who, based on other characteristics, would work much less.

As with weeks worked, it seems likely that at least part of the effect results from personal characteristics associated with or developed through high school work as distinct from later work. If higher wage rates were the result simply of the additional experience or associated acquired skills, one would expect both to be dominated eventually by post-high school work experience and the estimated effect to decline over time.[27] Note that the estimated coefficients on high school work do not simply reflect the fact that persons who work while in high school also are

Table 9.11 **Estimates of Wage Equation Parameters by Year**[a,b]

Variable	October 1972	October 1973	October 1974	October 1975	October 1976
Hours worked during high school					
1 to 15	0.0446	0.0593	0.0627	0.0446	0.0610
	(0.0297)	(0.0294)	(0.0255)	(0.0274)	(0.0238)
16 to 30	−0.0127	0.0407	0.0209	0.0637	0.0411
	(0.0284)	(0.0252)	(0.0244)	(0.0250)	(0.0209)
31 or more	0.0202	0.0971	0.0541	0.0876	0.0904
	(0.0342)	(0.0293)	(0.0284)	(0.0287)	(0.0251)
Class rank in	0.0011	0.0013	0.0009	0.0007	0.0008
high school	(0.0009)	(0.0006)	(0.0006)	(0.0006)	(0.0004)
Test score total	0.0294	0.1002	0.0363	0.0529	0.0996
	(0.0358)	(0.0351)	(0.0300)	(0.0325)	(0.0271)
Job training during	−0.0272	−0.0481	−0.0152	−0.0221	0.0196
high school	(0.0297)	(0.0266)	(0.0257)	(0.0236)	(0.0215)
Race	0.0164	0.0160	0.0078	0.0479	0.0431
	(0.0322)	(0.0287)	(0.0256)	(0.0297)	(0.0272)
Parents' income	0.0095	0.0077	0.0113	0.0101	0.0083
	(0.0026)	(0.0022)	(0.0021)	(0.0021)	(0.0019)
Dependents	0.0221	0.0306	0.0336	0.0318	0.0326
	(0.0143)	(0.0131)	(0.0109)	(0.0120)	(0.0091)
On-the-job	—	0.0012	0.0021	0.0041	0.0060
training months		(0.0041)	(0.0023)	(0.0026)	(0.0022)
School years	—	0.0735	0.0547	0.0433	0.0166
		(0.0429)	(0.0247)	(0.0202)	(0.0082)
Experience:					
First year	—	0.1266	0.0997	0.0540	0.0385
(1972–73)		(0.0471)	(0.0323)	(0.0307)	(0.0219)
Second year	—	—	0.0741	0.1055	0.0275
(1973–74)			(0.0345)	(0.0330)	(0.0231)
Third year	—	—	—	0.1168	0.0602
(1974–75)				(0.0289)	(0.0239)
Fourth year	—	—	—	—	0.0513
(1975–76)					(0.0247)
Part-time working	−0.0141	0.0762	−0.0961	0.0045	−0.1456
	(0.0242)	(0.0280)	(0.0344)	(0.0317)	(0.0305)
Rural	0.0101	−0.0287	−0.0542	−0.0029	−0.0514
	(0.0238)	(0.0208)	(0.0210)	(0.0220)	(0.0193)
West	−0.0153	−0.0301	−0.0014	0.0950	0.0814
	(0.0242)	(0.0237)	(0.0218)	(0.0234)	(0.0213)
State wage	0.0582	0.1218	0.0885	0.0855	0.0775
	(0.0209)	(0.0174)	(0.0147)	(0.0145)	(0.0111)
Test score missing	0.0713	0.2851	0.1144	0.2148	0.3366
	(0.1072)	(0.1032)	(0.0887)	(0.1003)	(0.0856)
Class rank missing	0.0106	0.0215	0.0181	0.0053	0.0361
	(0.0438)	(0.0378)	(0.0383)	(0.0388)	(0.0326)

Table 9.11 (continued)

Variable	October 1972	October 1973	October 1974	October 1975	October 1976
Parents' income missing	0.0896	0.0720	0.0891	0.1192	0.0786
	(0.0399)	(0.0364)	(0.0331)	(0.0368)	(0.0305)
Experience missing	—	0.0549	0.0408	0.0786	0.0275
		(0.0484)	(0.0335)	(0.0277)	(0.0204)
Constant	0.5374	0.1421	0.5558	0.4187	0.4897
	(0.1108)	(0.1165)	(0.1115)	(0.1288)	(0.0981)

[a]The data pertain to the first full week in October of each year.
[b]Standard errors in parentheses.

employed more upon graduation and thus have higher wage rates because of more accumulated post-high school experience. The measured effect of high school experience is in addition to work experience after high school, also included in the equations.

Test scores and class rank are also positively related to wage rates. The effect of class rank seems to diminish somewhat with time, but the test score coefficients follow no apparent pattern. A standard deviation increase in the test scores total is associated with an average of estimated wage rate increases over the five periods of about 3%. The corresponding class rank effect is about 2 or 3%. The total effect of a standard deviation increase in both would be something like 5 or 6%. Together these measures may be assumed to represent some combination of academic aptitude, academic achievement, and academic success. Controlling for test score, class rank may also reflect effort in school comparable to hours worked as a measure of effort outside of school, as discussed in the section above on weeks worked. Any one of these attributes would presumably increase productivity per unit of time.

While traditional measures of academic success are positively related to wage rates, as are attributes associated with actual work experience in high school, high school training, which is presumably closely directed to the development of job skills, is not. The estimated coefficients on job training during high school are not significantly different from zero. This suggests that time taken from academic courses and devoted instead to job training has a negligible effect on future wage rates. If high school training contributes to the development of job-related skills, they are at least offset by the loss in traditional academic training related to job performance. It is also possible that persons who are relatively poor academic performers and would be relatively poor job performers are self-selected into job-training courses in high school. But, as mentioned above, our estimates are conditional on class rank and test scores, possibly the most common measures of high school performance.

One might suppose that the effect of high school training would be greater for persons who left school after high school graduation than for those who obtained further education. Vocational training, for example, may be more important in jobs filled by high school graduates than in those typically filled by college graduates. Our wage data for 1972 include only high school graduates; for that year the coefficient on high school training is negative but not statistically different from zero. In subsequent years, the sample with observed wage rates includes high school graduates as well as those with more education. Thus for 1974 and 1976 we reestimated the equations for high school graduates only; the coefficients on high school training were positive for each of these years but not statistically different from zero by standard criteria.[28]

While nonwhites worked about the same number of weeks as whites after controlling for other variables (table 9.8), the wage rates of nonwhites are a bit higher than those of whites, according to our estimates. The coefficient on race is positive in each of the five periods and significantly different from zero by standard criteria in the last two periods. In the fourth and fifth time periods, nonwhites are estimated to earn about 4% more per hour than whites. (The summary statistics in tables 9.5 and 9.6 show that the average wage rates of nonwhites were slightly lower than the white averages in all but the first period and that nonwhites worked somewhat fewer weeks per year than whites in each year, not controlling for other variables.)

Parents' income has a substantial effect on wage rates. An increase of $10,000 in parents' income is associated with an increase in wage rates of 8 to 12%. It may be that children of wealthier parents have different skills, values, or ambitions from those from poorer families. And, presumably, wealthier parents are able to find, or help to find, better paying jobs for their children. The preponderance of young persons say that their jobs were found through family contacts or through friends. The effect of this benefit as reflected in wage rates seems not to decline much over our five periods; the advantage is maintained for at least these first four years.[29] Recall that the positive relationship between parents' income and weeks worked declined over time.

Persons with dependents not only are employed more, but earn more per hour as well—approximately 3% per dependent in each period. This may result from greater pressure to find higher paying jobs, as well as to work more. Persons without dependents may be more willing to accept lower wages, at least temporarily, possibly while looking for another job.

On-the-job training does not yield appreciably higher wage rates during the first year or two after high school. But after that, when training has presumably paid off in better jobs, the effect shows up. By the fifth time period, the return to a year of on-the-job training is estimated to be

7.2%.[30] In the second, third, and fourth periods the estimated returns are 1.4, 2.5, and 6.1% respectively.

While the estimated effect of on-the-job training increases over time, our estimates suggest a decline over time in the return to years of post-secondary schooling. The estimates shown were obtained using nominal years of schooling.[31] As explained in section 9.2, these estimates should be expected to be biased. Indeed, the positive relationship between the unobserved determinants of wage rates and school attendance, together with the positive correlation among the wage disturbance (discussed in the next section), imply that the estimates are biased upward.[32] In the second, third, fourth, and fifth time periods—one, two, three, and four years after high school graduation—the schooling coefficients imply returns of 7.4, 5.5, 4.3 and 1.7% respectively. The results for the last period may be somewhat confounded because college graduates just entering the labor force are included in the sample. College graduates are likely to be in jobs with wage structures substantially different from persons without college degrees. There may not have been enough time for a college degree to pay off in terms of progression up the hierarchy associated with higher-level jobs.[33] In addition, the result may reflect declines in the return to college education.[34]

The estimated returns to experience are substantially greater than to schooling during these first years following high school graduation.[35] Unlike the effect of hours worked in high school, the effect of early experience on later wage rates declines according to this specification. For example, a year of experience during the first year after graduation is associated with a 13% increase in wage rates in the second time period (the second October after graduation). The effect declines to 10% by the third, 6% by the fourth, and 4% by the fifth period. In general, the effect on wage rates of recent experience is greater than the effect of earlier experience. Relative to the second, third, and fourth time periods—for which experience is relevant—the estimated effect of previous experience in the last time period is quite low, 4% for experience during the first two years and 5 or 6% for experience in the third and fourth years. Lest this pattern of results be taken too literally, we hasten to add two qualifications. The first is that the relative effect of experience across the time periods is dependent in part on changes in aggregate market conditions over the time period. Experience during the recession years is likely to have contributed less to earnings than experience in more expansive years. These results are of course determined in part by changes in aggregate market conditions over the 1972 to 1976 time period.[36]

Second, the specification as shown distinguishes experience by calendar year, but not by the number of years since leaving school. Thus, for example, experience in the "second year" may represent experience during the second year in the labor force for some persons, but during the first year in the labor force for others—those who went to school for one year after high school and then entered the labor force. We tried two other formulations to check the sensitivity of the results to changes in specification. For 1974 we distinguished a separate experience variable for each possible schooling/labor force sequence. Thus for persons who did not go to school after high school we allowed one experience variable for the first year in the labor force and another for experience in the second; these estimates (and standard errors) were .062 (.051) and .071 (.059) respectively. For persons who went to school the first year and entered the labor force the second, the coefficient on this first year of experience was .034 (.037). For those who were in the labor force the first year but went to school the second, the coefficient on the first year of experience was .042 (.059).

As mentioned above, for 1974 and 1976 we also estimated wage equations for persons with no post-secondary education. (Of course, persons in the sample in 1972 had no education past high school.) For this group, the estimated experience coefficients for 1974 were .116 (.057) and .134 (.074) respectively, as compared with .100 (.032) and .074 (.035) in table 9.11. For 1976, the coefficients were measured very imprecisely but tended to be somewhat larger than those shown in the table. Thus it seems clear that early experience affects later wage rates. The precise patterns of the effects shift with the sample and the specification although the differences are not statistically significant. Finally, we noted above that vocational training in high school was not significantly different from zero, even for persons with no post-secondary education. These estimates for 1974 and 1976 do reveal, however, that work experience in high school has a somewhat greater effect on wage rates for persons who got no further education than for the group as a whole.

The effect of experience as well as other variables is of course reflected in the small difference between the average wage rates of whites and nonwhites shown in table 9.5. For example, the average wage rate for whites is about 6% higher than for nonwhites in 1976. Our estimated coefficient on race for 1976 implies that nonwhites earn about 4% more than whites after controlling for other variables. But nonwhites work fewer weeks than whites in each year, as shown in table 9.6. Using these differences, the effect of fewer weeks worked per year on nonwhite wages would be about 1.3% in 1976, according to the estimated coefficients on experience in that year.

We also find that while part-time workers do not receive lower wages than full-time workers immediately after graduation, they do a few years later. By 1976, part-time workers were earning 15% less per hour than those working full time. It is likely that part-time jobs are less likely to be characterized by ladder movement and associated wage increases than full-time ones. This may not affect initial wage rates much, but would after some time when many full-time workers would have moved up the ladder.

We experimented with several regional and residential location variables. Only a rural indicator and an indicator for the western region are included in the specification shown. After controlling for an average state wage measure, none of the other controls for aggregate market conditions affected youth wages.

We will not comment on the "school nonattendance" estimates in table 9.12. They are essentially comparable to those in table 9.9 that were discussed above.

At the bottom of table 9.12, however, are shown the estimates of the correlations between the wage rate and school nonattendance disturbances. As in the weeks-worked results, we find a positive correlation between unmeasured determinants of school attendance and the disturbances in the wage rate equations, although the relevant correlations are much smaller. Thus, according to our results, persons who go to school, if they are working, earn more than those who in fact elect to work, even if the two groups of individuals have the same measured characteristics. The estimated correlation is .21 in 1972 and then rises to .36 in 1973. After that, they decline rather evenly to .19 in 1976. It is reasonable to expect them to decline as more and more persons enter the labor force after having been in school for one or more years.

9.3 The Persistence of Early Labor Force Experience

Early labor force experience may be related to later experience for at least four reasons: (1) Measured attributes of individuals are similar from period to period. For example, we have found that persons from wealthy families earn more per hour than those from poor families. And that persons with higher academic aptitude or measured achievement command higher wage rates than those with lower scores. (2) Some unmeasured attributes of individuals persist over time and are related to labor force experience. This reason is often referred to as heterogeneity. How much youths are helped by their families, for example, or difficult to define characteristics like motivation, may fall into this category. (3) Random factors that affect labor force experience, although not constant over time, may be related from one time period to the next. The fortunes or misfortunes of a large firm in a small town may be an example. (4) Finally, labor force experience due to random occurrences or shifts in exogenous variables in one period may affect outcomes in later periods. This possibility is often referred to as state dependence.[37]

The first of these reasons we have analyzed in section 9.2. The last three are the subject of this section, although we will not be able to distinguish each of them from all the others. Our analysis will concentrate on inferences that can be drawn from relationships among the distur-

Table 9.12 Estimates of School Nonattendance Equation Parameters by Year (Estimated with Wage Equations)[a]

Variable	October 1972	October 1973	October 1974	October 1975	October 1976
Test scores total	−0.4809	−0.5913	−0.6648	−0.5853	−0.5618
	(0.0697)	(0.0757)	(0.0774)	(0.0790)	(0.0818)
Class rank in	−0.0160	−0.0145	−0.0136	−0.0134	−0.0083
high school	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Race	−0.3242	−0.2732	−0.2534	−0.3473	−0.3863
	(0.0705)	(0.0765)	(0.0797)	(0.0814)	(0.0831)
Parents' income	−0.0209	−0.0201	−0.0151	−0.0201	−0.0153
	(0.0054)	(0.0057)	(0.0056)	(0.0057)	(0.0058)
Education of mother	0.1406	0.2221	−0.0123	−0.0087	−0.0019
less than high school	(0.0563)	(0.0619)	(0.0641)	(0.0653)	(0.0690)
Education of mother	−0.2725	−0.0607	−0.1357	−0.2226	−0.1247
college degree or more	(0.0911)	(0.0812)	(0.0840)	(0.0835)	(0.0808)
Education of father	0.2712	0.2179	0.2848	0.3237	0.1985
less than high school	(0.0549)	(0.0588)	(0.0621)	(0.0640)	(0.0670)
Education of father	−0.2385	−0.2521	−0.3495	−0.3798	−0.3068
college degree or more	(0.0778)	(0.0735)	(0.0742)	(0.0742)	(0.0702)
Hours worked during	0.1184	0.0998	0.1147	0.0786	0.0710
high school	(0.0199)	(0.0209)	(0.0214)	(0.0220)	(0.0219)
Job training during	0.4921	0.5072	0.4238	0.3498	0.1924
high school	(0.0576)	(0.0655)	(0.0708)	(0.0721)	(0.0748)

Rural	0.1195	0.1157	0.3155	0.2781	0.3078
	(0.0597)	(0.0632)	(0.0646)	(0.0676)	(0.0692)
Urban	−0.0605	−0.0747	0.0450	0.0520	−0.0817
	(0.0551)	(0.0570)	(0.0576)	(0.0580)	(0.0582)
State wage	0.1520	0.0815	0.0288	−0.0311	0.0029
	(0.0443)	(0.0447)	(0.0424)	(0.0388)	(0.0352)
Test score missing	−1.3909	−1.7156	−1.9021	−1.7546	−1.7570
	(0.2196)	(0.2393)	(0.2499)	(0.2542)	(0.2666)
Class rank missing	−0.6499	−0.4990	−0.5336	−0.5836	−0.3744
	(0.0960)	(0.1041)	(0.1043)	(0.1094)	(0.1133)
Parents' income missing	−0.2448	−0.2928	−0.2539	−0.3713	−0.2019
	(0.0891)	(0.0944)	(0.0972)	(0.1021)	(0.1026)
Constant	1.1362	1.9271	2.4743	2.7925	2.6646
	(0.2678)	(0.2945)	(0.3024)	(0.3086)	(0.3146)
Correlation with wage equation	−0.2115	−0.3610	−0.3557	−0.1932	−0.1937
	(0.1633)	(0.1414)	(0.1233)	(0.1416)	(0.1641)
Variance of wage error	0.3542	0.3496	0.3509	0.3649	0.3482
	(0.0100)	(0.0127)	(0.0102)	(0.0076)	(0.0070)
Likelihood value	−2538.7109	−2321.7729	−2354.3789	−2441.3557	−2444.0023
Sample size total	4000	3400	3300	3200	3100
Number with wage	1402	1489	1659	1728	2070

[a]Standard errors in parentheses.

bance terms in the wage equations and from relationships among nominal weeks worked, as well as disturbances from the weeks-worked equations. Because we have estimated weeks-worked and wage equations separately for each year, and because we have obtained wage equation estimates allowing for sample selection and weeks-worked equations allowing for both sample selection and a limit of fifty-two weeks, it is cumbersome to estimate unconditional correlations among the population disturbances, as specified in equations (1) and (5), for example. It is not straightforward to use the residuals because the independent variable is observed only for persons not in school and in the case of weeks worked because the independent variable is limited. For simplicity we will limit our discussion to the relationships over time, conditional on being in the labor force. This allows a rather straightforward variance components description of the structure of the correlations among the wage disturbances. We will consider them first. In addition to the variance components decomposition, we have used another method to describe the relationships among weeks worked over time. We will consider them second.[38]

For the wage rate disturbances we will be able to distinguish persistence over time due to heterogeneity from that caused by the last two reasons listed above. But we will not formally be able to distinguish the third from the fourth, that is, serial correlation from state dependence as they are interpreted here. What will show up as serial correlation in our analysis could result from what we would like to distinguish as state dependence. But we will be able to say something about the possible magnitude of a state-dependence effect. Because our analysis relies primarily on inferences based on the estimated correlations (or covariances) among the disturbances, we will not give much attention to the subsequent effects of changes in labor force experience due to shifts in exogenous variables in earlier periods (included under our fourth reason).[39]

Although subsequent analysis will use estimated covariance terms, it is informative to look first at estimates of the correlations between the disturbances in the wage equations.[40] They are reproduced below, together with a correlation matrix of the logarithm of nominal wages.[41]

Correlation Matrix of Disturbances
in the Wage Equations

October '72	1				
October '73	.538	1			
October '74	.304	.505	1		
October '75	.287	.373	.569	1	
October '76	.282	.412	.519	.727	1

Correlation Matrix of the Logarithm of Nominal Wages

October '72	1				
October '73	.563	1			
October '74	.342	.544	1		
October '75	.323	.411	.590	1	
October '76	.329	.458	.558	.752	1

The pattern of correlations suggests that unmeasured influences on wage rates are to a large degree temporary ones that do not persist from early to later years. The correlation between the first and the fifth wage disturbances is only .282. The correlations also suggest increasing consistency over time. For example, the correlation between the first and second disturbances is .538, but .727 between the fourth and the fifth. The correlations drop rapidly with increases in the time interval between periods. This can be seen by a glance at the last row of the correlation matrix, where the correlations between the last year's disturbance and those for prior years are recorded. Whatever the cause of the observed persistence, it declines rapidly over time to a floor of about .3. (As we will see in a moment, that can be attributed to persistent individual specific characteristics.) A casual comparison of the correlations suggests that the effect of individual specific characteristics on wage rates is dominated by random components that are serially correlated. We shall be more precise about that.

Suppose that the wage equation disturbances can be decomposed into individual specific and random terms. Let each disturbance be written as

$$\varepsilon_{ti} = u_i + e_{ti}$$

where u is an individual specific term, presumed to persist over the period of our data, and e is a random term. Suppose that the variance of u over individuals is σ_u^2 and the variance of e, allowed to differ from period to period, is given by σ_t^2. Also, assume that the terms e_{ti} follow a first order auto regressive process. Then the variances among the disturbances can be written as:

$$\sigma_{11} = \sigma_u^2 + \sigma_1^2$$
$$\sigma_{12} = \sigma_u^2 + \rho\sigma_1^2$$
$$\sigma_{13} = \sigma_u^2 + \rho^2\sigma_1^2$$
$$\sigma_{14} = \sigma_u^2 + \rho^3\sigma_1^2$$
$$\sigma_{22} = \sigma_u^2 + \sigma_2^2$$
$$\sigma_{23} = \sigma_u^2 + \rho\sigma_2^2$$
$$\sigma_{24} = \sigma_u^2 + \rho^2\sigma_2^2$$

$$\sigma_{33} = \sigma_u^2 + \sigma_3^2$$
$$\sigma_{34} = \sigma_u^2 + \rho\sigma_3^2$$
$$\sigma_{44} = \sigma_u^2 + \sigma_4^2$$

We have estimates of the σ_{ij} based on residuals from the equations estimated above. Using a maximum likelihood procedure, we fitted these estimates to the specification just described. That is, we estimated σ_u^2, ρ, $\sigma_1^2, \ldots, \sigma_4^2$.[42] There are several special cases of this more general model. We shall mention two. One is obtained by supposing that the random components are not serially correlated, so that ρ is zero. (This would of course rule out state dependence.) In this case, all the covariances would be equal. The corresponding correlations would be the same, except to the extent that the variances of the random terms differ. The second constrains σ_u^2 to be zero; it rules out heterogeneity. Then the correlations between disturbances one period apart are given by $\rho\sqrt{\sigma_t^2}/\sqrt{\sigma_{t+1}^2}$, and for two periods apart by $\rho^2\sqrt{\sigma_t^2}/\sqrt{\sigma_{t+2}^2}$, etc. If the random term variances are equal, the correlations become ρ, ρ^2, etc.

Estimates of the components of variance for the wage disturbances, based on the unconstrained model, are recorded below.

Components of Variance Estimates and Standard Errors for the Wage Rate Covariance Structure[43]

Individual specific variance, σ_u^2	0.032	(0.009)
Random variance, period 1, σ_1^2	0.089	(0.013)
Random variance, period 2, σ_2^2	0.086	(0.008)
Random variance, period 3, σ_3^2	0.093	(0.013)
Random variance, period 4, σ_4^2	0.107	(0.012)
Random variance, period 5, σ_5^2	0.090	(0.013)
Serial correlation coefficient, ρ	0.454	(0.082)

We have also estimated a components of variance specification of the wage disturbances with the random component variances constrained to be equal. The results are as follows:

Constrained Components of Variance Estimates and Standard Errors for the Wage Rate Covariance Structure

Individual specific variance	0.034	(0.009)
Random variance	0.091	(0.009)
Serial correlation coefficient, ρ	0.430	(0.090)

They suggest the same general conclusions as those based on the unconstrained model, although we reject the hypothesis of equal variances. It is clear that both individual specific and random terms are important determinants of variance. These estimates suggest that between 23 and 27% of the error variances can be ascribed to individual specific characteristics that persist over the five time periods. The bulk of the variance, however, remains in the additive random terms. Those random terms are correlated over time. The estimated serial correlation coefficient in the unconstrained model is .454. We conclude that whatever the cause of this correlation over time, its effect is not lasting. The estimated effect of serial correlation on the aggregate correlations in the matrix above declines rapidly. Ignoring differences in random term variances, without the individual specific terms the estimated correlations between the disturbances one, two, three, and four periods apart would be .454, .206, .094, and .042 respectively.[44]

Thus we conclude that whatever causal effect there may be of early wage rate experience on later wage rates, it does not last very long; it is essentially absent after four or five years.

The correlations among the weeks-worked disturbances for persons not in school are shown in the first tabulation below. The correlations among nominal weeks worked are shown in the second.[45]

Correlation Matrix of Weeks-Worked Disturbances

October '72 to October '73	1			
October '73 to October '74	.351	1		
October '74 to October '75	.240	.333	1	
October '75 to October '76	.170	.270	.640	1

Correlation Matrix of Weeks Worked

October '72 to October '73	1			
October '73 to October '74	.394	1		
October '74 to October '75	.285	.373	1	
October '75 to October '76	.196	.302	.655	1

For comparison with the results for the wage disturbances, we fit the same variance components specification to the weeks worked residual covariance structure. The results are as follows:

Components of Variance Estimates and Standard Errors for the Weeks Worked Covariance Structure

Individual specific variance, σ_u^2	26.19	(19.14)
Random variance, period 1, σ_1^2	130.52	(26.76)
Random variance, period 2, σ_2^2	125.52	(16.84)
Random variance, period 3, σ_3^2	139.74	(25.60)
Random variance, period 4, σ_4^2	128.04	(26.48)
Serial correlation coefficient, ρ	.343	(.133)

The estimates are quite similar to those pertaining to the wage disturbances, although the proportion of variance due to individual specific terms is smaller—between 16 and 18% depending on the year. As with the wage disturbances, without individual specific terms, the correlations among the errors would be quite small. Ignoring differences in random term variances, the implied correlations one, two, and three periods apart are .343, .118, and .040 respectively. Thus, whatever the reasons for the correlation over time—including a possible state dependence effect—it is not lasting. These results based on weeks worked residuals are similar to those obtained for wage disturbances.

But in this case, the disturbances, like nominal weeks worked, are limited by the upper bound on total weeks worked. In practice, the estimated correlations are not affected much by the truncation of weeks worked. Correlation matrices of nominal weeks worked, and of weeks worked disturbances, based only on observations with weeks worked less than fifty-two, are very close to those presented above. Nonetheless we found it more informative to describe relationships among weeks worked through a series of transition matrices, than to describe the relationship by an estimated components of variance structure. Our procedure was the following.

For each year we classified weeks worked into four intervals: 0 to 20, 21 to 40, 41 to 51, and 52. For each pair of years we calculated the transition probabilities of moving from an interval in the earlier year to each of the intervals in the second year. These are presented in figure 9.2 with the entries shown as percentages. For example, the matrix headed "1974–75" in the middle of the table says that 71% of the persons who worked 52 weeks in 1974 also worked 52 weeks in 1975; 4% worked between 0 and 20 weeks. The numbers below and to the left of each matrix are marginal proportions (percentages). All entries have been rounded to the nearest percentage.

The figure can also be used to calculate for each pair of years the joint probability of each of the interval combinations. For example, the matrix headed "1973–76" in the lower left of the figure says that 1% of the 892 persons who were not in school in both 1973 and 1976 worked less than 20 weeks in each of those years (13% of 8%).

Recall that some persistence over time is due to measured attributes of individuals that are similar from one period to the next. The slightly higher correlations among annual weeks worked than among the weeks-worked residuals reflects the effect of these variables. It can be seen from the matrices above, however, that this difference is small. Only a small proportion of the variance in weeks worked is explained by measured individual attributes. The transition matrices in figure 9.2 present a blowup of the information contained in the nominal weeks-worked correlation matrix. Thus persistence is somewhat higher than that due to unobserved components alone, but not much.

1973–74

9	30	24	23	24
20	11	35	33	21
31	2	12	42	43
40	4	8	26	62
	7	16	32	45

N = 1045

1973–75

8	27	27	16	30
20	9	25	25	41
31	6	12	29	53
41	6	8	19	66
	8	14	23	54

N = 868

1974–75

7	31	27	15	27
18	15	29	21	35
32	6	14	34	46
44	4	8	18	71
	8	15	23	54

N = 1164

Weeks Worked Interval
0 to 20
21 to 40
41 to 51
52

1973–76

8	13	26	20	41
19	13	24	23	40
32	4	15	29	53
42	5	8	22	65
	7	15	24	54

N = 892

1974–76

7	24	24	20	32
17	14	25	22	39
32	5	15	35	46
44	4	8	20	68
	7	14	25	53

N = 1093

1975–76

8	50	33	9	8
15	12	40	22	26
25	3	13	61	23
53	1	5	11	83
	7	14	24	54

N = 1581

Fig. 9.2 Transition Probabilities (Percentage) by Weeks-worked Interval for Each Two-year Combination, 1973–76

The transition matrices reveal several phenomena. The upper bound on weeks worked is reflected in the large probabilities of remaining in the fifty-two week "interval" from one period to the next, much larger than for any other interval. This is apparently because many persons who work fifty-two weeks are indeed constrained by this limit. Any who "would work" fifty-two weeks or more are observed to remain at the limit. Even persons observed to work fifty-two weeks in one year may still be at fifty-two weeks in the second even if their "unobserved propensity" to work declined between the two time periods. From the diagonal matrices it can be seen that those who remain at the limit for consecutive years increases from 62% between the first and the second to 83% between the third and the fourth.

Persistence in general increases over time, as can be seen from a comparison of the diagonal elements of the three diagonal matrices. For example, 30% of persons who are in the lowest interval in the first year are also in that interval in the second. But 50% of those who are in this interval in the third year are also there in the fourth. Apparently individual patterns become increasingly established.[46]

While experience in the fourth year seems strongly related to that in the third, the relationship between experience in the last year and earlier years declines rapidly with increasingly distant time periods. This pattern can be seen best by looking at the last row of matrices for 1976. Of persons in the four intervals in 1975; 50, 12, 3, and 1% respectively are in the lowest interval in 1976. Of persons in the four intervals in 1973, the corresponding percentages are 13, 13, 4, and 5. Whereas the likelihood that a person who was in the lowest interval in 1975 was also there in 1976 was 50 times as high as if he worked 52 weeks in 1975; if he were in the lowest interval in 1973, the likelihood of being in the lowest interval in 1976 was only about 2.5 times as high as if he had worked 52 weeks in 1973. These numbers are consistent with the simple correlations among weeks worked.

The numbers of persons who remain in the lowest intervals also can be inferred directly from figure 9.2. For example, 1% of persons who were not in school in both the first and the last year worked 20 weeks or less in each of the years.

We conclude, as with wage rates, that whatever the determinants of weeks worked, they do not for the most part persist over these four years. Recall that a small part of the relationship seen in the transition matrices is due to measured individual attributes. They are not distinguished in the matrices from unmeasured individual attributes, individual specific terms commonly referred to as representing heterogeneity. Both measured and unmeasured individual specific characteristics produce some persistence over time. (The proportion of the residual variance due to individual specific terms, implied by the "residual" covariance matrix, was presented above.) The remainder of the relationship over time may be due to a true state-dependence effect or to serial correlation induced by correlation over time of other factors that affect weeks worked. Whatever the reason, however, there seems to be very little room for a state dependence effect of labor force experience in the first year on experience in the last. Any effect there may be dies out rapidly.

As youngsters age their patterns of labor force experience become increasingly stable, as we might expect to find among persons moving from full-time school to full-time work, a process that is likely to involve considerable searching, job changing, and the like before settling into more or less permanent employment.[47]

Unmeasured determinants of wage rates in the early periods show little relationship to unmeasured determinants in later years. Unmeasured determinants of weeks worked in the earliest period show little relationship to those in the last. There is, however, a dependence between the two. As shown in table 9.11, experience in earlier years does affect wage rates in later years.[48]

9.4 Summary and Conclusion

We have used the National Longitudinal Study of 1972 High School Seniors to analyze the relationship between high school preparation and other personal characteristics on the one hand and early labor force experience on the other.

In general, the data do not suggest to us severe employment problems for this sample of high school graduates. There are very few persons who are chronically out of school and unemployed. Estimated unemployment rates are moderate and employment ratios high. The implications that we draw from these data are at variance with those based on the Current Population Survey data, which suggest substantially higher unemployment rates for high school graduates and considerably lower employment ratios.

Average wage rates of employed whites and nonwhites who are not in school are very similar. Wage equation estimates reveal that, after controlling for other variables, nonwhites earn slightly more per hour than whites. But average weeks worked per year are less for nonwhites than whites although annual weeks-worked equations that control for other variables indicate that nonwhites are employed about as many weeks per year as whites with similar characteristics. At the same time, nonwhites are more likely than whites to be in school; controlling for other variables, the probability of being in school is at least .10 higher for nonwhites in each of five periods covering four post-high school years.

Although traditional measures of academic success—standardized test scores and class rank—are related to employment and wage rates following high school, measures of vocational and industrial training are not. Training presumably directed toward job-related tasks does not enhance post-high school labor force experience, but attributes associated with traditional measures of academic success do.

Hours of work while in high school are very strongly related to weeks worked in particular and also to wage rates in each of the four years following graduation. An additional five hours of work per week in high school, at least up to 20, is associated with as much as 1.5 more weeks worked per year in each of the four post-high school years. The evidence suggests that this is due to individual attributes associated with working while in high school; these attributes may or may not be developed by this experience. Together with the effect on the hourly wage rate, the effect on earnings is quite substantial. This suggests to us that training only, without the attributes associated with work effort and or doing well in school, will not increase one's chances in the labor force. On the other hand, on-the-job training after high school is associated with higher wage rates. Possibly none of these findings should be especially surprising. They reinforce the frequently mentioned claim that ill-defined attributes associated with working hard and "doing well," perhaps the work ethic, are important determinants of labor force "success." This idea seems to come through strongly in our statistical results.

The results should not be interpreted to mean that vocational training will not help persons do jobs better. It seems to us more likely that the kinds of training in current high school curriculums do not. On-the-job training, for example, does have a significant effect on later wage rates. This is, of course, training combined with work. We were unable to distinguish training in vocational high schools from training in other high schools. Vocational high schools may provide better training and attract different kinds of students. More detailed investigation could reveal particular types of students for whom high school vocational training does enhance subsequent labor force experiences. We will pursue both of these possibilities in future research. It may also be that selection and tracking mechanisms in high school channel those least likely to succeed, either in or out of school, into nonacademic courses. Our results, however, are conditional on test scores and class rank, both common measures of high school performance.

Finally, we addressed the question of persistence over time of early labor force experience. An important question is whether or not early realized experience itself, after controlling for individual characteristics of persons, has an effect on later experience. Is there a "state dependence" effect? Our analysis suggests that if there is such an effect, it does not last long. There seems to be almost no relationship of this kind between weeks worked during the first year after high school graduation and weeks worked four years later. That is, we find no relationship other than that due to individual specific attributes. And random fluctuations in wage rates in the first year or two, resulting from nonindividual specific attributes, have almost no relationship to wages three or four years later. Thus our findings do not motivate or increase concern that there may be something intrinsically damaging about particular kinds of early labor force experience. After controlling for measured characteristics of individuals, we cannot identify a lasting effect of initial realized employment on later employment or of initial realized wage rates on later wage rates. We do find, however, that early weeks worked have an effect on wage rates; but even this effect may be rather small after four years.

Although early random fluctuations in weeks worked have little effect on later weeks worked and early random fluctuations in wages little effect on later wages, our results show a distinct trend of greater consistency between one year and the next as persons age. Employment patterns in the third year, for example, are much more likely to carry over to the fourth than are first-year patterns to carry over to the second. We find a concomitant increasing wage penalty associated with part-time work as persons age.

Along with the lasting relationship between high school work experience and later wage rates, as well as employment, we find that the effect of weeks worked in the first year after graduation has a substantial effect on wage rates in subsequent years, although the effect may decline over time.

There are three distinct findings here and we will put them all together. First, the estimated "effects" of high school work experience on weeks worked and wage rates after high school are about the same over the four post-high school years. Second, the effect of early post-high school weeks worked on wage rates in subsequent years is substantial but may decline over time, with weeks worked in the most recent year being more important than experience in earlier years in the determination of current wages. Third, we find no lasting effect of nonindividual specific random disturbances in early post-high school weeks worked on weeks worked in later years. And there is no lasting effect of nonindividual specific random disturbances in initial wage rates on later wages, although weeks worked in early years have an effect on later wage rates, as the second finding describes.

Thus our findings suggest, albeit indirectly, that to prepare persons for the labor force, programs that emphasize work experience for youths may be the most likely to succeed. Indirectly, they suggest that the concern that low-level or dead-end jobs will hinder subsequent labor market performance is likely to be misplaced. Even though we cannot be sure that the characteristics of those who now work in high school will be gained by those who don't, should future generations of them be got to work, the weight of our evidence is that it offers the best chance of enhancing future labor market experience. Certainly our evidence suggests that it should be given precedence over specific job training in high school. If there is a second priority, our evidence suggests that general academic preparation has a greater payoff than current high school vocational training.

Appendix: Estimation

Consider the weeks-worked and the school nonattendance equations—given in equations (5) and (2) in the text—for any one of the four annual time periods:

$$y_i = \begin{cases} Y_i = X_i\beta + \varepsilon_i & \text{if } X_i\beta + \varepsilon_i \leq 52, \\ 52 & \text{if } X_i\beta + \varepsilon_i > 52, \end{cases}$$

$$S_i = Z_i\delta + \eta_i, \text{ with}$$

$$s_i = \begin{cases} 1 \text{ if } S_i \geq 0, \\ 0 \text{ if } S_i < 0, \text{ and} \end{cases}$$

$$\begin{bmatrix} Y_i \\ S_i \end{bmatrix} \sim N\left[\begin{pmatrix} X_i\beta \\ Z_i\delta \end{pmatrix}, \begin{pmatrix} \sigma^2 & \rho\sigma \\ & 1 \end{pmatrix}\right]$$

where lower case y_i is observed weeks worked and capital Y_i is the unobserved propensity to work and ρ is the correlation between Y_i and S_i.

There are three possibilities: individual i is in school so that $S_i < 0$; he is not in school and is working less than fifty-two weeks so that $S_i > 0$ and y_i is observed with $y_i < 52$; he is not in school and is working fifty-two weeks so that $S_i > 0$ and $y_i = 52$. The probabilities of these outcomes, given X_i and Z_i, are represented respectively by:

(1) $$Pr(S_i < 0) = 1 - \Phi[Z_i\delta] = P_{1i}$$

(2) $$\begin{aligned} &Pr(S_i > 0 \text{ and } y_i \text{ observed, with } y_i < 52) \\ &= Pr(S_i > 0 \mid Y_i) f(Y_i) \\ &= \Phi\left[\frac{Z_i\delta + \frac{\rho}{\sigma}(y_i - X_i\beta)}{\sqrt{1-\rho^2}}\right] \cdot \frac{1}{\sigma}\phi\left(\frac{y_i - X_i\beta}{\sigma}\right), \\ &= P_{2i} \end{aligned}$$

(3) $$\begin{aligned} &Pr(S_i > 0 \text{ and } y_i = 52) \\ &= Pr(S_i > 0 \text{ and } Y_i > 52) \\ &= Pr(\eta_i < Z_i\delta \text{ and } \varepsilon_i < X_i\beta - 52) \\ &= \int_{-\infty}^{Z_i\delta} \int_{-\infty}^{X_i\beta - 52} f(\eta_i, \varepsilon_i) d\varepsilon_i d\eta_i \\ &= \Phi[Z_i\delta, (X_i\beta - 52)/\sigma; \rho] \end{aligned}$$

where f is a bivariate normal density function and Φ must now be interpreted as a standardized bivariate normal distribution function with correlation parameter ρ. The log-likelihood function for the complete sample of observations is given by

$$L = \sum_i^{N_1} \ln P_{1i} + \sum_i^{N_2} \ln P_{2i} + \sum_i^{N_3} \ln P_{3i}$$

where the three summations distinguish the groups corresponding to the three possible outcomes. This likelihood function is maximized to obtain estimates of β, δ, σ, and ρ.

There are three expectations that it is useful to distinguish, together with the derivatives with respect to the variables x. They are given by:

(1) $$E(Y \mid X) = X\beta$$

(2) $$\begin{aligned} E(y \mid X) &= Pr(Y \geq 52) \cdot 52 + Pr(Y < 52) \cdot E(Y \mid Y < 52) \\ &= \left\{1 - \Phi\left[\frac{52 - X\beta}{\sigma}\right]\right\} \cdot 52 \\ &\quad + \Phi\left[\frac{52 - X\beta}{\sigma}\right] \cdot X\beta - \sigma\phi\left(\frac{52 - X\beta}{\sigma}\right) \end{aligned}$$

(3) $$E(y \mid X \text{ and } s = 1) = \frac{Pr(Y \geq 52, s = 1)}{Pr(s = 1)} \cdot 52$$
$$+ \frac{Pr(Y < 52, s = 1)}{Pr(s = 1)} \cdot E(Y \mid Y < 52, s = 1),$$
$$= \frac{\Phi[Zi\delta, (Xi\beta - 52)/\sigma; f]}{\Phi[Zi\delta]} \cdot 52$$
$$+ \frac{\Phi[Zi\delta, (52 - Xi\beta)/\sigma; -f]}{\Phi[Zi\delta]} \cdot E(Y \mid Y < 52, s = 1).$$

The derivatives of the expected values with respect to X_j are given by:

(a) $$\partial E(Y \mid X)/\partial X_j = \beta_j$$

(b) $$\partial E(y \mid X)/\partial X_j = \beta_j \cdot \Phi\left[\frac{52 - X\beta}{\sigma}\right]$$

Recall that our maximum likelihood procedure estimates β_j. The derivative of the expected value of observed weeks worked is given by β_j times the probability that Y is less than 52. At $X\beta = 0$, this derivative is approximately equal to β_j in our sample $\Phi[52/\sigma]$ is close to 1. It is informative to evaluate the derivative b at say the mean of X. In our sample, $E(y \mid \bar{X})$ is about 43 weeks. Thus the derivative of y at this point gives a reasonable indication of the effect of a change in an X value when y is approaching its maximum. Comparable adjustments can be made for persons who elect not to go to school.

The wage specification prescribes only two possible outcomes, analogous to the first two presented above for weeks worked. An individual is either not in the sample with a measured wage ("in school") so that $S_i < 0$ and W_i is observed. These probabilities are given by:

(1) $$Pr(S_i < 0) = 1 - \Phi[Z_i\delta]$$

(2) $$Pr(S_i > 0) \text{ and } W_i \text{ observed}) [= Pr(S_i > 0 \mid W_i) f(W_i)$$
$$= \Phi\left[\frac{Z_i\delta + \frac{\rho}{\sigma}(W_i - X_i\beta)}{\sqrt{1 - \rho^2}}\right] \cdot \frac{1}{\sigma}\phi\left(\frac{W_i - X_i\beta}{\sigma}\right)$$

where σ here is the standard deviation of W_i given X_i and W_i represents the right-hand side variables in the wage equation, not all of which correspond to those in the weeks-worked equation. The likelihood function is formed as above. Maximization of its yields estimates of β, δ, ρ, and σ.

Table 9A.1 **Estimates of Weeks-worked Equation Parameters for 1973 by Method of Estimation**[a,b]

Variable	Tobit *with* sample selection, persons not in school[a]	Tobit *without* sample selection, persons not in school	Least squares, persons not in school	Least squares, persons in school
Hours worked during high school				
1 to 5	0.1700	0.4522	1.0774	4.3217
	(1.1629)	(1.9881)	(1.2973)	(1.0991)
6 to 10	2.5027	2.8743	2.8273	4.9857
	(1.1893)	(2.0018)	(1.2663)	(1.1254)
11 to 15	7.3619	7.6174	5.5637	9.1454
	(1.3896)	(2.2155)	(1.3636)	(1.2168)
16 to 20	6.8180	7.5513	5.8188	11.2879
	(1.1109)	(1.8405)	(1.1591)	(1.0926)
21 to 25	7.8500	8.1490	6.2343	11.1932
	(1.2329)	(1.9460)	(1.1950)	(1.1718)
26 to 30	10.9685	12.3107	8.9362	12.7374
	(1.3189)	(2.0724)	(1.2203)	(1.3088)
31 or more	12.5225	13.8282	9.1276	13.4815
	(1.1273)	(1.6996)	(1.0468)	(1.1888)
Class rank in	0.2323	0.0205	−0.0176	−0.0709
high school	(0.0267)	(0.0258)	(0.0151)	(0.0147)
Test score total	12.1144	2.8301	2.4563	−1.0417
	(1.4197)	(1.5196)	(0.9271)	(1.0616)
Job training during	−1.4376	0.8732	1.3674	2.8242
high school	(0.7151)	(1.1221)	(0.6967)	(1.0530)
Race	−1.9184	−4.2791	−3.4954	−3.8434
	(1.3714)	(1.3949)	(0.9061)	(1.0742)
Parents' income	0.6370	−0.0120	−0.0615	−0.3025
	(0.1168)	(0.1179)	(0.0730)	(0.0673)
Dependents	4.2551	2.8608	1.8077	3.1252
	(0.5997)	(0.7401)	(0.5091)	(0.8161)
On-the-job training years	—	—	—	—
Rural	−2.5912	−0.7243	−0.3656	2.0630
	(0.9628)	(1.3676)	(0.8149)	(1.0417)
Urban	−1.4913	−2.1929	−1.2437	0.5384
	(0.8016)	(1.1968)	(0.7210)	(0.6827)
State wage	−2.1755	−0.0931	0.1629	1.6083
	(0.9391)	(1.0075)	(0.5938)	(0.6341)
State unemployment	−1.0645	−1.4112	−0.9576	−0.0160
	(0.4069)	(0.4399)	(0.2604)	(0.2551)

Table 9A.1 (continued)

Variable	Method of estimation: Tobit *with* sample selection, persons not in school[a]	Tobit *without* sample selection, persons not in school	Least squares, persons not in school	Least squares, persons in school
Test score missing	33.0777	6.6222	5.9033	−3.3432
	(4.3147)	(4.4468)	(2.8358)	(3.5306)
Class rank missing	10.1779	1.9033	−0.3888	−2.4658
	(1.9191)	(1.8276)	(1.2259)	(1.5017)
Parents' income missing	6.6426	0.4868	−0.7339	−4.6648
	(1.7567)	(1.7427)	(1.0865)	(1.2153)
Constant	27.2947	40.2316	—	—
	(5.3019)	(5.8471)		

[a]Reproduced from table 9.8, October 1972 to October 1973.
[b]Standard errors in parentheses.

Table 9A.2 Means and Standard Deviations of Variables

Variable	Mean	Standard deviation	Sample[a]
Hours worked during high school:			
1 to 5	0.088	0.2832	A
6 to 10	0.113	0.3161	A
11 to 15	0.083	0.2763	A
16 to 20	0.127	0.3335	A
21 to 25	0.127	0.3324	A
26 to 30	0.110	0.3132	A
31 or more	0.191	0.3929	A
Class rank in high school	35.833	25.8589	A
Test score total	2.677	0.8367	A
Job training during high school	0.232	0.4219	A
Race	0.162	0.3683	A
Parents' income	8.846	5.8960	A
Dependents	0.604	0.7817	A
On-the-job training weeks	1.337	3.0180	A
Rural	0.266	0.4416	A
Urban	0.290	0.4539	A
State wage	4.814	0.6820	A
State unemployment	8.359	1.9841	A
Education of mother less than high school	0.259	0.4380	B
Education of mother college degree or more	0.117	0.3209	B
Education of father less than high school	0.325	0.4684	B
Education of father college degree or more	0.190	0.3923	B
Experience:			
First year (1972–73)	0.509	0.4532	C
Second year (1973–74)	0.584	0.4471	C
Third year (1974–75)	0.730	0.3852	C
Fourth year (1975–76)	0.830	0.3003	C

[a]The statistics in this table were calculated from the data used in estimating the 1975–76 weeks-worked model and the 1976 wage model. The particular sample used in calculating the mean and standard deviation of each variable is indicated by A, B, or C.

A: Persons working and used in estimation of the 1975–76 weeks-worked equation, 2150 observations.

B: Persons used in estimation of the school attendance equation estimated in conjunction with the 1975–76 weeks-worked equation, 3100 observations.

C: Persons used in estimation of the 1976 wage equation, 2070 observations.

Notes

1. For more detail, see Levinsohn et al. (1978).
2. The number is in fact .068.
3. In appendix table 9A.1, we have presented an example of estimates for persons in school. For some parameter estimates, differences between the two groups are substantial.
4. We can give a behavioral interpretation to this model by supposing that in each year t each individual attaches some value U_{t0} to going to school and some value U_{t1} to staying out of school. The values may depend, for example, on the expected effect of each of the choices on future earnings. Suppose that both U_{t0} and U_{t1} depend on individual characteristics z_t and random terms e so that $U_{t0} = z_t b_{t0} + e_{t0}$ and $U_{t1} = z_t b_{t1} + e_{t1}$. Then assume that the no-school alternative is chosen if $U_{t1} - U_{t0} = z_t(b_1 - b_0) + (e_{t1} - e_{t0})$ is greater than zero. If we define $S_t = U_{t1} - U_{t0}$, $Z_t = z_t$, $\delta_t = b_{t1} - b_{t0}$, and $\eta_t = e_{t1} - e_{t0}$, we can attach a random choice model interpretation to the specifications defined in equations (3), (4), and (5), with the individual profit specifications interpreted as yielding reduced form parameter estimates. This is similar to the more elaborate specification used by Willis and Rosen (1978).
5. See, for example, Hausman and Wise (1977). The expected value of y_i, given that individual i is in the sample is given by:

$$E(Y_i | s_i = 1) = X_i\beta + \rho\sigma\frac{\phi(Z_i\delta)}{\Phi[Z_i\delta]}$$

6. A maximum likelihood method for doing this is laid out by Hausman and Wise (1977).
7. We have not considered hours worked per week.
8. The expected value of weeks worked is given by $E(y) = Pr(Y>52)\cdot 52 + Pr(Y<52)\cdot E(Y|Y<52)$.
9. This could be done by estimating the four weeks-worked equations jointly with the four sample selection equations. Such a procedure would also yield estimates of the correlations among the random terms in equations (2) and (5), but it also presents substantial computational complexity.
10. Even though we have used only a subsample of the whole data set, to get a given number with "good" weeks-worked data, we have to have a much larger total sample size if only persons never in school are considered to have observations on the y_t.
11. By forming for example the appropriate inverse Mills ratio and entering it as a variable in each of the weeks-worked equations.
12. It may be technically inconsistent to use weeks worked while at the same time using the logarithm of wages, since earning is usually assumed to be lognormal. That is,

$$E = Y\cdot H\cdot W, \text{ and } \ln E = \ln Y + \ln H + \ln W$$

where E is annual earnings, H is hours per week, and W is the hourly wage rate. But since our weeks-worked results suggest a slightly better fit using weeks rather than their logarithms, we have reported these results.
13. We predicted the conditional expectation of S_1, S_2, and S_3, given that a person was in the sample. We also predicted the sum of the schooling variables, conditional upon being in the sample.
14. Griliches, Hall, and Hausman (1978) found that corrections for the endogeneity of schooling increased the coefficient for schooling in their wage equation, even after correcting for sample selection. Our attempts to instrument schooling suggested, however, that the schooling coefficients in our model may be biased upward.
15. We experimented with two approaches. One was to include in the no-wage group all persons without a wage, whether they were students or unemployed nonstudents. The status equations in this case are simply sample selection equations; they cannot be interpreted as school attendance equations. The other approach was to eliminate altogether

from consideration nonstudents who were also unemployed in the October period being considered. The status equations may again be interpreted as school attendance equations, but the wage equation estimates are biased to the extent that they are affected by the elimination of unemployed nonstudents. In practice, the two procedures led to very similar wage equation estimates. (In fact, even the estimates in the status equations were affected very little by the selection procedure used.) The results reported below were based on the second method.

16. This is consistent with the findings of Freeman (1978) in a current NBER working paper.

17. The appropriate comparison may be the high school wage versus the wage with additional schooling—say a college degree. But the appropriate high school wage may be local, while the college wage may reflect a national market.

18. For a detailed discussion of the determinants of college going behavior, see Manski and Wise (1978), Radner and Miller (1975), or Kohn, Manski, and Mundell (1974). Work of Manski and Wise currently in progress suggests that blacks, once admitted to schools, are less likely than whites to choose four-year colleges, after controlling for SAT scores, parents' income, and other variables.

19. The estimated effects of hours worked in high school are somewhat lower with than without them and the estimated effects of high school training somewhat lower as well. The school attendance equations estimated with the wage rate equations as shown in table 9.12 include these two variables. Their inclusion in the wage sample selection equation has little effect on the wage equation parameters.

20. More precisely, the estimates indicate a substantial relationship if hours worked exceed 15.

21. For more details, see figure 9.1 and the text discussion of it, and the appendix.

22. $\Phi[52 - X\beta)/\sigma] = \Phi[(52 - 30)/20] = \Phi[1.1] = .8643$, if $X\beta$ is 30 and σ is 20. Sigma is close to 20 in each of the four years. For more details see the appendix.

23. The other variables included in the regression are: Number of siblings 0.01 (0.01), State wage − 1.62 (0.90), State unemployment − 0.03 (0.38), Rural − 1.87 (1.03), Town − 1.85 (0.99), Urban − 1.88 (0.98), South −0.19 (1.25), East − 1.32 (1.19), West − 0.55 (1.38), Test score missing − 9.37 (2.76), Class rank missing 0.55 (1.18), and Parents' income missing 1.63 (1.05).

24. Griliches (1977), in an analysis of the Parnes National Longitudinal Survey, as well as National Longitudinal Study data, also found that work in high school was virtually unrelated to family socioeconomic variables.

25. At least in the first two years the estimates are not precisely measured. The state wage may be considered as an instrument for the individual wage. Possibly it is too weakly related to individual wages to pick up any labor supply effect that might be present. A more highly parameterized instrumental variables specification of the model might yield a different result. One interpretation of our results is that the higher the "going" wage the less likely are employers to be willing to fill jobs with youths.

26. Indeed, the estimates imply a relationship that is slightly U-shaped, with hours between 16 and 30 generally associated with slightly lower wage rates than high school hours of either more than 30 or greater than zero but less than 16.

27. Of course, high school work could enhance one's ability to learn from later work experience, and thus a decline in the effect would not necessarily be expected.

28. If the same specification as shown in table 9.11 is used, but with two high school training variables—one for persons with a high school degree only, and a second for persons with post-secondary education—the results are similar; the relevant coefficients are not significantly different from zero for either group.

29. We cannot rule out the possibility that family income captures individual attributes contributing to job performance and associated with income, although we think this is unlikely having controlled for school performance and other characteristics. Wise (1975a,

1975b) found that family background was not related to earnings or performance in a given corporate job setting after individual academic and nonacademic characteristics were controlled for.

30. This is consistent with other estimates of this type. See, for example, Griliches and Mason (1972) or Hausman and Wise (1972). A comparable monthly coefficient in Hausman and Wise, for example, is .0063 versus our estimate of .0060 in 1976.

31. In fact, the number of October time periods that the person was in school.

32. We experimented with the instrumental variable approach described in section 9.2 using the expected value of school in a given year, conditional on being in the labor force in that year. It did indeed yield lower estimates, but because we were not satisfied with the procedure in general, we have reported the uncorrected estimates. In subsequent work, we will set up a more highly specified simultaneous equations model that allows for the simultaneous determination of school and work experience, together with a sample selection equation. Such a system is cumbersome to apply over repeated years when the ß parameters are estimated separately for each year. Because this was a primary concern of our analysis, we elected to work with a less complicated specification in this investigation.

33. The increase in salary with experience is generally higher for white collar workers than for blue collar workers.

34. See, for example, Freeman (1976a).

35. If there is an endogeneity bias with respect to experience, its sign is not clear. Because schooling is positively related to the error in the wage equation, and experience is accumulated only if a person is not in school, the estimates would tend to be biased downward. On the other hand, the error in weeks worked is positively related to school attendance. Experience may also be endogenous in that it may be determined in part by past wages and the wage disturbances are correlated over time.

36. And again, there may be some compounding of results because of the entry of recent college graduates in the last time period.

37. Although this terminology is intuitively appealing when there are discrete distinct states that are not artificial divisions of a continuous measure, it may be a misnomer here. Still we will stick with it.

38. A straightforward way to estimate the population covariance is to obtain joint maximum likelihood estimates of the parameters, including a covariance term, for each pair of years. This is expensive when different ß parameters are allowed for each year. An easier and less expensive procedure is to use estimates for individual years (like ours) to obtain consistent estimates of the ß parameters for each year, and then to use them in a second maximum likelihood stage to estimate covariances, assuming the means implied by the ß's from the first stage. On the basis of preliminary analysis, however, we concluded that these alternatives would not change the conclusions that we reach on the basis of covariance estimates conditional on being in the labor force.

These same alternatives could be used to obtain population covariances among the weeks-worked disturbances, with the added complication of the upper limit on weeks. We will pursue this in subsequent work. But as with the wage disturbances, we concluded that the substance of our conclusions would not be changed by a more precise and detailed analysis.

39. For a more detailed analysis of state dependence following a somewhat different procedure, see Ellwood (chapter 10 of this volume). Related analysis is also contained in Brown (chapter 12 of this volume).

40. The correlation for any two periods is based on persons who were in the sample in both periods. The residuals are calculated conditional on being in the sample (not in school).

41. We also calculated a correlation matrix for the logarithm of nominal wages based on the sample of persons who had a recorded wage rate in each period (447 out of 3280 who worked in at least one of the five periods). The correlations are quite close to those shown

here, although the correlations between adjacent years are a bit larger in the later years—.811 between 1975 and 1976—indicating somewhat greater stability after four years than for the sample as a whole. The correlation between 1972 and 1976 is .319, slightly smaller than the one shown in the text.

42. Although our procedure is consistent, it is not efficient. A correct procedure would use a minimum modified χ^2 procedure analogous to generalized least squares. Given our purpose and relatively large sample size, we did not pursue this approach.

43. These are asymptotic standard errors based on the maximum likelihood estimation procedure and the associated information matrix. They should be considered only as illustrative. A more efficient and consistent procedure would take account of the variance-covariance matrix of the initial covariance matrix estimates. Such a procedure is described in Hausman and Wise (1978). Because our original sample is so large, we suspect that the marginal gains from using this procedure would not be great.

44. Recall that if all variation were due to individual specific terms, correlations over time would be one; they would also be one if state dependence were extreme so that persons could not "change states".

45. The correlation matrix of weeks-worked disturbances is based on an earlier set of statistical results that are substantially the same as those reported in table 9.8. A correlation matrix based on nominal weeks worked for persons always in the sample—728 out of 2933 who were not in school in any of the years—reveals no systematic differences from those shown here.

46. These conclusions remain unchanged if the matrices are based only on persons who were not in school in any of the four years.

47. Relationships like those described in this section hold as well for persons who were in the labor force in each of the periods, who had no post-high school training.

48. See greater elaboration on this point in Ellwood (chapter 10 of this volume).

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Comment Frank Levy

I will begin by saying I enjoyed the Meyer and Wise chapter very much. In particular, the authors' presentation of their data and discussion of their methodology make the chapter a pleasure to read. It contains the clearest discussion of the self-selected sample problem I have seen to date. In what follows, I will criticize not so much what the authors did do but what they didn't or couldn't do. This technique—discussing the paper the authors should have written—is normally the height of bad form. It is necessary in this case because the chapter appears not in isolation but in a volume on teenage unemployment.

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The majority of chapters in this volume are directed to a common underlying question: How seriously should the country regard teenage unemployment rates? As evidenced by the Fedlstein and Elwood chapter (2), the question contains a red herring. Most teenagers are white and when one corrects for macroeconomic conditions, white teenagers, if anything, are doing better over time. Table C9.1 presents aggregate labor force statistics for black and white males and females, ages 16–19, for 1964 and 1978—two years that were roughly comparable in macroeconomic terms.[1] These data indicate that the correct underlying question should be: How seriously should the country regard black teenage unemployment rates? Because Meyer and Wise focus on males, I will do the same.

One's opinion of teenage unemployment depends upon a variety of factors. One of the most important is surely whether or not teenage unemployment indicates the presence of long run-problems. Meyer and Wise answer this question with a "Yes, but . . . " That is, hours worked in high school have a statistically significant and surprisingly long-lived impact on weeks of work in the years after graduation. But these differences take place in a context where levels of unemployment and, in particular, racial differences in those levels are smaller than one might imagine from the Current Population Survey. This context—the general lack of serious problems—is the bottom line of their story and so it bears some scrutiny.

Table C9.2 contrasts two sets of labor force statistics. The first are NLS statistics from Meyer and Wise's table 9.1 for 1975–76, the last year of the sample. In this year most of the NLS young men would have been about 21 years old. The second set of statistics are calculated for 20 and 21 year old young men from the March 1976 Current Population Survey. Both sets refer to young men who are out of school.

In contrast with the NLS, the CPS numbers suggest that things are not so rosy and that black/white differences are relatively large. There are, however, a number of ways to explain the differences.

The first is the exclusion from this NLS sample of persons who did not graduate from high school. The exclusion is important: CPS tabulations for 1976 indicate that among young black men out of school aged 20–24, 27% had less than a high school education. A second factor *may* be the geographic distribution of the NLS sample. Young black men differ from other young race-sex groups by historically showing particularly poor labor market experiences in central cities. In 1976, for example, the CPS shows out-of-school young black men aged 20–24 have an unemployment rate of .27 in central cities and .15 out of central cities. Despite these differences, lower black family incomes are still associated with rural areas and it may be that the NLS sample, in order to find more low income blacks, focused the sample in that direction.[2] Finally, part of the

Table C9.1

	Black males			White males			Black females			White females		
	E/P	*U*	*LFP*	*E/P*	*U*	*LFP*	*E/P*	*U*	*LFP*	*E/P*	*U*	*LFP*
1964	.33	.23	.43	.38	.17	.46	.16	.35	.25	.30	.12	.34
1978	.23	.42	.40	.50	.15	.59	.17	.44	.31	.47	.14	.55

SOURCE: Current Population Survey for March 1964 and March 1978.

Table C9.2 1976 Labor Force Statistics for Out-of-school Young Men from the NLS and the CPS

	NLS (HS graduate only)		CPS (All men out of school aged 20–21	
	Whites	Blacks	Whites	Blacks
U	.07	.08	.15	.20
E/P	.90	.97	.79	.69
LFP	.97	.95	.93	.86

SOURCE: Meyer and Wise, table 9.6.

NLS/CPS difference may be accounted for by respondent misreporting (see Freeman and Medoff, chapter 4), but this explanation, to the extent that it is correct, seems more applicable to the level of labor market performance than to black/white differences. To summarize, Meyer and Wise's description of their data as a cohort, particularly for blacks, may not be representative. Correspondingly, smooth movement and serious employment problems can exist simultaneously, though among different subgroups.

The central finding of the Meyer-Wise chapter is the importance of work while in high school on subsequent labor market experience. This finding is surely correct. Analysis of the CPS data indicates that a young adult's labor market performance in a given year is strongly associated with his weeks of work in a preceding year even after his other characteristics are controlled. The question is: How does one interpret such a finding? Meyer and Wise offer two possible interpretations. First, the experience of working may actually impart useful skills to teenagers, general skills that may help teenagers in later market experiences. Alternatively, those who work in high school may be self-selected and have a relatively strong propensity to work even when all other variables are held constant. The first interpretation argues strongly for reorienting vocational programs toward cooperative education and other actual work experiences. The second interpretation weakens the first by assigning positive impact not to work but to the intrinsic (and unobserved) characteristics of persons who work.

In sorting through these competing interpretations, it is worth considering a third interpretation that lies somewhere between them. Hours worked in high school may say something about people who can get work; people who have not only the motivation, but the other attributes that permit them to become employed. Many of these attributes, while uncorrelated with motivation, may be equally unobservable in a typical data tape. As the labor market skews increasingly toward services, for example, a teenager's ability to deal with the public—his style—may become as important as any other factor in his employment prospects.

Trying to separate these hypotheses is important, particularly for young black men. The principal factor behind their deteriorating employment rates contained in table C9.1 is a rapid increase in the unemployment of young black men in school. In 1964, the in-school unemployment rate for young black men was .22, and roughly two out of every ten unemployed black young men aged 16–19 were in school. By 1978, their in-school unemployment rate had risen to .53 (.65 in central city schools), and six out of every ten unemployed young black men were in school. This increase was far in excess of any increases in black school enrollment and it occurred despite their constant level of in-school labor force participation. Changing attitudes and rising transfer payments undoubtedly played some part in these statistics, but common sense suggests the importance of falling demand. Casual discussion with high school pacement officers in Washington, D.C. and in the surrounding suburbs—placement officers who look for part time jobs for students—reinforce the idea that, with respect to job vacancies, the suburbs are a different world. All of this, then, argues in support of Meyer and Wise's principal policy conclusion that within vocational education, more emphasis should be put on finding people jobs. To say this, however, is only to begin the process. Within an inner city vocational education high school, most instructors will readily acknowledge that their courses—e.g., diesel mechanics—are substantially enhanced if the student can get a co-op placement in the same field. But finding such placements is very difficult and it is not clear to me what federal, state, or local government agencies can do to help.[3]

A quick reading of Meyer and Wise might suggest that I pose too hard a problem. They seem to argue strongly that what is important is work per se—bad jobs do not necessarily lead to bad jobs and so any first job will do, whether or not it is involved with the student's field of study. This conclusion cannot be taken too literally. In the first place, the period over which the NLS data was collected saw little federal provision of jobs for in-school youths. Thus most of the work experience recorded in the NLS tapes was private work experience. In the typical private organization, there is a greater tendency to think about costs and assign them to individuals. When a teenager messes up, everybody knows what is lost and the teenager's supervisor takes some heat. Correspondingly, there is a greater incentive for some discipline to be part of the workplace. Such discipline will be present in government projects only insofar as vocal people—people who can create a flap—care about the output that is being produced. Thus, Meyer and Wise's results probably refer to hours of actual work rather than hours of holding a job. Along the same lines, it should be noted that any student who worked an *average* of twenty hours or more per week during the school year was probably not working in just any job. The more menial the job, the more likely it is associated with high turnover and resultant unemployment, either because the employee quits or the employer fires him. These arguments suggest that while government policy should help create work for teenagers, purposeless work may be as bad or worse than no work at all.

While Meyer and Wise's findings on unemployment may be unrepresentative, their findings on wages seem to be correct. When other factors are held constant, racial factors per se exert relatively small influence on hourly earnings. In one sense this is cold comfort, because in life all other things are not held constant. Nonetheless, such results narrow the variables with which policy makers should be concerned.

What, then, is one to conclude? Seven years ago, Anthony Pascal and Leonard Rapping wrote a classic article testing for discrimination in organized baseball.[4] They chose the subject not only because (I assume) they were sports fans, but because baseball, more than most other team sports, is a sum of individual players in which each player has statistics—batting average, fielding average, etc.—that accurately reflect his product. They found that if a black man made it into the big leagues, his characteristics were paid for on the same basis as with white males. But they also found that holding constant all factors except race, a black was much less likely to make it into the big leagues. The labor market for black men today seems to resemble baseball writ large. If a black man finds work, he is likely to be paid on a par with white men with similar characteristics. But the chance that a black man will find work is less than the chances for a similar white and, if anything, seems to be marginally declining. Meyer and Wise have offered one important insight into how that process might be reversed. One can hope it will be utilized in future policy design.

Notes

1. For example, in 1964 the unemployment rate for all men aged 25–54 was 3.2%. In 1978, the corresponding unemployment rate was 3.4%.

2. In fact, Meyer and Wise do find a negative impact on weeks worked for living in a city, particularly for the sample's early years. This impact arose from a single urban variable,

however, and was probably muted by the fact that whites do not seem to have such central-city problems. Had city residence been allowed to interact with race (or, more appropriately, had separate analyses been done for blacks and whites), a sharper picture would have emerged.

3. In fact, 1978 legislation created substantial federal tax incentives for the employment of low-income young adults, including both full time jobs and co-op placements. Unfortunately, these tax credits have not been widely publicized and so they seem to be known primarily to large-scale employers—e.g., McDonalds—who can afford to scan constantly new federal laws. By contrast, large numbers of small-scale employers, placement counselors, and youths themselves seem to be unaware of the credits.

4. Anthony H. Pascal and Leonard A. Rapping, "The Economics of Racial Discrimination in Organized Baseball" in *Racial Discrimination in Economic Life*, Lexington, 1972.

Comment Gary Chamberlain

This very informative chapter carefully presents a great deal of empirical detail. The concluding section of the chapter gives a concise statement of the authors' findings, only some of which will be referred to in my comments. The authors have been particularly aware of problems caused by sample selection bias, and my discussion will focus on the issues raised by their treatment of these problems.

The core of the model is a wage equation, a weeks-worked equation, and an equation for being out of school. The wage equation is estimated jointly with the out-of-school equation; the weeks equation is also (separately) estimated jointly with the out-of-school equation. This procedure is repeated for each of the five years.

The authors are concerned that the regression function for wages (or weeks) conditional on being out of school differs from the regression function that does not make this distinction. This latter regression function corresponds to imputing a wage to the in-school individuals, and then fitting a regression of wages on the current level of schooling and other characteristics for the entire sample. In thinking about this distinction, I found it useful to consider a third regression function, namely, wages on schooling and the other variables, once everyone has finished schooling. As we follow the sample, the authors' procedure will eventually, once everyone is out of school, give a least-squares estimate of this regression function. Then we shall be conditioning on the amount of schooling that the individual eventually attains; the selection of being out of school is roughly that this ultimate level of completed schooling is less than current age minus six. So if we are making inferences conditional on completed schooling and age, no additional conditioning is required; hence there is no sample selection bias.

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For a variety of reasons we might consider schooling to be endogenous and use an instrumental variable estimator on the complete sample. There would then be a sample selection problem if we were to use this estimator on the out-of-school subsample, since an instrumental variable that satisfies orthogonality conditions in the full population will not satisfy them when we condition on schooling.

Similar arguments apply to the weeks equation except that schooling has been excluded from the weeks equation. An interpretation of the weeks equation as a reduced form labor supply schedule implies that all the variables in the wage equation should appear in the weeks equation. Schooling and experience, however, are excluded. The authors note that schooling is not significant when it is included; but if one argues that its regression coefficient is really zero, then there cannot be a sample selection bias from selecting on an irrelevant variable.

Nevertheless, the authors do find evidence for sample selection bias, particularly in the weeks equation in the first year. This is based on the estimated correlation between the disturbance in the weeks equation and the disturbance in the latent variable equation that generates the probit probability of being out of school. Alternatively, we can think of including a term (the inverse of Mill's ratio) in a regression for the out-of-school subsample. This term reflects the probability of being in the sample; a nonzero coefficient is evidence for sample selection bias. Note that this term is a nonlinear function of the variables in the probit equation. These are essentially the same variables that appear in the wage or weeks equation. So an alternative interpretation is that a nonlinearity has been found.

Now one might argue that it does not matter which interpretation we follow: a specification error has been detected and corrected. This is incorrect. Under the sample selection interpretation we would evaluate the effect of the explanatory variables by the linear term in the specification that includes the Mill's ratio. Under the functional form interpretation, we would evaluate the derivative of the nonlinear function at some average level of the explanatory variables; this often gives results similar to fitting a linear least-squares approximation. If the functional form interpretation is correct, then just looking at the linear term corresponds to measuring the effect of the explanatory variables at extreme (infinite) values; for only then is the truncated mean of the disturbance term independent of shifts in the truncation point.

I want to consider next some of the issues that arise when schooling is considered to be endogenous. Suppose that wages are affected by unobserved variables, which might correspond to different sorts of initial ability. The authors' procedure amounts to imputing a wage to those still in school, and then fitting, for the entire sample, a regression of wages on current schooling and on other variables. Consider the coefficient of schooling in this regression. In the second year, the schooling variable is an indicator for whether or not the individual went directly on to college after high school. The coefficient on this variable partly reflects a value added from that year of college, but it also reflects the mean ability differential between high school and college students. More precisely, in the first year of the sample, when everyone has the same amount of schooling, there is a differential between the mean wage of high school students and the (imputed) mean wage that the college students would have received had they not gone on to college. In subsequent years, the estimated coefficient of schooling will confound the value added of schooling with this mean wage differential.

This point is relevant for measuring the effect of vocational training. Suppose that people only get vocational training if they do not plan to attend college. Then the coefficient of vocational training in the full sample is partly measuring the imputed mean wage differential between high school and college students. If the mean wage of high school students is less than the imputed mean wage of college students in the first year of the sample, when everyone has the same amount of schooling, then the estimate of the vocational training effect is biased downward. A more appropriate measure might be based on comparing the measured wages of high school students with vocational training to the wages of high school students without it. There would still, of course, be the problem that the selection of high school students into vocational training programs is partly based on unmeasured differences in abilities.

Another sort of sample selection issue arises because each year new people enter the sample as they complete their schooling. The experience variable is specified by the year in which it occurred. So in the last year of the sample (1976), a year of experience in 1975 is assumed to have the same coefficient for a high school student, for whom it may be the fourth year of experience, and for a college graduate, for whom it is the first year of experience. Curvature in the profile, due to the investment content of a year of experience varying with the level of experience, is not permitted. The variables are freely interacted with time. Because of the changing sample composition, however, time and experience are not identical; yet at several points in the chapter, changes in the coefficients over time are interpreted as if the same group of people were being followed as the group accumulated experience. In particular, the patterns of change in test score coefficients and schooling coefficients are obscured by the continual entry of people with no experience.

These points have some relevance, I think, for the estimated effects of schooling, test scores, and experience, and the pattern of their changes over time. The policy implications of the paper, however, rest more on the lack of a vocational training effect and on the strong relationship between work in high school and subsequent weeks worked. I have commented on how the estimation procedure affects the measurement of a vocational training effect. An additional point, as the authors note, is that it would be useful to distinguish whether or not the training was given by a vocational high school. As for the relationship between work in high school and subsequent weeks worked, the authors point out that the correlation does not decline over time, whereas the correlation between weeks worked in different years after high school rapidly declines as the time interval between the years increases. This suggests that the relationship between work in high school and subsequent work may be due to unmeasured characteristics of the individual that are not affected by work in high school. In that case, the policy implications are not clear; but it certainly is an intriguing finding and an invitation for futher research to determine what work in high school is measuring.

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