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ABSTRACT

The study's objective were: (1) to describe the patterns of work and welfare experience in low-income families and to explain their causes, and (2) to apply the findings to the policy problem of deciding which groups of welfare recipients should be required to work and provided with what mix of manpower services. The effectiveness of work registration requirements was analyzed. The intention was to isolate the extent to which the welfare system itself causes job instability. The data sources were the Graduate Work Incentive (Negative Income Tax) Experiment and the Panel Study on Income Dynamics. The many findings are reported in terms of (1) work patterns, (2) the effects of welfare on work, and (3) welfare dependency. It was learned that those who can and cannot work are not easily distinguished on the basis of characteristics that could be specified. Labor market problems are not clearly linked with demographic characteristics. Overwhelmingly, males in the low-income population move from welfare to work on their own, and so, apparently, do most female heads of low-income families, over time. However, there is much movement from work to welfare, and little movement out of the low-income ranges. (Author/AJ)

WORK AND WELFARE PATTERNS IN LOW INCOME FAMILIES

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Work and Welfare Patterns in Low Income Families

Summary and Conclusions

I. Objectives

The objectives of this study originally were two: first, to describe the patterns of work and welfare experience in low-income families and to explain the causes of such patterns; secondly, to apply the findings on work and welfare patterns to the policy problem of categorization: deciding which particular groups of welfare recipients should be required to work and provided with what mix of manpower services.

Conventionally, persons in the low-income population are examined at a point in time and then characterized as being "on welfare," or "employed," or "employed while on welfare." Thinking in terms of these categories, the public and its official representatives seek then to move people from the welfare category to either the employed or the employed while on welfare categories. This study was directed primarily towards discovering the usual patterns of movements between these categories, thereby determining the value of the conventional categories and, by inference, of program objectives framed in these common terms. Thus, the study sought to answer questions such as whether, if examined at a second point in time, those initially on welfare still would be there; and whether those initially employed still would constitute the bulk of the employed or whether the employed would consist of a new set of people. If there usually is a substantial flow among the categories, then programs which affect this flow must be judged not on the basis of whether or not their participants change status, but rather whether the program induces the desired change sooner than it would otherwise happen. Similarly, program success depends on whether undesired changes are either prevented or delayed.

In analyzing the causes of work and welfare patterns over time, the ~~intention~~ of the study was to isolate the extent to which the ~~welfare system itself, as~~ opposed to personal or labor market factors, caused job instability. We hoped to apply these findings to the problems of categorization. Fundamentally, the results were not used to yield a set of categories because it was learned that associated with a small set of identifiable demographic characteristics was a wide range of labor market experiences. Thus, those who can and cannot work or those who need a particular package and quantity of manpower services are not easily distinguished on the basis of characteristics that could be specified in laws or regulations. Well-specified categories or groups requiring services are difficult to generate. Another component of a categorical program often is a work test. It can be studied separately. The second phase of this project is designed to analyze the effectiveness of work registration requirements in overcoming voluntary or induced instability.

II. Data Sources

The data for the study comes from two sources: (1) the Graduated Work Incentive Experiment, sometimes referred to as the Negative Income Tax (NIT) Experiment and (2) the Panel Study on Income Dynamics. The NIT experiment was conducted in the period 1968-1971 in four cities in New Jersey and Pennsylvania and was managed generally by the Institute for Research on Poverty of the University of Wisconsin. The data from this experiment are derived from a set of thirteen quarterly interviews, administered over a 36-month period, one at the outset of the experiment and then at intervals of three months during the actual experiment. Originally, 1357 families, whose incomes were below 150 percent of their respective poverty lines, were selected. Some of these received any one of a variety of experimental treatments under a negative income tax, while the rest were assigned to a "control group." Most of our work is done with only 894 (of the 1357) families, chosen because continuous information on their welfare and experimental experience was available to us. These data are referred to in this study as the "Wisconsin data."

The Panel Study on Income Dynamics covered the period 1967-1971 for a nationally representative sample heavily weighted, though, with low-income families. A survey was conducted by the Institute for Social Research at the University of Michigan of 5000 families, each interviewed annually for five years. Our study is based on a subsample of 1635 families referred to as the "Michigan data." The families selected for the subsample had income in the bottom fifth of the income distribution in any of the five years. Further, the subsample was restricted to families which were essentially intact (except for the possible departure of any member) and whose head was not over 60 in the first year of the study.

Neither the Wisconsin nor Michigan data contain continuous work and welfare histories. Thus, we could not trace the week-to-week or even month-to-month experiences of families in the samples. In the Wisconsin sample, respondents provided information about the nature of their labor force activity and earnings only for the last week of each of the twelve quarters; about their presence on a cash welfare or the NIT experimental program at any point during a quarter; and about the size of their welfare and NIT payments for the quarter. In the Michigan sample, respondents provided information on their labor force activity and earnings for the previous year, as well as on the total amount of welfare payments received by the family during the year. The major limitations of these two data sets in studying work and welfare patterns is that they do not record changes in labor force and welfare status within a quarter (or year) and the reasons for such changes. In offering periodic information, however, on earnings and welfare payments, the data sets do present a unique opportunity to deduce a general picture of patterns in employment and welfare status and their causes. In sum, the data do not allow statements about the duration in weeks or months of welfare or unemployment spells. But they do permit an analyst to capture important aspects of fluctuations over time in work and welfare experience.

III. Findings

A. Work Patterns

The work patterns of males and females in both the Wisconsin and Michigan data sets were investigated separately.

Among male heads of families in the low income population, there is a variety of work patterns and substantial evidence of fluctuations in employment status and earnings over time. At any point in time during the NIT experiment, the Wisconsin data indicate that roughly 86 percent of the male heads were employed. During the three year experiment, however, roughly 96 percent of the males who basically remained with their families worked at one time or another. Similarly, in the Michigan data, we found that over time almost all male heads worked at one point or another. Over a five year period, 96 percent of the male heads worked at some time. Thus there is not a fixed group of employed working poor. Rather there is a flow of males through employment, with the group as a whole evidencing a high degree of labor force attachment.

Besides the small proportion of regularly non-employed males (who typically suffer from some disabling condition), other groups of workers in the Wisconsin sample can be identified by their work patterns. One interesting group, roughly one-fifth of the total, averages more than 41 hours per week during the entire experimental period. A majority of these men has substantial fluctuations in earnings, but the fluctuations do not result from unemployment. They result mainly from fluctuations in overtime hours or from moving in and out of moonlighting jobs. These very hard workers tend to be young, healthy, more educated, but nevertheless poor or near poor. Another group containing over 30 percent of the total consists of men who work steadily at the full-time level. Thus about half the Wisconsin sample consists of men who worked at least full-time during the entire experiment. In the Michigan data, we also find a sizable group of stable workers: nearly two-thirds of the male heads averaged 1800 hours or more per year over the five years.

Now consider the remaining half of the Wisconsin sample which is characterized by some degree of employment instability. One group, about 30 percent of the total, shows great instability. When working, these people work full-time and earn wages similar on the average to those of other groups. ~~However~~, they often are unemployed and change jobs frequently. The remainder experience one or two brief spells of unemployment, but work most of the time.

Although the study concentrated on males, substantial attention was devoted to the work effort of females. In the Wisconsin data, roughly only 15 percent of the female spouses were employed at any point in time. Interestingly, in the Michigan data 77 percent of the female heads of families worked at some time during the five years. And over one-third of the latter group averaged 1800 hours or more of work per year over the five years.

B. Effects of Welfare on Work

Work effort is affected by welfare policies. Both implicit tax (or benefit-loss) rates associated with earnings as well as welfare benefit levels have small but statistically significant effects on the quantity of work effort. Welfare programs of the sort studied generally do not discourage work altogether but may discourage it temporarily. The effect shows up in either increased fluctuations in work effort or longer spells of unemployment. When the men work, it is largely at full-time work, plus possibly some overtime. Also, work response to changes in welfare programs differs by race -- with whites being more negatively affected than blacks -- as well as according to the program already in place.

Suppose, for example, that the welfare guarantee is increased by $1,000 from an initial level of $2,400, which is close to the current national average guarantee available to a male-headed family of four, including AFDC-UF or General Assistance and Food Stamps. Using the Michigan data, for white males we predict a reduction in annual earnings that ranges from $210 per year at a benefit-loss rate of 25 percent to $525 at a benefit-loss rate of 70 percent. At a wage rate of $3.80 an hour, these amount to reductions in annual hours of work of 55 and 138 respectively. For Blacks and persons of other races, the similar reductions are $129 and $323. The corresponding reductions in annual hours of work at a $3.80 hourly wage are 34 and 38.

Now suppose that the welfare program benefit-loss rate is increased by 10 percentage points from an initial level of 40 percent. At a guarantee of $2400 for white males, the predicted decline in earnings is $126, or 33 hours. Again, the induced decline in earnings is lower for males who are black or of other races, amounting to $78 or 21 hours.

C. Welfare Dependency

The extent of welfare dependency is affected by the labor market experience of individuals. But it also depends greatly on the characteristics of the welfare program they face. Dependency, measured by time spent on welfare or amount of payments received over time, can be influenced markedly by simple changes in program characteristics, even if work behavior is completely uninfluenced by the program changes.

This fact can be illustrated by the Wisconsin data where we found, not surprisingly, that males who averaged high earnings during the experimental period received lower welfare payments than did those with low earnings. But whereas the differences between the two groups were substantial when considering regular welfare, they were relatively minor when looking at NIT payments. Unlike the regular AFDC-UF program, the NIT plans allowed families with working heads to receive payments and earnings simultaneously. Thus, men with "unstable-low" earnings who faced one of the NIT treatments received NIT payments averaging $255 per quarter compared to $231 for those with "stable-high" earnings, a difference of only $24 per quarter. In contrast, the difference is much greater for recipients of AFDC-UF, where men with "unstable-low" earnings received an average of $172 in AFDC-UF payments per quarter compared to $53 for those with "stable-high" earnings.

IV. Policy Implications

A. A major objective of welfare programs is to move people from "welfare to work." It is necessary in this connection to distinguish a short-term success -- getting a welfare recipient to work -- from a long-term success -- getting a recipient to work in a situation where the probability is very low that he will leave work and return to the welfare rolls. Overwhelmingly, males in the low-income and near low-income populations typically move from welfare to work on their own. Over a stretch of time, most female heads of families in these income groups appear to do likewise. But of equal significance is the fact that there is much movement in the other direction, from work to welfare. While there is much movement between work and welfare, there is little and slow movement out of the low-income ranges for most families finding themselves there. A program, therefore, which seeks to move people from welfare to work may be successful on a short term basis but unsuccessful on a long term basis. In evaluating short-term results, a program may overlook the possibility that its long run effects may be quite different.

These arguments suggest possible refinements in the measurement of program success. Thus, a welfare recipient returning to work is only a partial measure of program success. The critical element is how rapidly the change is made. Moreover, there is an additional dimension of success -- how long will it last. The program will be more successful, obviously, the more it stabilizes the employment of low-income persons (who eventually might again become dependent on welfare).

B. If movement in and out of employment and welfare dependency is common among low-income persons and if reductions in work effort induced by welfare programs take the form of longer stretches of unemployment, government monitoring of work effort becomes very difficult. In this context, administrators of a work registration requirement become responsible for reducing the duration of stretches of unemployment, a task difficult both to perform and to measure. A short-term program success would be a reduction by a work requirement in the stretch of unemployment in comparison to what would have taken place in the absence of the work requirement. Moreover, short-term success could conflict with the prospect of long-term success to the extent that welfare recipients are forced to foresake search for more attractive and stable jobs.

C. Moderate liberalization of welfare programs does not run the risk of eliminating work effort among the poor in general. Work and welfare will continue to go together, both serially and simultaneously. But liberalization may induce more cutbacks in work among some workers, as returns to work are delayed, overtime and moonlighting reduced, and voluntary job separations increased.

D. Liberalization of welfare programs will extend welfare dependency -- simply as a matter of arithmetic. Raising benfit levels, for example, extends coverage and makes it more difficult for people to become totally ineligible. If work effort is affected negatively by liberalization, then dependency will increase for a second reason.

E. Attempts at categorizing people on the basis of a small set of identifiable work-related characteristics for the purpose of providing them with different welfare or manpower program "treatments" will be frustrated by the fact that labor market problems are not clearly linked with particular demographic characteristics. People with the same characteristics have widely varying labor market experiences. Thus, we are unable to develop a set of simple rules that eliminates the need for case-by-case discretion.

CHAPTER I

Welfare Turnover: A Review of the Literature

Welfare program administrators often are committed to moving recipients from "welfare to work" and thus, presumably, off of welfare. In order to evaluate their success in achieving this objective, three issues need to be considered.

1) It is necessary to distinguish short-term from long-term success. A person may move from welfare to work only to return again at some future time. It is thus desirable to investigate the extent and circumstances of recidivism among welfare recipients.

2) There are a number of ways of viewing and measuring short-term success. One could simply count transitions from welfare to work, a procedure useful primarily in distinguishing permanent from transitory recipients. Since so many recipients are transitory, further information about these can be gained by considering the timing of transitions, for example, by measuring the average length of a spell on welfare and the frequency of such spells. Further refinement is possible if one recognizes that a recipient may obtain a job and nevertheless remain on welfare (at least under some forms of welfare programs). Although welfare payments are still received, they are reduced downward. A reduction in a welfare payment thus may be at least a partial success even if the person does not leave welfare altogether. Such considerations become more relevant as the welfare system is liberalized, but they require measurement of changes in welfare payments as well as transitions in welfare status.

3) The causes of observed welfare patterns need to be considered. Obviously, the work patterns of welfare recipients affect their welfare patterns. The next chapter reviews the literature on work effort. In addition, however, the structure of the welfare program may have a significant effect on a family's welfare pattern. By raising benefit levels, for example, a welfare program makes it more difficult -- in a purely mechanical sense -- for recipients to reduce their welfare benefits to zero and thus leave welfare.

Most of the above issues have been studied previously to some extent. This chapter will review previous findings and indicate the principal shortcomings in past work.

Introducing their study of turnover in the California AFDC program, Boskin and Noid, assert that "among the many badly mistaken popularly held views about welfare, especially AFDC, is the view that the population of recipients is more or less permanently entrenched in a welfare dependency status."[1] Similarly, in evaluating their findings from a study of turnover in the New York City welfare programs, Rydell, et al, claim that "by tapping new sources of data on welfare case histories, [they] replace the popular notion of a 'permanent', static welfare population with a comprehension of its true dynamic, changing nature."[2] That there is not a permanently dependent welfare population, especially in the categorical programs offering assistance to families with children, is the unanimous conclusion of prior studies of welfare dynamics. This conclusion, although it reflects an accurate description of the available data, provides an incomplete view of welfare turnover. What has been ignored or given insufficient weight in the interpretations of existing studies is the impact of the characteristics of welfare programs themselves on turnover. Benefit levels, benefit-loss (or tax) rates, income accounting systems, work registration requirements, and the myriad of other welfare rules and their administration all affect turnover -- even if they have no impact on recipient behavior -- by determining the conditions of their eligibility.

High welfare turnover, an undeniable phenomenon under AFDC, in part reflects substantial short-run fluctuations in the non-welfare incomes of low income households. It masks, however, the relatively small degree of upward movement in the annual incomes of such units: progress out of poverty simply is not extensive and dramatic. High welfare turnover presently results from short-period fluctuations in income interacting with a welfare system that was designed to be responsive to such changes. Although both by design and administrative default the system of late has become somewhat less responsive to income fluctuations, the high turnover that remains is substantially an artifact of a particular set of welfare system rules and their often arbitrary administration. It requires no great imagination to develop different welfare turnover patterns, given the same set of non-welfare income patterns, with minor alterations in the characteristics of the welfare system. A convenient example is that the migration of an AFDC household from one jurisdiction to another results in a case closing and, most probably, a case opening. Complete federalization of AFDC, therefore, would reduce turnover that results from geographical migration.[3] In general, were AFDC to become responsive largely to long run changes in family income, the turnover emphasized in existing studies would be attenuated dramatically.

Besides measuring the extent of turnover, previous studies relate turnover rates to the personal characteristics of recipients, mainly to those reflecting their capacity to earn income. The findings generally are consistent among studies and not surprising: the duration of a spell on welfare is shorter the younger and more educated the family head, the smaller the size of his family, and the higher his potential wage. These studies are reviewed in this chapter. Prior to reviewing the studies on turnover rates and their relationship to the characteristics of welfare families, we illustrate how changes in a welfare program can affect welfare turnover.

A. Welfare Turnover Under Alternative Program Characteristics

The limited literature on welfare dynamics focuses on the links between welfare turnover and personal characteristics, giving some attention also to the effects of the economic environment. With a notable exception, it inadequately emphasizes the links between welfare program characteristics and turnover. As a preface to our literature review, we offer some illustration of the obvious point that, given a particular pattern in which income accrues to a household, its welfare experience will vary with welfare program parameters.

Table I-1 contains the income pattern of a hypothetical household. The head experiences two stretches of unemployment, each preceded by a period in which the

TABLE I-1

Welfare Benefits and Welfare Turnover Under Alternative Program Characteristics

Month	Monthly Income	$g_m = 300$, $t = .50$, Monthly Accounting[a]	$g_m = 400$, $t = .50$, Monthly Accounting[b]	$g_m = 300$, $t = .75$, Monthly Accounting[a]	$g_m = 300$, $t = .50$, Monthly Accounting[b]
1	550	25	125	0	25
2	600	0	100	0	0
3	1400	0	0	0	0
4	0	300	400	300	0
5	0	300	400	300	200
6	600	0	100	0	0
7	550	25	125	0	25
8	600	0	100	0	0
9	550	25	125	0	25
10	1400	0	0	0	0
11	0	300	400	300	0
12	0	300	400	300	200
Total Annual Payments		1275	2275	1200	475
Number of Months on Welfare		7	10	4	5
Average Monthly Payment While on Welfare		182	228	300	95
Number of Spells on Welfare		5	3	2	5
Average Length of Welfare Spell		1.4	3.3	2	1

FOOTNOTES

TABLE I-1

a. The formula for determining monthly benefits, b, is:

$$b = g_m - ty_c$$

where g_m = the monthly guarantee,

t = the tax rate on income,

y_c = current monthly income.

b. The benefit formula is:

$$b = g_m - t\,(y_c + y_o)$$

where the notation is the same as in footnote a above, except that y_o = income from the carryover account up to an amount equal to the monthly breakeven level, $\frac{g_m}{t}$, minus y_c.

spouse works for one month. Columns 1 through 4 reveal its pattern of welfare experience under alternative negative income tax programs. The programs are characterized in the example only by a guarantee, g_m, a monthly benefit available if income in that month is zero; a tax rate, t, on all income, which reduces benefits as income rises; and an income accounting system, which determines both the frequency with which benefits are adjusted to income and the length of time over which incomeis "remembered" when benefits are computed.

As the monthly guarantee is raised from $300 to $400, comparing columns 1 and 2, not only would the household's monthly and annual payments rise, but its number of months "on welfare," that is, months when it received a positive payment of any amount, would increase from seven to ten months. It being harder to get off of welfare, its discrete spells on welfare decrease from five to three. The average duration of its spells on welfare would more than double, going from 1.4 to 3.3 months. These changes in the measures of its welfare experience over time assume no impact of the welfare system on the pattern in which income accrues.

A comparison of columns 1 and 3 demonstrates how changes in the benefit-loss or tax rate affect wlefare experience, when all other program characteristics and the income pattern are held constant.

Column 4 is added to indicate the impact of a change in the income accounting system, and also may be compared with column 1. In column 1, only income received in a particular month determines benefits in that month.. An alternative is to reduce benefits not only on the basis of current income, but also on the basis of income in excess of the monthly breakeven level in past months. Thus, in the third month, the family has $800 in excess of the $600 breakeven level (the income level at which benefits are zero when the guarantee is $300 and the tax rate 50%). The "excess" of $800 in the third month is ignored in calculating benefits in the future months under the monthly accounting system (in column 1). However, in an accounting system where income is "remembered" in future periods, the $800 excess will be added to actual income for a number of months in calculating benefits. The memory system in column 4 assumes that the $800 excess is used up in amounts equal to the difference between the monthly breakeven level and current monthly income. Thus, $600 of the $800 is applied in the fourth month and the remaining $200 is applied in the fifth month. Stretching out the period over which non-welfare income is remembered results in fewer months on welfare. In this case, the number of spells remains the same, but the average duration of a spell declines from 1.4 to 1 month.[4]

Program characteristics, like the ones illustrated, change periodically in AFDC. Benefit levels vary as state legislatures choose. The tax rate on earned income dropped in July 1969 when the 1967 Amendments to the Social Security Act took effect. Those provided for the "exemption" in computing benefits of the first $30 and one-third of additional monthly earnings. The AFDC accounting system changes implicitly. For example, household incomes are investigated with varying frequency in response to varying political pressures to eliminate overpayments to recipients. Examples of how administrative procedures influence welfare turnover are offered in the concluding section. Welfare patterns observed under a particular configuration of program parameters, therefore, must be interpreted with caution.[5]

B. The Welfare Experience of AFDC and AFDC-UF Families

How long do families remain on welfare once they are there? With what frequency do their cases close? Once closed, with what frequency do their cases re-open? Four studies containing descriptive data on welfare dynamics attempt to answer these questions.[6] We present their basic findings and comment on their techniques. A summary of their characteristics and findings appears in Table I-2.

Boskin and Nold used longitudinal data on 440 female-headed families in the California AFDC program. The families all went on AFDC in 1965, not necessarily for the first time, and then were observed for a sixty month period. For each month during the period, Boskin knew whether or not the families were receiving some AFDC payment. Overall, Boskin observed high turnover in this population. The mean spell on welfare was a stretch of roughly 26 months; while the median length of any one welfare spell was less than 14 months.[7] Roughly three-fourths of the 440 families

TABLE I-2

Summary of Available Information on Welfare Turnover

		STUDY				
		Boskin-Nold[a]	Rydell[b]		Saks[c]	Ketron[d]
A. Type of Data		Longitudinal	Longi-tudinal	Cross-sectional	Cross-sectional	Matched Cross-sectional
B. Welfare Jurisdictions Represented		California	City of New York		City of New York	All States
C. Time Period Covered		1965-1970	1967-1972		December 1967	May 1969 and 1971
D. Duration of One Welfare Spell	AFDC Mean	26	--	34.9	41.2	--
	AFDC Median	14	20	34	--	38
	AFDC-UF Mean	--[e]	--	38.3	27.2	--
	AFDC-UF Median	--	6	12	--	54
E. Average Monthly Percentage Leaving Welfare	AFDC	3.9	4.5	--	2.4	1.7
	AFDC-UF	--	10.6	--	3.7	1.2
F. Percentage Distribution of Cases by Number of Spells	AFDC 1	73	66	75		
	AFDC 2+	27	34	25	54	61
	AFDC-UF 1	--	47	62	46	39
	AFDC-UF 2+	--	53	38		

FOOTNOTES

TABLE I-2

a. The Boskin and Nold statistics are obtained as follows:

D. The mean is the inverse of the average monthly probability of leaving welfare in their sample. As explained in footnote 7, this is a rough approximation of the true mean duration. The median provided in the author's paper is for the total amount of time spent on welfare during the 60 month study period. Since some families were on welfare two or more times during the period, the true median is less than 14 months.

E. This was provided by the authors in correspondence, since such probabilities were presented only for sample sub-groups in their paper.

F. Boskin and Nold, p. 11.

b. The Rydell statistics are obtained as follows:

D. The authors advised us that a mean could be calculated from two sources, a statistical report published by the New York City Department of Social Services and a table in their report. From the statistical report, we derived the average number of cases receiving both AFDC and AFDC-UF in each of the six years, 1967-1972. Multiplying these averages by twelve and summing over the six years, we obtained the total number of "case-months" in each caseload. Dividing the sums by Rydell's estimates of the total number of separate cases receiving AFDC and AFDC-UF during the six years, we arrived at an estimate of the mean length of a welfare spell. The mean for AFDC seems plausible, but not that for AFDC-UF. (City of New York, Department of Social Services, Monthly New York City Public Assistance Summary, 1960-1973; and Rydell, et al, Table 2.3, p. 14. The medians from the longitudinal data are obtained from Rydell's Table 2.11, using the weights provided in Table 2.8. The medians for the cross-sectional data are from Table 2.14, using the weights in 2.12. Table 2.14 appears only in Rydell's draft report.

E. These are closing rates for cases closed within three months of their opening. Table I-3 contains these rates for subsequent periods following their opening.

F. The distribution for the longitudinal data is from Rydell, Table 2.10. The distribution for the cross-sectional data is from Table 2.9.

c. The Saks statistics are obtained as follows:

D. The means are presented in Saks, p. 125.

E. These are the reciprocals of the mean durations presented above. They are rough approximations of the true leaving rates, following the same argument offered in footnote 7 of the text.

FOOTNOTES

TABLE I-2.

F. The numbers presented here are for the State, not the City, of New York, and for AFDC-UF and AFDC combined. (The ratio of AFDC to AFDC-UF in New York City was about 12:1) Since the city caseload comprised 73 percent of the state caseload at that time, the numbers should be indicative of the distribution for the city. (U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Findings of the 1967 AFDC Study, Part I, Table 12.)

d. The Ketron, Inc. statistics are obtained as follows:

D. Ketron, Inc. did not provide medians, or data which permitted their derivation. Since they studied the national caseload, we derived these from the national survey of the AFDC population. (U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Findings of the 1969 AFDC Study, Part I, Table II.) Unfortunately, the national survey data combines AFDC and AFDC-UF cases, in the ratio of 20:1.

E. Ketron provides the probability that a case will close within one year of its opening. Assuming, contrary to fact, that the case closing rate would be constant throughout the year, we divided these annual closing rates by twelve. Ketron, Inc., p. 6 and p. 20).

F. Ketron, Inc. did not compute these percentages for AFDC units in their matched sample, which is but a part of the total survey sample. We report these for the entire, combined AFDC and AFDC-UF samples represented in the 1969 AFDC Survey (U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Findings of the 1969 AFDC Study, Part I, Table 11).

e. Dashed lines in the table mean that the authors did not provide any information on the matter, not even data from which we could derive estimates.

had but one spell on welfare, spending an average of nearly 40 percent of the five years receiving AFDC. The other fourth had two or more welfare spells, and spent an average of half the period on welfare. Only 75 of the 440 families spent the entire 60 months on welfare. Nearly 4 percent of the cases still on welfare left the rolls every month. Once off the rolls, the probability of welfare recidivism was 4.9 percent per month.[8]

Saks used cross-sectional data from the 1967 survey of the AFDC and AFDC-UF caseloads conducted by DHEW.[9] He separated New York City cases from those receiving welfare in the remainder of New York State. In this survey, caseworkers were asked to report, among other items, on the length of time that an AFDC case had been open since its most recent opening, ignoring lapses off welfare of three months or less. Saks then computed the average length of time since the most recent opening for cases in his cross-section. He found it to be over 41 months for an AFDC family in New York City. Although Saks does not measure the length of completed spells, his figures do contrast sharply with the 26 months mean in California. For an AFDC-UF case in New York City, Saks calculated a mean duration of 27 months. Whereas in California only one-fourth of the AFDC cases were recidivists, in New York City -- according to the Saks data -- nearly half were. Obviously, with the duration on welfare being longer, the monthly probability of leaving was much lower in New York.

Besides the fact that California and New York City welfare systems, local economies, and caseload composition may differ, there are peculiarities in the data that could cause the differences in the findings of Boskin and Nold and Saks. The Saks data will yield longer durations for two reasons. One is simply that the caseworkers were instructed to ignore lapses in cases of three months or less in recording the duration of a current spell. A second is more complicated, and offered initially by Rydell. Consider a span of three months, in each month of which one case lasting one month and one lasting six months is added to AFDC. At the end of the third month, four cases are on welfare, three of the six month and one of the one month variety. The inference is that if we follow opening cohorts, as Boskin and Nold did, we may get a different picture of the average time on welfare than if we look at a cross-section of cases. Long term cases are more dominant in cross-sectional than in longitudinal data on cohorts. In Rydell's data, for example, over two-fifths of cases in a given opening AFDC cohort remain on welfare for more than three years; whereas over two-thirds of cases on AFDC at a particular point in time are on welfare for more than three years.[10]

There are two factors, however, that tend to reduce somewhat the average duration in the Saks data: his cases have not completed their spell on AFDC; and, as Saks notes, the growth of the New York City caseload in the mid-sixties would make it especially likely that the caseload would contain a large number of relatively new cases. On balance, one would expect the Saks' data necessarily to yield longer durations than the Boskin data. Their results are not really comparable.

Like Saks, Ketron, Inc. used cross-sectional data from the DHEW surveys of the AFDC caseload. In this case, Ketron drew on the 1969 and 1971 surveys, attempting to determine how many families that were on welfare in 1969 were still on in 1971, nineteen months later. Two adjustments had to be made in the 1971 data to allow their comparison with the 1969 data. First, the data from the two samples were matched, so that all cases in the 1971 survey that were not on welfare 19 months before were eliminated. Secondly, 1971 AFDC cases that were AFDC-UF cases in 1969, prior to a father's departure from his family, were treated as AFDC-UF cases in 1971. Clearly, this reduces the observed turnover in AFDC-UF; it also raises calculated turnover in AFDC from what it would be in absence of the adjustment because fewer female heads are observed now in the 1971 cells. It is interesting to note the reversal in length of durations between the Saks and Ketron studies. Saks, of course, studied only New York City, whereas the Ketron study included the national caseload. Nevertheless, Ketron finds that AFDC-UF cases remain on welfare longer when their immediate transfer from AFDC-UF to AFDC is disregarded as a case closing.[11]

By far the most extensive study of welfare done to date is that done by Rydell at the New York City Rand Institute. Both cross-sectional and longitudinal data on particular cohorts were used. As noted above, the different data sources yield different results about welfare dynamics. As can be seen in Table I-2, the cross-sectional data yield longer average durations than do the data on opening cohorts; not surprisingly, then, the former also contain a larger proportion of persons who are not welfare recidivists during the 72-month study period. The average durations found by Rydell for New York are longer than those of Boskin and Nold for California, when the two sets of longitudinal data are compared. In contrast to Ketron's results for the nation, Rydell finds that AFDC-UF cases have shorter welfare spells than AFDC cases. Even after Rydell accounts for movement of AFDC-UF cases to other categories, AFDC-UF durations are shorter than those for AFDC cases.[12]

Some of the most interesting descriptive data in the Rydell study, reproduced here in Table I-3, are on case closing and case reopening rates. The top bank of Table I-3 indicates that case closing rates drop markedly with increasing case age: the longer cases remain on welfare, the less likely they are to leave welfare in any particular month. This finding, buttressed by the Ketron study of the national caseload, is important, for the Boskin-Nold and Saks studies assumed no change in case closing rates as cases aged.[13] Note also how case reopening rates fall with the passage of time: if a closed case reopens, it is likely to do so quickly. Rydell's data further indicate that, in all, roughly one-half of closed AFDC cases and three-fifths of closed AFDC-UF cases reopened within the 5-1/2 year study period. Similarly, under one-half of all opened AFDC cases and three-fifths of all opened AFDC-UF cases are cases of welfare recidivism.[14] This substantial degree of quick recidivism may be reflective of the fact that what gets families off of welfare are short run, not long run, increases in family income.

TABLE I-3

Monthly Percentage Closing and Reopening Rates[a]

Time Since Addition of Cases to Welfare Rolls	A. Monthly Closing Rates ~~of~~ Openings by Time Since Opening		
	AFDC, 2 or more children	AFDC, 1 child	AFDC-UF
3 months	4.4	4.8	10.6
6 months	3.2	4.2	7.0
1 year	2.0	3.0	4.0
1.5 - 3.0 years	1.5	1.9	1.9
3.5 - 5.0 years	2.2	1.1	0.7

Time Since Closing	B. Monthly Reopening Rate Into Former Type of Assistance		
3 months	5.1	2.8	2.2
6 months	3.6	2.3	1.5
1 year	1.4	0.9	0.6
1.5 - 3.0 years	0.7	0.4	0.3
3.5 - 5.0 years	0.3	0.2	n.a.

Time Since Closing	C. Monthly Reopening Rate Into Different Type of Assistance		
3 months	0.3	0.4	1.4
6 months	0.5	0.7	3.1
1 year	0.3	0.5	1.8
1.5 - 3.0 years	0.2	0.2	0.9
3.5 - 5.0 years	0.2	0.2	0.5

a. Source: C. Peter Rydell, Thelma Palmiero, Gerard Blais, and Dan Brown, Welfare Caseload Dynamics in New York City, R-1441-NYC, The New York City Rand Institute, October 1974, pp. 28 and 35.

In sum, high turnover does seem to characterize the AFDC and AFDC-UF programs. The measurement of the turnover, however, with a given set of program parameters, is affected markedly by the choice of longitudinal or cross-sectional data.

C. The Correlates of Welfare Turnover

To date, measures of welfare turnover have been related to measures of the personal characteristics of recipients and of the local economy. The personal characteristics are presumably related to earnings and to other factors directly affecting welfare experience. Increasing attention has been given to a third set of variables, measures of welfare programs. Our point is not that the latter have been totally ignored in all studies; it is, rather, that they have been given insufficient attention in interpreting turnover rates and their determinants. In particular, what has not been stressed is that high turnover reflects in part, at least, the parameters of the AFDC and AFDC-UF programs, not only increases in the incomes of recipient households. This section reviews findings on the correlates of turnover and places them in perspective.

Boskin and Nold focus principally on labor market variables in explaining both case closings and case openings. They develop for each female head of family in their sample an expected market wage and an expected duration of unemployment once in the labor market, from a regression of these variables on personal characteristics using data in the 1967 Survey of Economic Opportunity. Added to these measures of the opportunities afforded by the local economy are variables on personal characteristics that may affect labor market prospects, such as race, non-wage income, age, health, occupation, and presence of pre-school children.

The authors find the wage and unemployment variables, plus race and non-wage income, significant in explaining case closings. Only the wage and race variables are significant in explaining openings. In short, whites, those with higher non-wage incomes, and those with better labor market opportunities all have better prospects of leaving welfare. Once off welfare, non-whites and those whose expected wage is below the legal minimum are more likely to return to welfare. Other personal characteristics are not significant in explaining case closings or openings. Since duration on welfare is inversely related to case closing rates, or the probability of leaving welfare, the average durations for persons of different characteristics can be inferred from the above. For example, women whose expected wage is below the minimum will average roughly 75 percent more time on welfare per spell than women whose wage is above the minimum, their other charcteristics held constant.

On theoretical and statistical grounds, the Boskin and Nold paper are open to question. Levy has noted that a model of welfare turnover for female-headed families that relies too heavily on labor market factors is ill-founded in view of the small proportion of such cases closed for reasons related to employment. In the first quarter of 1973, for example, less than 7 percent -- probably less than 5 percent -- of AFDC cases in California closed because of employment.[15] Yet Boskin and Nold ascribe the increase in average durations on AFDC from roughly 22 to 37 months to the difference in expected wages faced by AFDC mothers.[16] In effect, Boskin and Nold related welfare experience to all the exogenous variables of their system. Some of these exogenous variables may affect welfare directly, but many of them affect it only through the channel of earnings. It is tempting to wonder whether spelling out the intermediate steps might have given a clearer picture of structural relationships. In any case, while we accept the fact that welfare turnover, especially case openings in AFDC-UF, is related to employment factors, a more complete model would consider non-labor market factors as well.

Using no particular theoretical structure, the Ketron study estimates the relationship between case closing rates and the race, age, education, and the number of children of AFDC mothers. Additionally, the length of time a case has been open is included as an independent variable. The probability of a case closing falls (or the duration on welfare rises) if an AFDC mother is non-white, has a large number of children and is older. It also falls dramatically with the "age" of the case.[17] For example, a family headed by a white mother under 26 with one child has a .35 chance of leaving AFDC in the next twelve months, if her case has been open for less than one year; whereas the same type of family which already has been on AFDC for more than two years faces but a .09 chance of leaving within the next year. This 75 percent decline in the probability of leaving AFDC as the case ages induces Levy to speculate that the

> . . .welfare system contains several "tracks: a number of families come on the rolls at a particular time. For some, the rolls are acting as a backstop through a difficult period and they leave the rolls as soon as circumstances change. For others, welfare becomes a long-term phenomenon.[18]

This point stands in contrast to earlier emphasis simply on high turnover in AFDC.

Saks also recognizes that the decision to leave AFDC is explained by changes in earnings in only a minority of instances. Migration and technical disqualifications, for example, account for a sizable fraction of case closings. Still, though, indicators of employability are statistically significant in explaining the duration of welfare spells. Saks finds, for example, that the expected wage of both AFDC and AFDC-UF family heads is negatively related to the duration of a welfare spell. Among the personal variables, age and disability are positively correlated with duration. Saks notes that the correlation between age and duration may indicate that older persons may simply have had more time to be on welfare.

Lastly, Saks discusses two types of effects of welfare program variables: they may influence recipient behavior, and thereby affect eligibility; or they may affect mechanically the conditions under which recipients are eligible, even if they have no influence on behavior. Thus, Saks asserts that a fall in the tax rate will have two different results: it will induce more labor but it also raises the welfare breakeven level so it will increase expected duration of cases.[19] He does not distinguish the two types of effects in interpreting his statistical work.

In disucssing the positive correlation that he finds between the guarantee level and expected duration, he refers only to behavioral effects: lower guarantees "push" family heads, especially males, to search more assiduously for non-welfare income alternatives.[20] We shall return shortly to Saks' overall discussion of the relationship between welfare program characteristics and welfare participation.

Limited analysis of the data emanating from the Michigan Panel Study of Income Dynamics has been done at Michigan.[21] Dickinson combined all low income families and then estimated the correlates of the probability of any low income family going on any cash public assistance program during the five year study period; and the correlates of the probability of a family leaving welfare after having been on. The most important correlates of going on are the departure of a male head from his family and the age of the children in a family. Families on welfare whose heads change from male to female or remain female are much less likely than male-headed families to leave welfare. Interestingly, in a study which considered family structure, family composition, not labor market, variables were critical in getting on and off. Lastly, holding constant a large array of factors, families in the Northeast were most likely to get on and least likely to leave welfare. What distinguishes the Northeast from other regions must be their more generous and liberally administered welfare programs.

Consistent with the results of other studies already reported, the Rydell study finds that variables related to the prospect of employment affected turnover. Case closing rates in AFDC are negatively correlated with the age of the mother and the size of her family, as well as positively correlated with indicators of her employability. For example, controlling for other factors, the probability that a recently opened AFDC case, where the mother had one child, would close within its first year on welfare was .030 in any given month; where the mother had two or more children, that probability dropped by a third to .019. Similarly, the probability that a case headed by an "unemployable" mother would close was roughly one-third less than the probability that the case of an employable mother would be terminated.[22] (Employability here resulted from caseworker judgment.)

Another interesting outcome of this study is the development of predictive models for the number of case openings and the number of case closings in each welfare category, including AFDC and AFDC-UF. Monthly openings in an assistance category are regressed, in a log linear regression, against six variables: the average number of monthly openings in the previous twelve months, the number of caseworker workdays in a month, the welfare department's acceptance rate for new applications, the deflated welfare guarantee, the local unemployment rate, and the number of recent "general service" births. Monthly closings are regressed against previous closings, caseworker workdays, the welfare guarantee, and the unemployment rate.

Rydell notes that changes in guarantees and acceptance rates by the welfare department could have altered openings and closings by having both behavioral and mechanical effects: welfare progessively was made relatively more attractive, thereby possibly inducing people to forego work and choose welfare; and increasing numbers of people were made eligible even if they in no way altered their behavior. According to Rydell's findings, the acceptance rate and the welfare guarantee usually did have powerful effects on openings. Using results from regressions which have values of R^2 of .87 and .48, respectively, Rydell reports that in both AFDC and AFDC-UF, a 1 percent increase in the acceptance rate increased the number of openings by an identical 1.57 percent.[23] Thus, to estimate the effects of a 5 percent change, an increase in the AFDC and AFDC-UF acceptance rates from 700 to 735 would increase new openings in AFDC from roughly 4500 to 4853; and from roughly 250 to 270 in AFDC-UF. In AFDC, the impact on openings of a change in the guarantee level was statistically significant only at a low level and, in any case, was fairly small: the elasticity of openings with respect to the guarantee was .3. In AFDC-UF, a 1 percent increase in the guarantee had an effect six times as large, 1.84 percent, and was statistically significant. An increase in the average AFDC-UF guarantee in 1971 from \$300 to \$315 would have increased monthly openings from roughly 250 to 273. Were the .3 figure significant at a higher level, raising the average AFDC guarantee from \$250 to \$262.50 would increase monthly openings from 4500 to 4568. It should also be noted that besides affecting openings directly, Rydell found that an increase in the acceptance rate increases the number of AFDC and AFDC-UF applications. This, in turn, increases openings.

The predictive model for the number of case closings yields the conclusion that, as Rydell hypothesized, case closings are negatively related to welfare guarantees and the local unemployment rate. A 1 percent increase in the AFDC and AFDC-UF guarantees over the period 1963-1971 led to a .45 percent and a .38 percent decline in the respective number of case closings. These results come from regressions which have values for R^2 of .58 and .57, respectively.[24] Thus, for example, a 5 percent decline in AFDC guarantees in 1971, from \$250 to \$237.50, would reduce closings from roughly 225 to 214 per month.

While it appears, then, that legislative and administrative changes in welfare parameters have affected the probability of both going on and getting off welfare, the other interesting results from the Rydell equations are the significant coefficient on a measure of <u>recent</u> unemployment -- and the statistically insignificant effect of <u>lagged</u> unemployment.[25] These suggest that welfare experience is related to events in the labor market; and that potential recipients are in jobs which are unprotected by unemployment insurance. Having limited assets, unemployment results quickly in a move to welfare.

Data on their independent variables are of some separate interest. The welfare department's acceptances in AFDC rose from 523 per 1000 applications in 1963 to 772 in 1968, as labor markets and incomes generally improved; and then declined to 698 per 1000 in 1971, as times worsened. The mean number per 1000 applications over the nine years was 662. Similarly, in AFDC-UF the comparable figures are 571, 766, and 666, with a mean of 665.[26] As just suggested, we will expand below on how these fluctuating acceptance rates probably were a consequence, not of a changing mix of applicants, but of deliberate administrative policy to control growth of welfare in New York City. Monthly welfare guarantees also varied over the nine year period. Average monthly guarantees in constant dollars, among families of all sizes, rose from $187 in 1963 to $267 in 1970, and fell to $225 in 1971; the mean for nine years was $224. In AFDC-UF, the comparable figures are $240, 325, and 307, with a mean of $279.[27] A likely hypothesis is that most of the variation over time comes not from a changing mix of families, but from legislated changes in benefit levels.[28]

D. The Administrative Factor in Welfare Turnover

Employment-related factors, although important with respect to AFDC-UF turnover, do not account for the majority of AFDC case openings or closings. Welfare departments keep detailed records on openings and closings. Quint and Brown study the reasons for these in 1972 in New York City. Their conclusion is that " administrative actions rather than events related to client need prompt a good deal of case turnover. . ."

Again, the quantity of turnover in their study is very high. Among all cases receiving AFDC in 1972 in New York, 28 percent experienced a case opening and/or a case closing within that year; for AFDC-UF, the figure was 48 percent. Quint and Brown note that the large amount of openings and closings in that year is in part a function of the policy of zero-caseload growth instituted by Mayor Lindsay. To implement this policy, steps were taken to reduce openings and increase closings. Openings were reduced, for example, as investigations of income at the time of application were made more extensive. Closings were increased, for example, with the tougher enforcement of the work test.

The data on reasons for case openings and closings in Table I-4, suggest a number of inferences. One is that although employment accounts for most case openings

TABLE I-4

Percentage Distribution of Case Openings By Reason for New York City in 1972[a]

Reasons for Case Openings

	A. All Cases		B. Administrative Churning Cases Excluded	
	AFDC	AFDC-UF	AFDC	AFDC-UF
NEED RELATED				
Unemployment	13.3	67.0	15.0	73.1
Income Loss	3.3	8.4	3.7	9.1
Medical	17.5	2.9	19.8	3.1
Household Change	45.1	7.1	51.1	7.8
Other Need Related	8.0	5.5	9.1	6.1
Total	(87.2)	(91.0)	(98.7)	(99.3)
NON-NEED RELATED				
Closed in Error	7.6	6.6	--	--
Administrative	5.2	2.3	--	--
Unexplained Illegal	0.0	0.1	--	--
Total	(12.8)	(9.0)	(1.3)	(0.7)
TOTAL	100.0	100.0	100.0	100.0

Reasons for Case Closings

	A. All Cases		B. Administrative Churning Cases Excluded	
	AFDC	AFDC-UF	AFDC	AFDC-UF
NEED RELATED				
Employment	10.8	29.9	12.2	31.9
Income Increase	3.1	3.6	3.5	3.9
Death	0.2	0.1	0.2	0.1
Household Change	7.8	0.7	8.8	0.7
Unspecified Need	20.5	17.4	23.3	18.5
Total	(42.3)	(51.7)	(47.9)	(55.1)
NON-NEED RELATED				
Lost Contact	31.4	18.5	33.0	18.4
Administrative	25.5	28.7	18.5	25.7
Unexplained Illegal	0.8	1.1	0.6	0.8
Total	(57.7)	(48.3)	(52.1)	(44.9)
TOTAL	100.0	100.0	100.0	100.0

a. Source: Janet Quint and Dan Brown, Welfare Case Turnover in 1972, City of New York, Human Resources Administration, December 1, 1973, Tables II-4, II-6, IV-1, IV-3.

in AFDC-UF, it explains but a small fraction of AFDC openings. Changes in household composition, no doubt the departure of the male head, is the modal opening reason in AFDC. A second inference is that reasons not related to need, while relatively unimportant in relation to openings, account for a majority of case openings in both caseloads. Non-need factors, including "administrative" ones, dominate case closings. It is possible, of course, that closings attributable to "lost contact" in part may be related to changes in need.

"Administrative reasons" for case turnover include changes in welfare regulations on, among other matters, income documentation and work registration. Some of these administrative actions have a very transitory effect. Indeed, many closings not related to need reopen within two or three months of being closed. Quint and Brown have labeled certain of these pairs of closings and reopenings "administrative churning." Reopenings within 30 days of closing whose reason for reopening was "administrative;" and reopenings which result from cases being "closed in error," together form a conservative definition of administrative churning. They accounted for 12 percent of all openings and nine percent of all closings in New York in 1972. Managment reforms, then, induced by political pressure, substantially reduced the length of spells on welfare. Although, looking at the right side of Table I-4, the exclusion of administrative churning cases raises the proportion of AFDC and AFDC-UF openings and closings that are related to need, non-need factors still weigh heavily in closings. It should be noted that the earlier estimates of welfare spells and turnover provided by the Rydell study were arrived at <u>after</u> cases experiencing administrative churning were excluded from their data base. As Table I-4 shows, however, along with employment and family changes, non-need related factors remain as a strong influence in determining turnover.

E. Welfare Turnover and Program Structure: Some General Observations

That the characteristics of welfare programs affect participation in them is obvious -- and certainly not a startling discovery. Virtually any economist using cross-sectional data in the context of the standard work-leisure model will recognize that participation will vary among welfare jurisdictions with guarantees and tax rates. In contrast to social workers, it took economists some time, however, to recognize the importance of the non-economic characteristics of welfare programs in determining participation. Albin and Stein wrote in 1968 of how welfare authorities can constrain the choices of potential users of welfare by imposing a work requirement and can lower the value of assistance by making its receipt distasteful. Thus, they recognized the administrative factor in setting the number of recipients at a moment in time.[29] More recently, Daniel Saks has written an excellent dissertation in which he characterizes welfare departments as discriminating monopsonists, setting guarantees at levels where they will be unable to finance all eligible applicants; and, consequently, having to resort to their own arbitrary criteria in selecting successful applicants.[30] Non-economists, led perhaps by Frances Fox Piven and Richard A. Cloward, have long been outspoken about how caseloads are deliberately manipulated and controlled by administrators acting under political pressure.[31]

Another part of Saks' work was cited above as an application of the point that the economic parameters of welfare programs affect participation over time. The study by Rydell and his colleagues was cited for incorporating the idea that the "administrative factors" in welfare programs have an important influence on turnover. We have tried to stress these examples and use them to offer a perspective on welfare turnover that has been absent from the literature. If turnover is so much a function of program characteristics, economic and administrative, then any particular findings on turnover should be interpreted as having applicability limited to the specific program studied.

In 1972, the New York Department of Social Services undoubtedly altered welfare patterns when it implemented its policy of zero-caseload-growth. The techniques used were many.[32] To reduce openings, extensive documentation of income was demanded; reversing a previous departmental assumption -- when an old regulation was "discovered" -- that applicants were needy until proven otherwise, recipients were kept off AFDC until this extensive documentation was completed; and, interestingly, older women deliberately were substituted for young men and women as caseworkers in determining the eligibility of applicants. Within one year, the acceptance rate in AFDC dropped by one-fourth.[33] To increase closings, face-to-face recertification of cases was instituted; absent fathers were searched for by the state tax agency; fraud cases were publicized heavily. Case closings, both voluntary on the part of clients and initiated by the welfare department, rose markedly as a result of those adjustments in administration.[34] Not only the lasting, but even the temporary overall effects of these policies are uncertain, however, as some have argued that AFDC cases often were shifted to the "Home Relief" and Supplemental Security Income programs after being denied AFDC. Undoubtedly, though, AFDC and AFDC-UF welfare patterns were influenced. In view of what has been said, we question the significance of the finding of high turnover that appears in some of the literature. Moreover, what would happen to welfare turnover were we, to use Saks' terms, to rationalize welfare programs and use the guarantee and tax rate, instead of administrative discretion, as the control variables in determining eligibility?

Evidence on turnover in the low income rather than the welfare population is presented by the data, drawn from the Panel Study on Income Dynamics, in Tables I-5 and I-6. An examination of it will provide an understanding of why low income families move in and out of welfare ~~dependency~~ over long stretches of time. Tables I-5 and I-6 show the changes over a five year period in the annual non-welfare incomes of families which were in the bottom fifth of the income distribution in any year between 1967 and 1971 and whose annual incomes were ~~observed for~~ each of those five years, even if there was an addition or subtraction from the 1967 family unit.[35] Table I-5 shows that among families in the bottom fifth in 1967, 88 percent had annual incomes that were less than 150 percent of their respective poverty lines. The data in Table I-6 indicate the proportion of the 1968-71 period that families in particular

TABLE I-5

Percentage Distribution of Families With 1971 Ratios of Non-Welfare Income to 1971 Poverty Line By 1967 Ratio [a]

Ratio of 1967 Family Non-Welfare Income To 1967 Family Poverty Line	Ratio of 1971 Family Non-Welfare Income to 1971 Family Poverty Line							Number in Row
	0-.25	.26-.50	.51-75	.76-1.00	1.01-1.25	1.26-1.50	1.50+	
0-.25	50.0	17.4	12.8	5.0	5.3	3.2	6.4	282
.26-.50	16.6	22.1	19.8	19.0	8.3	4.0	10.3	253
.51-.75	10.1	9.7	14.8	24.1	12.5	10.5	18.3	257
.76-1.00	3.8	6.3	12.2	14.6	16.4	11.8	34.8	287
1.01-1.25	4.5	3.6	9.9	18.4	20.2	14.8	28.7	223
1.26-1.50	4.2	2.5	12.5	18.3	21.7	16.7	19.1	120
1.51+	5.7	6.7	11.9	17.6	17.6	9.8	31.6	193
Number in Column	246	172	219	263	220	152	343	1615

a. Source: Panel Study on Income Dynamics data tape. For this tabulation, we selected families whose family income was obtained for each of the five study years; whose income, in any of the five years, was in the botton fifth of the income distribution; and whose head was not over 60 in the first year of the study. A description of the data source appears in Chapter III.

TABLE I-6

Proportion of Time Spent in Income/Poverty Line Strata in Period 1968-1971, Given 1967 Income/Poverty Line Strata[a,b]

Ratio of 1967 Family Non-Welfare Income to 1967 Family Poverty Line	Ratio of 1971 Family Non-Welfare Income to 1971 Family Poverty Line							Number in Row
	0-.25	.26-.50	.51-.75	.76-1.00	1.01-1.25	1.26-1.50	1.51+	
0-.25	56.8	17.4	9.8	4.7	4.0	2.5	4.8	282
.26-.50	16.6	28.9	21.6	12.8	6.8	4.4	8.6	254
.51-.75	7.3	13.3	22.1	21.7	14.7	8.5	12.3	257
.76-1.00	3.1	5.7	12.5	21.6	16.8	13.1	29.1	287
1.01-1.25	3.2	3.7	9.6	20.6	25.8	13.6	23.4	224
1.26-1.50	2.9	3.7	8.7	17.5	26.2	18.1	22.7	120
1.51+	2.9	2.8	9.4	12.6	16.5	14.2	41.1	194
TOTAL								1618

a. Source: Panel Study on Income Dynamics data tape. For this tabulation, we selected families whose family income was obtained for each of the five study years; whose income, in any of the five years, was in the bottom fifth of the income distribution; and whose head was not over 60 in the first year of the study.

b. This table was suggested to us by Table 2 in the paper by Levy, et al.

income/poverty line strata in 1967 spent in the same or neighboring strata. For example, families whose 1967 welfare incomes were less one-fourth of their 1967 poverty lines spent 56.8 percent of the next four years in the same position; and spent all but 4.8 percent of the next four years below 150 percent of their poverty lines. From Table I-6, we can infer that in 83.9 percent of the "family years" following 1967,[36] the families below 150 percent of their poverty lines in 1967 remained in that position.

We may consider the implications of these data in the context both of a universal negative income tax program and the current welfare system. Assume that one existed in 1967 which had guarantees equal to poverty lines and whose benefit-loss or tax rate was 67 percent. Assume also that guarantees were changed each year for increases in the cost of living. Such a program would have disbursed benefits to all families in any year whose annual incomes were below 150 percent of their poverty lines. The data in Tables I-5 and I-6 show that, among families receiving payments in 1967, there would be continuing eligibility for payments in 83.9 percent of the subsequent "family years."[37] These figures suggest that were we to look at payments per year based on annual income,[38] within the context of a somewhat generous universal negative income tax program,[39] welfare turnover would be reduced dramatically -- because the movement out of poverty is limited in scope.

F. Conclusion

Having analyzed the literature on the nature and determinants of welfare experience in the AFDC and AFDC-UF programs, we may conclude the following:

1. Turnover in the welfare population is high. Most families going on welfare leave the program within a few years. While there is substantial movement from welfare to non-welfare status, the latter often being attained as a consequence of re-employment, there also is substantial welfare recidivism. A study covering a 5-1/2 year period in New York City showed that within that time span roughly one-half of closed AFDC cases and three-fifths of closed AFDC-UF cases re-opened. Thus, while there is much short-term

success in removing families from dependency, long-term success is much less likely.

2. Short-term success largely has been measured by the number of months families spend on welfare. Besides examining the length of welfare spells, one could examine changes in welfare payments over time, reductions in the latter being another indicator of success. (This is done for the first time in this report.) Estimates of the average duration of welfare spells in previous studies vary according to whether cross-sectional or longitudinal data are used and among different welfare subpopulations. Cross-sectional data necessarily contain more long-term welfare cases than do longitudinal data following particular cohorts of welfare families because of the accumulation over time of the long-term cases. Spells on welfare usually are somewhat shorter in AFDC-UF than in AFDC. Estimates of average spells on AFDC from longitudinal data fall under 2 years; from cross-sectional data, estimates of average spells on AFDC are over three years. As noted, estimated average duration on the AFDC-UF program is shorter. Substantial variation in average spells from a limited number of studies may reflect the fact that the studies are done in different states or perhaps should lessen confidence in any particular estimate.

3. As might be expected, variations in length of spells on welfare are associated with differences in family structure and labor market experience. Male-headed families and families with a head who has a good chance of becoming employed are more likely than female-headed families and families with heads of limited employability to leave welfare. Also of critical importance in determining welfare experience over time, however, is program structure. Families of given structure and with given labor market prospects are more likely to remain on welfare the more generous is the welfare program they face. Generosity may take the form of high guarantees, low tax rates, or lenient administration.

While re-employment, the return of an absent male head, or toughened administration may result in short-term success, i.e., the removal of families from welfare dependency, such success often will be temporary. Over the long-term, changes in annual family incomes are rather small for most units in the low income population. Most low income families therefore, remain at risk for long periods. Unemployment of their heads or other small changes in their circumstances frequently will result in their return to welfare. The importance of program structure in determining welfare experience and the long-term nature of most poverty should place in perspective the frequently emphasized phenomenon of high welfare turnover.

CHAPTER I

FOOTNOTES

1. Michael J. Boskin and Frederick C. Nold, " A Markov Model of Turnover in Aid to Families with Dependent Children," Institute for Mathematical Studies in the Social Sciences, Stanford University, Stanford, California, Technical Report No. 125, March 1974, p. 1.

2. C. Peter Rydell, Thelma Palmiero, Gerard Blais, and Dan Brown, Dynamics in New York City, R-1441-NYC, The New York City Rand Institute, October 1974, p. 1.

3. Ketron, Inc., "Estimates of Annual Natural Turnover Rates From 1969 and 1971 AFDC National Survey," Wayne, Pennsylvania, August 23, 1973, p. 3.

4. For a more detailed explanation of income accounting systems in welfare programs see: Jodie T. Allen, "Designing Income Maintenance Systems: The Income Accounting Problem," in U.S. Congress, Joint Economic Committee, Subcommittee on Fiscal Policy, Studies in Public Welfare, Paper No. 5 (Part 3), March 12, 1973, pp. 47-49.

5. A graduate student of ours, Barry Sun, is simulating welfare patterns that result from variations in welfare program characteristics in his doctoral dissertation, currently in progress under this research grant.

6. One study of welfare turnover, covering part of the period observed in the Rydell study, is discussed in the next section of this chapter. It focuses mainly on the administratively provided reasons for case openings and closings. The Rydell study actually characterizes turnover in New York City over the long run. (Janet Quint and Dan Brown, Welfare Case Turnover in 1972, City of New York, Human Resources Administration, Office of Policy Research, Document No. 8857632-12, December 1, 1973). Another study of welfare turnover, using longitudinal data on the Alameda County, California AFDC and AFDC-UF caseloads, currently is being conducted by Frank Levy, Clair Vickery, and Michael Wiseman, University of California-Berkeley, Department of Economics.

7. Boskin and Nold do not provide the mean and the median. We inferred that the mean spell on welfare was roughly 26 months from the fact that the average monthly probability of leaving welfare was 3.9 percent. The expected duration of a spell on welfare is roughly equivalent to the reciprocal of this probability. We say roughly because, in general, the inverse of a mean is not equal to the mean of the inverse of the numbers being averaged. Boskin provides the median amount of time spent on welfare during the 60 month study period; it was 14 months. Since some families went on and off welfare more than once, the median length of a single spell had to be less than 14 months. (Boskin and Nold, p. 11)

8. This number was obtained from correspondence with the authors.

9. Daniel Holtzman Saks, "Economic Analysis of an Urban Public Assistance Program: Aid to New York City Families With Dependent Children in the Sixties," doctoral dissertation, Princeton University, February 1973, chapter V.

CHAPTER I

FOOTNOTES - CONTINUED

10. Rydell, et al, pp. 20-21.

11. Ketron, Inc., p. 18.

12. Rydell, et al, p. 22.

13 Saks tested his data to see whether case closing rates changed with the amount of time already spent on welfare. His test did not allow him to reject the hypothesis that there was no change with time. (Saks, pp. 134-145.)

14. Rydell, et al, pp. 36 and 43.

15. Available data on the reasons for case closings do not distinguish the AFDC-UF from the AFDC caseloads. Thus, the estimate of 5 percent is a best guess made from a variety of available sources. (See: Department of Health, Education, and Welfare, National Center for Social Statistics, "Applications and Case Dispositions for Public Assistance, " January-March 1973, NCSS Report A-12, Table 15).

16. We say "roughly" because the authors do not provide information on the proportion of persons in each of their categories for which they report expected durations on AFDC. These proportions would allow the calculation of weighted average durations. Our averages are made very casually from their tables.

17. Ketron, Inc., pp. 10 ff.

18. Frank Levy, Clair Vickery, Michael Wiseman, "Income Dynamics of the Poor," University of California-Berkeley, December 13, 1973, p. 34.

19. Saks, p. 122.

20. Ibid., p. 128.

21. Katherine Dickinson, "Transfer Income," in James N. Morgan, et al, Five Thousand American Families - Patterns of Economic Progress, Vol. 1, chapter 5, pp. 263-69.

22. Rydell, et al, pp. 52-57.

23. Ibid., pp. 92-94.

24. Ibid., p. 98.

25. Ibid., pp. 170-1.

26. Ibid., p. 79.

27. Ibid., p. 86.

28. Ibid., p. 82.

29. Peter S. Albin and Bruno Stein, "The Contrained Demand for Public Assistance," Journal of Human Resources, Vol. 3, No. 3, Summer 1968, pp. 300-11, chapter 3.

CHAPTER I

FOOTNOTES - CONTINUED

30. Saks, chapter 3.

31. Frances Fox Piven and Richard A. Cloward, Regulating the Poor: The Functions of Public Welfare (New York: Random, 1971).

32. These were discussed in a personal interview between Martin Burdick, of the New York City Department of Social Services, and Leonard J. Hausman in April 1974.

33. Quint and Brown, p. 18.

34. Mr. Berlinger, former Welfare Inspector General for the State of New York, comments that as a result of the recent imprisonment of a welfare recipient for fraud in Albany, New York, the Albany welfare department was flooded with client requests to close their cases. (Personal interview with Leonard J. Hausman, April 1974).

35. Table 5 is patterned after Table A1.6 in the previously cited Morgan study. Table I-6 is patterned after Table 2 in the paper by Levy, *et al.*

36. The 83.9 percent figure is a weighted average of the first six numbers in the column headed "1.50+" in Table I-6.

37. Among those receiving some payment in 1967, 89.1 percent in 1968, 84.4 percent in 1969, and 84.0 percent in 1970 would have been eligible for a new NIT benefit. Of course, some persons not eligible in 1967 also would have received some benefits in the years 1968-1971.

38. Note that we are talking about payments per year based on income per year. Existing studies of turnover really look at payments per month based on income per month and net assets from prior income.

39. Such a program is not entirely out of the realm of possibility. Perhaps five states have AFDC programs that have comparable guarantees and tax rates. A welfare reform proposal, submitted by DHEW to the White House in the fall of 1974, had a 50 percent tax rate and only a slightly less generous guarantee.

CHAPTER II

Employment And Earnings Among Heads of Welfare Families: A Review of the Literature

Researchers have joined welfare administrators in proclaiming -- sometimes extolling -- the high degree of turnover in AFDC and AFDC-UF. The fashion in characterizing employment patterns among female heads of AFDC families had been to declare their unemployability. Readers may recall President Johnson's chief domestic adviser announcing that 1 percent of all AFDC recipients were employable. Unfortunately, he included all AFDC children in his base. The previous chapter attempted to challenge the first fashion. Recent analyses have compelled a change in the latter. In the past six or seven years, both researchers and administrators have begun to speak of how "work and welfare go together."[1]

The change was induced by the study of longitudinal data. Cross-sectional data on the labor market activities of low-income women reveals limited work effort. This is not the case when their behavior is monitored over time. Over time, large fractions of the heads of low-income families move in and out of jobs. While researchers and administrators have learned that "work and welfare go together," the implications of the findings from the longitudinal data on the serial mixing of the two have not been generally recognized. In particular, if low-income persons generally make regular transitions between employment and non-employment, then programs that promote the movement from welfare to work generally will affect the timing of transitions that otherwise would have taken place. Short-term success for this type of program involves a reduction in the length of time recipients spend out of work. If low-income persons have a high probability of re-entering unemployment, short and long-term success must be distinguished, the latter implying a sharp reduction in the probability of becoming unemployed once a person is working.

This chapter starts with a review of the literature on the work effort, at a point in time and over time, of female heads of families receiving welfare. Attention then is turned to work effort among male heads of poor families, only a fourth of which receive welfare at a moment in time. The second part of this chapter reviews the literature on how welfare programs have affected the labor market behavior of the heads of low income families. Our review is necessarily brief, for it is a review of the reviews. The literature on studies using non-experimental data has been surveyed several times, and very well, by others. We turn then to review of the literature arising from the Wisconsin data on the negative income tax experiment, attempting therein to set the stage for our analysis of its data.

A. Descriptive Data on Labor Market Behavior

1. Female Heads of Families

The AFDC program was started to allow female heads of households to be full-time mothers. Like married women in low and moderate income families, however, AFDC mothers apparently are compelled by their financial condition to enter the labor market.

The labor force participation of female heads of AFDC families is extensive but intermittent.[2] Table II-1 shows that at a point in time only a small fraction, roughly 15 percent, of AFDC mothers work. Moreover, their employment rate over time is quite stable, rising from 15.6 percent in 1961 to only 16.1 percent in 1973. Coincident with the decrease in the AFDC tax rate on earnings in 1968-69, there has been a noticeable shift in the mix of employment towards full-time work, and a recent rise in their labor force participation rate.

What the low employment rates fail to reveal is the turnover in employment among AFDC mothers. Beginning with Table II-2, we note that over half of the employed mothers have held their current jobs for but a year or less. In the general labor

TABLE II-1

Employment Status of AFDC Parents

	1961	1967	1969	1971	1973
Employed Mothers, Total[a,b]	15.6	14.9	14.5	15.0	16.1
Full-time	5.5	7.2	8.2	9.0	9.8
Part-time	10.1	7.7	6.3	6.0	6.3
Unemployed	---[d]	6.9	5.8	5.7	11.5
Not in Labor Force	---[d]	78.2	79.7	79.3	72.4
TOTAL	100.0	100.0	100.0	100.0	100.0
Employed Fathers, Total[c]	---[d]	---[d]	---[d]	---[d]	11.7
Full-time					5.3
Part-time					6,4
Unemployed					27.8
Not in Labor Force					60.5
TOTAL					100.0

a. Sources: U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Study of Recipients of Aid to Families With Dependent Children, November-December 1961: National Cross Tabulations, August 1965, Table 18; ________, Findings of the 1967 AFDC Study, July 1970, Part I, Table 38: ________, Findings of the 1969 AFDC Study, Part I, December 1970, Table 19; ____; Findings of the 1971 AFDC Study, December 1971, Part I, Table 21; ________, Findings of the 1973 AFDC Study, Part I, June 1974, Table 33 and 44.

b. These distributions are for AFDC mothers who are living with their families. In 1973, 6.6 percent of AFDC mothers did not reside with their families. Also note that in 8.3 percent of AFDC cases, an incapacitated father, not mother, was the family head.

c. At any one point in time in 1973, among roughly 3 million AFDC families just under .4 million had natural or adoptive fathers at home. Of these men, 75 percent received AFDC, while the rest were receiving other types of public assistance.

d. These data were not published by the National Center for Social Statistics.

TABLE II-2

Employment and Unemployment Experience of AFDC Parents[a]

	Mothers			Fathers		
	Number	Percent	Percent	Number	Percent	Percent
Total, AFDC Parents in Home (with known employment status)	2,793,547	100.0		379,048	100.0	
Currently Employed	449,746	16.1		44,241	11.7	
Currently Employed (with known job length:)	381,879		100.0	31,103		100.0
1-12 months	220,334		57.7	19,558		62.9
13-24 months	63,793		16.7	5,119		46.5
25+ months	97,752		25.6	6,435		20.6
Never employed	615,840	22.0		3,819	1.0	
Unknown Whether Ever Employed	503,891	18.0		16,987	4.5	
Previously Employed Currently Not	1,224,070	43.8		314,001	82.8	
Previously Employed Currently Not (with known months since last job:)	1,038,247		100.0	270,662		100.0
1-12 months	291,921		28.1	95,904		35.4
13-24 months	173,279		16.7	52,859		19.5
25+ months	573,047		55.2	121,897		45.0

a. Source: U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, <u>Findings of the 1973 AFDC Study</u>, Part I, June 1974, Tables 34, 35, 45, and 46. Since the numbers of unknowns in the tables that we used were inconsistent, arbitrary judgments had to be made to arrive at the estimates presented here.

force, the average duration of a job is well over two years.[3] Among the non-employed mothers, the same table shows that over one-fourth have been separated from their jobs for less than one year, with another one-fifth having had a job within the past one or two years.

A better indication of the extensive but intermittent involvement of AFDC mothers in the job market comes through in Tables II-3 and II-4. The former table is from a study conducted by DHEW in ten states, in which over 11,000 low income families had their welfare and employment experience traced over a period of 37 months. Half of the sample was composed of active AFDC cases, one-fourth of closed AFDC cases, and one-fourth of families whose application for AFDC were rejected; female-headed families comprised 85 percent of the sample. Over the three year period, three-fifths of the family heads worked at one time or another; and 35 percent worked for one-third or more of the period. Robert Williams eliminated from this sample of 11,000 families those cases which were either closed, rejected, or active where the male head was present, thereby reducing the sample to 5,863 active female-headed AFDC cases. Williams found that mothers in over half of the latter cases worked at some point during the three years and, again, that about a third worked for a year or more during the 37 months.[4]

The Census data in Table II-4 are consistent with the findings of the previous study. There we see that roughly two-fifths of all women who headed low-income families worked during the one year period; almost all female-headed low income units also were AFDC families.[5] Only a small fraction, 6.9 percent, of all female heads of poor families worked full-time all year. A third data source yields more corroborating evidence: A special study of six scattered cities estimated, very conservatively, that half of all AFDC families had earnings at some time during the year.[6] Since AFDC families with incomes above poverty lines are very likely to contain working mothers, a fair generalization is that roughly half of female-headed AFDC families have some earnings from a head, although these largely accrue for only part of the year.

TABLE III-3[a]

Number of Months of Employment, By Number of Months on Welfare.
(In Percentages)

Months on Welfare	Months Employed: None	1-4	5-8	9-12	13-16	17-20	21-24	25-30	31-36	37	No Answer
None	27.4	27.8	25.5	32.0	29.3	31.2	26.7	33.3	36.6	67.1	100.0
1-12	21.3	25.6	19.6	23.6	20.4	22.3	27.1	42.6	42.0	13.2	-
13-24	14.3	15.8	14.3	19.3	35.4	29.9	31.9	9.2	7.3	6.9	-
25-36	16.4	24.6	36.7	19.0	8.2	10.0	6.2	7.8	4.9	3.7	-
37 months	20.4	6.1	3.8	5.9	6.7	6.6	8.0	6.8	9.1	9.1	-
No. inf.	0.2	0.1	0.2	0.2	-	-	-	0.3	-	0.1	-
TOTAL	100.0 (1794)	100.0 (1223)	100.1 (1118)	100.0 (622)	100.0 (624)	100.0 (458)	99.9 (501)	100.0 (739)	99.9 (790)	100.1 (3731)	100.0 (2)

a. Source: U.S. Department of Health, Education, and Welfare, Welfare Policy and Its Consequences for the Recipient Population: A Study of the AFDC Program, by Samuel M. Meyers and Jennie McIntyre, 1969, Table 5.3.

TABLE II-4

Work Experience of Heads of Low Income Families in 1972[a]

	TOTAL FAMILIES		MALE HEADS		FEMALE HEADS	
	Number	Percent of total	Number	Percent of total	Number	Percent of total
Total	5,075	100.0	2,917	100.0	2,158	100.0
Head did not work last year	2,328	45.9	994	34.1	1,336	61.9
Head worked last year	2,716	53.5	1,894	64.9	822	38.1
Head in Armed Forces	29	.6	29	.1		
Worked full year, total (50 to 52 weeks)	1,188	23.4	982	33.7	206	9.5
Full-time	1,006	19.8	857	29.4	149	6.9
Part-time	182	3.6	125	4.3	57	2.6
Worked part year, total (less than 50 weeks)	1,529	30.1	913	31.3	615	28.5
Full-time						
40 to 49 weeks	194	3.8	162	5.6	33	1.5
27 to 39 weeks	254	5.0	182	6.2	72	3.3
14 to 26 weeks	309	6.1	187	6.4	122	5.7
13 weeks or less	290	5.7	152	5.2	138	6.4
Part-time						
40 to 49 weeks	55	1.1	37	1.3	18	.8
27 to 39 weeks	72	1.4	41	1.4	31	1.4
14 to 26 weeks	140	2.8	66	2.3	74	3.4
13 weeks or less	215	4.2	87	.3	128	5.9
Earnings as a percent of total money income among families with workers						
Total Families	3,205	100.0	2,112	100.0	1,092	100.0
1 - 24	666	20.8	320	15.2	346	31.7
25 - 49	343	10.7	163	7.7	180	16.5
50 - 74	342	10.7	181	8.6	161	14.7
75 - 99	436	13.6	330	15.6	107	9.8
100	1,418	44.2	1,119	53.0	299	27.4
Mean Earnings	$2,456		$2,527		$1,350	

a. Source: U.S. Department of Commerce, Bureau of the Census, Current Population Reports, Consumer Income: Characteristics of the Low Income Population, 1972 Series P-60, No. 91, December 1973, Tables 30, 41, and 42.

Not surprisingly, in only 20 percent of the part-year cases is the inability to find work cited as the major reason for the part-year employment. Similarly, complete absence from the labor force is attributable chiefly to the unavailability of work in but 4 percent of those cases. Household responsibilities are the major barrier to full-year employment among low income women.[7]

To further buttress the point that employment provides only partial support for poor female-headed families, the data in the bottom half of Table II-4 are helpful; nearly half of such families with earners receive less than 50 percent of their annual income from employment. Mean annual earnings among this entire component of the working poor is $1,350.

Such earnings reflect not only part-year employment, but also low wage rates. We must look, then, beyond labor force participation, employment rates, and annual hours of work to wage rates. These, of course, are largely related to the industrial and occupational position of the women. The data in Table II-5 reveal the concentration of AFDC mothers in two low wage occupational categories, clerical and service workers; with the census data indicating that nearly two-thirds of those who are clerical workers are lower level clericals, not secretaries or stenographers. William's dissertation, using the data from the DHEW study of AFDC in ten states, supplies the only available information on wage rates earned by female heads of AFDC units. As of 1968, two-thirds of such women never had earned over $1.50 per hour. Similarly, two-thirds of those employed at the time of the survey were earning less than $1.50 per hour.[8] AFDC mothers, then, are largely intermittent labor force participants who are low wage clerical or service workers. Clearly, they typically are incapable of financial self-support at acceptable income levels, but are <u>not</u> unemployable.

TABLE II-5

Occupations of Heads of Low Income Families
(Percentage Distribution)

Occupations[c]	AFDC Family Heads[a] Mothers	AFDC Family Heads[a] Fathers	Low Income Family Heads (Census)[b] Females	Low Income Family Heads (Census)[b] Males
Prof. and managers, except farm	3.4	2.7	5.2	14.4
Sales Workers	4.9	2.2	4.0	3.5
Clerical	18.9	1.9	15.7	2.7
Secretary Steno Typists	---[d]	---	4.6	.2
Other clerical	---	---	11.1	2.5
Craftsmen and kindred	2.5	14.4	1.2	19.3
Operatives, except transportation	12.5	9.7	18.4	11.9
Trans. equipment operator	.2	7.8	.5	8.3
Laborers, except farm	8.2	36.7	1.1	12.7
Farm workers and managers	3.3	16.7	4.3	19.6
Service, except household	32.4	7.9	33.8	7.2
Private household workers	13.5	.1	15.7	.3
TOTAL (with known occupation)	100.0 1,537,943	100.0 332,489	100.0 822,000	100.0 1,894,000

a. Source: U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Findings of the 1973 AFDC Study, Part I, June 1974, Tables 36 and 47.

b. Source: U.S. Department of Commerce, Bureau of the Census, Current Population Reports, Consumer Income: Characteristics of the Low Income Population, 1972, Series P-60, No. 91 December 1973, Table 28.

c. Excluded from the distributions are persons whose occupations are not provided for a variety of reasons.

d. Breakdown not available.

2. Male Heads of Families

Before characterizing the labor market behavior of male heads of welfare families, one must distinguish this group from male heads of non-welfare, low income families. In 1972, there were 2.9 million poor families with male heads. In January 1973, under .4 million male-headed units received AFDC, while perhaps an equal number were on General Assistance. The male heads themselves received AFDC or AFDC-UF payments in 75 percent of the cases where their families received such payments; and in three-fifths of the latter cases the male was "incapacitated." In another 10 percent of the .4 million male-headed AFDC cases, the male head received Aid to the Permanently and Totally Disabled. This detail serves to distinguish the welfare poor from the non-welfare poor within the group of male heads of low income families. The male heads of the welfare families must establish their disability or involuntary unemployment to be eligible for public assistance that is available to intact families. Consequently, their labor market involvement should be much more limited than that of the male heads of non-welfare poor families.

We know that there is high welfare turnover among AFDC-UF cases. The same is likely under General Assistance. Probably, then, one-third of all 2.9 million male-headed low income families received some cash public assistance in 1972; (with a higher fraction, undoubtedly, receiving Food Stamps.) How do the welfare families fare in the labor market while they receive assistance? Information is somewhat meager. Table II-1 reveals that 60 percent of heads of AFDC or AFDC-UF families are not in the labor force; only 11.7 percent of the total is employed. However, in Table II-2 we see that another one-third of such men have worked within the year preceding the survey date.

In contrast with the male heads of welfare families, males who head low income families participate heavily in the labor market. The latter include the former, of course, so the contrast between the wlefare and non-welfare male heads is greater than indicated by the available data. The labor force participation rate of male heads at a point in time is 61 percent; their employment/population rate is 56 percent. During the year, two-thirds of poor male heads do some work for pay, with 30 percent of the entire group working full-time all year. Half of the part-year workers cannot find jobs, and two-fifths of those who never entered the labor force during the year explain their absence by the unavailability of work.[9] As a group, then, low income males participate very heavily in the labor force during the year. Intermittent work, where it is the case, is in good measure involuntary.

Low income is a product of some combination of a limited number of hours worked and low wage rates. Obviously, for the 30 percent of all poor male heads who work full-time, full-year the problem of low income results from low wage rates. Unfortunately, no data exists on wage rates earned by low income males. From Table II-5, we can see that low income male heads on welfare are heavily concentrated on low wage occupations, unskilled laborers and farm laborers. Other low income male heads are more evenly distributed among unskilled and semi-skilled occupations. Three-fourths of low income male-headed families receive 75 percent or more of their annual income from earnings. But mean earnings among those who were employed for at least part of 1972 was only $2,564.

In sum, both female and male heads of poor families are heavily involved in the labor market, although participation is lower during periods on welfare. This characterization of labor market behavior, confined as it has been to welfare and non-welfare families largely living below annual poverty lines, might be expected to hold with greater force were welfare to cover families at higher levels in the income distribution. Under any likely welfare reform plan, most beneficiary families will probably mix work and welfare either simultaneously or serially.

Only recently has work progressed on how individuals mix periods of work and non-work serially.[10] Research on patterns of employment and unemployment has not focused exclusively, though, on persons in the low income population. Using data from the Survey of Economic Opportunity on the number of spells of unemployment experienced by respondents in 1966, Robert Hall was able to associate the probability of entering and leaving unemployment with the characteristics of individuals and their environment. Interesting results are that for men the probability of entering unemployment falls with an increase in the available wage, but does not decline with increasing family incomes. A spouse's income may encourage men to leave their jobs and search for better work; if this is so, an income transfer could have similar effects. Probabilities of entering and leaving unemployment, and thus the duration of spells of unemployment, also vary markedly by sex, race, and age.

What stands out in Hall's work is the instability of work among many women, blacks, and youths. This instability is not sufficiently reflected by group differences in (point in time) unemployment rates. For example, the unemployment rate among women is higher than that among men. But the duration of unemployment spells is less among women. In order, therefore, to maintain a higher unemployment rate, many more women than men must experience unemployment over time. This suggests extensive employment instability among women. Hall then used data on quits vs. layoffs to distinguish between voluntary and involuntary unemployment. His data indicate that disproportionate employment instability among black males aged 54-59 may be involuntary; for quit rates were equal but layoff rates were higher when he compared blacks and whites. This is only a start, for it is exceedingly difficult to discriminate between unemployment originating with the individual from unemployment that has its source in the job.

B. Studies of the Effects of Welfare Programs on Labor Market Behavior

Having presented data on the labor market behavior of the heads of low income families, we can proceed to a discussion of how the availability of welfare programs influences that behavior. While we have focused on the activities of the heads of families, previous empirical work also has included married women.[11] The work effort of such women becomes particularly important when transfer programs cover the near-poor as well as the poor.

1. The Theory of the Labor Supply Effects of Welfare

The economic theory of labor supply yields predictions on how transfer programs affect work effort. In particular, the theory suggests how income guarantees and tax or benefit-loss rates separately affect the amount of labor supplied by a household. The effects on work effort of other program characteristics, like the income accounting system and the work registration requirement, could also be analyzed, but these have so far received far less attention than the guarantee and tax rate. The income guarantee works like an increase in unearned income, tending to discourage work effort. The tax rate works like a cut in the wage rate and has a more complicated effect. On the one hand, it makes the return to an hour of work smaller, making work less attractive. On the other hand, it makes the person worse off, which could induce him to work harder. It can be shown that the net effect from an income guarantee and tax rate combined will always be in the direction of discouraging work effort. An important qualification must be made, however. The net impact on work effort from a new income transfer program depends on whether a household will be above or below its breakeven level of income and also on the types of programs under which it already receives benefits. For households well above breakeven levels, the effects are likely to be nil; their net wage rates are unchanged, if they are not covered, and the guarantee or some net benefit is far from its reach. For households below breakeven levels and currently not receiving any benefits, the net impact on work effort is predicted to be negative. In addition, some households slightly above the breakeven level may choose to reduce their work effort to become eligible. It is important to remember, however, that most low income families already receive some income transfers. Their guarantees and tax rates, whether from AFDC, AFDC-UF, Food Stamps, or public housing, are not negligible, especially since the simultaneous receipt of multiple benefits is common. Any conclusions on what happens to the work effort of these families depends on how the new program changes their net guarantee and tax rates.

In most studies of work effort, the term labor supply has been operationalized by reference either to participationin the labor force or time worked during some recent period, usually either the past week or the past year. Attention has focused, then, on a particular quantity of effort, but not on the timing or the pattern of effort. As Cain and Watts have noted, however, an actual count of hours worked per year may capture some of the impact of transfer programs on the timing of work: most workers are restricted by institutions to working 40 hours per week all year, or to not working at all; thus, if workers register less than 2,000 annual hours worked it is because of some (full-time) unemployment, voluntary and involuntary, not because they are working less than 40 hours per week. In an interim report on the NIT experiment, for example, Watts suggested that the effect of income transfers might be to induce workers to prolong their search for new jobs once separated from a job.[12] If hours worked per year picks up these timing effects of income transfers, it clearly does so imperfectly. In any case, it is important to note that the theoretical predictions about the consequences of income transfers have not been related directly to the timing or pattern of labor supply. This study, however, makes some empirical effort in that direction.

Although the relationship between welfare programs and employment turnover has gone virtually unstudied, that between welfare programs and the quantity of work effort has received extensive attention. In fact, three critical reviews of the literature have been written.

2. Empirical Estimates of Labor Supply Effects

Inferences on how welfare programs affect work effort largely are drawn from cross-sectional data that are not related to an income transfer program. The data typically contain observations on individuals facing different market wage rates and different amounts of non-employment income. It is assumed that all individuals in the cross-section have the same behavioral parameters. Differences in observed work effort are then explained by differences in explanatory variables like the wage rate and income. Under these assumptions, differences over time in the wage rate and income for a single individual are predicted to lead to similar differences in work effort. Since the guarantee level of a transfer program has an effect like unearned income, and the tax rate like the wage rate, the effects of the transfer program on work effort can be deduced. Most studies of this type rely on data from the mid-sixties or earlier. These studies usually ignore the affect on the net wage rates facing persons in their samples of the tax rates in existing transfer programs, but coverage of such programs, especially male-headed families, was far more limited at that time than now.

Garfinkel has summarized the results of eight studies using cross-sectional data which are unrelated to particular welfare programs.[14] Two types of results are reported: the percentage change in "labor supply" per $1000 increase in the annual NIT guarantee, and the percentage change in "labor supply" per 10 percentage point increase in the tax or benefit-loss rate. The measures of labor supply vary widely among the studies, from hours worked last year to the ratio of earnings last year over the predicted wage for last year.[15] Garfinkel characterizes the divergence among the studies in the estimated effects as being "striking and disturbing." To cite the extremes, the high estimate predicts a 5 percent decline in labor supply per $1,000 increase in the NIT guarantee; the low estimate predicts a .6 percent reduction in labor supply. With respect to the tax rate, the extreme estimates are a 5 percent decline in labor supply per $1,000 increase in the NIT guarantee; the low estimate predicts a .6 percent reduction in labor supply. With respect to the tax rate, the extreme estimates are a 5 percent decrease in labor supply per 10 point increase versus a 3 percent increase per 10 point increase. Given the mean wage rates and mean amounts of non-employment income, all of these studies predict that an NIT program with a $3,000 guarantee and a 50 percent tax rate would cause a decrease in labor supply but the amount of the predicted decrease ranges from 3 to 40 percent for male heads. Garfinkel's best judgment is that the range of estimated effects is more narrow, going only from 1 to 6 percent.

Given the recent expansion of programs for low income families containing guarantees and tax rates, the estimated additional labor supply effects of a $3,000 - 50% NIT program would have to be less than what is suggested by Garfinkel's second range of estimates. By July 1972, the average cash welfare plus food stamp benefit available to a male-headed family of four in the U.S. was $2,431,[16] while its total tax rate over the first few thousand dollars of earnings exceeded 30 percent. Of course, guarantees and tax rates varied greatly around these averages, but these figures provide an indication of the fact that a new NIT program will not be raising non-employment incomes and tax rates from near zero levels, even for male-headed families.

In summarizing the findings of non-experimental studies on how income transfers affect the labor supply of married women and female heads of households, again we rely on Garfinkel's presentation. With respect to the latter demographic groups, program data as well as the regular cross-sectional data are available for analysis. Compared to the labor supply response of male heads of households, we would expect that of married women and female heads of households to be greater. Working less than full-time all year is more acceptable for women than for men. Given prevailing attitudes on the ability to raise children and do daily housework chores, women are assumed more able than men to put to productive use time not spent at market work. Faced with an income transfer, then, we would expect women to withdraw their labor more readily than men. In fact, the cross-sectional studies yield results consistent with this expectation. Per $1,000 increase in the guarantee, the range of labor supply reductions is from a low of 4 to a high of 30 percent. (For male heads, recall that the estimates ranged from .6 to 5 percent.) Per 10 point increase in the tax rate, the estimated labor supply reductions ranged from 4 to 10 percent. (For male heads, the range was from a 3 percent increase to a 5 percent decrease.) Studies of female heads using cross-sectional data indicate that their labor supply may be somewhat less sensitive than that of married women, but still much more sensitive than male heads to both guarantees and tax rates. Studies of female heads of households using data from the AFDC program support the position that the sensitivity of the labor supply of female heads is greater than that of male heads.[17]

In viewing these results, Ashenfelter and Ehrenberg note that the estimates of the impact of income transfers on the labor supply of both categories of women are much more divergent than are those for men. Along with Garfinkel, they have listed the many serious sources of bias in them, concluding that what we really have learned is something qualitative: that the labor supply of women is more sensitive than that of men.[18] Going beyond these summaries, we should not overlook the important fact that by July 1972, the average AFDC plus Food Stmap benefit available to a female-headed family of four was $3,442. By July 1974, this sum exceeded $3,700.[19] With the value of Medicaid averaging $800 per female-headed family of four across the country and given the virtual universal coverage of poor female-headed families by these three programs, most such families faced cumulative income guarantees that are unlikely to be raised by a new, universal NIT program. Such families also faced total benefit-loss rates averaging about 30 percent; considering work-related expenses, their gain from an increment of $1 in earnings usually was less than 50 cents.

A welfare reform program is thus likely to make little change in the benefits available to female headed households. If these facts and assumptions are accurate, it follows that knowing less precisely the labor supply sensitivity of low income women should not be troublesome for policy makers. Welfare reform is likely to bring larger environmental changes for low income men, whose supply of labor also is quantitatively more important.

C. <u>A Review of the NIT Experiment and the Labor Supply Studies Resulting From It.</u>

1. <u>A Description of the Experiment</u>

The first NIT experiment was not a great success as an experiment, but in the three years that it ran in each location, it accumulated such a rich body of data that careful analysis could provide extensive new information on numerous aspects of the behavior of low income families. The first NIT experiment was launched in four cities, three in New Jersey and one in Pennsylvania. Altogether a sample of 1357 families was selected. Each family was interviewed thirteen times, once before the experiment started and then once each quarter during the three years of its duration. The questions ranged widely over many aspects of behavior including extensive questioning on labor market activities of all family members. The data from the experiment, referred to in this study as the "Wisconsin data," thus provide a three year time series for each family in a large cross-section. Whereas the previous cross-sectional studies had to infer individual behavior completely from comparisons among individuals, the NIT data provide not only the comparative data, but also a limited amount of variety in experiences for each family over time. With such a body of data an analyst has an opportunity to study not only what determines the level of a variable, but also what influences its pattern over time.

The main purpose that led to all the data collection was, of course, to conduct an experiment to test the effects of a negative income tax. Households selected for participation were randomly assigned either to an experimental or a control group; once designated to receive the experimental treatment, households were assigned, not randomly, to one of eight NIT treatments. The latter assignments, based on a complicated model designed to minimize costs for the information obtained, generally placed households with lower pre-enrollment incomes under the less generous experimental plans and households with higher pre-enrollment incomes under the more generous plans. The NIT treatments varied only in the guarantees and tax rates facing the households, and were in effect for a period of three years. The control group was to receive no payments. The experiment was thus designed to detect the effects on many aspects of behavior of several distinct variants of an NIT.

As might have been anticipated in a social experiment, unexpected changes in the welfare environment substantially altered the actual treatment of families, muddying the previously clear-cut distinctions between the various experimental treatments. New Jersey instituted the AFDC-UF program shortly after the NIT experiment began. This meant that families in the control group who were not expected to receive welfare benefits as long as they remained intact now became eligible for them; it also meant that families in the experimental groups now could choose between receiving an NIT payment, an AFDC-UF payment, or neither.[20] Before its guarantee was cut very late in the experiment, the annual guarantee in the New Jersey AFDC-UF program was $4,164 for a family of four. It was higher than the guarantee in seven of the eight experimental plans. Pennsylvania also had a welfare program with a guarantee of $3,756, exceeding five of the eight experimental guarantees.

Three additional characteristics making AFDC-UF more attractive than NIT were the automatic eligibility of AFDC-UF recipients for sizable food stamp and medicaid benefits; the deductibility in AFDC-UF of work-related expenses from earnings, which greatly reduced the effective benefit-loss or tax rate; and the short accounting period in AFDC-UF, which minimized the effect of previous income on current benefits.

In comparison, NIT had the advantage that establishing eligibility for benefits was easier because the male head need not have been fully unemployed -- as was necessary under AFDC-UF -- to get on the program. It also was easier to maintain eligibility for partial NIT benefits because the male head was subjected only to an earnings test; in AFDC-UF, male heads had to forego eligibility if they worked for 100 or more hours in any month, no matter what their earnings were. On balance, it should have been easier to get on and remain on the NIT plans, but it should have been more attractive to be on AFDC-UF than on most of the NIT plans while unemployed or working part-time.

The relative attractiveness of welfare and the various NIT plans best is demonstrated by the choices that the families made. Families in the control group, obviously, could choose between a welfare payment and no payment. Families in the experimental groups could choose between welfare or NIT payments, or neither. Previous studies conducted by the staff of the Institute for Research on Poverty at the University of Wisconsin (hereafter, the "Wisconsin staff,") used a "continuous husband-wife sample." The choices made by the families in the sample with which the Wisconsin staff worked are presented in Table II-6. Our work is based on a somewhat different sample, chosen so that we have a complete welfare record for every family included. ~~Table~~ II-7, presents the choices made by families in our sample. In both samples, participation in AFDC-UF was extensive. Note that in both samples, beginning with the third period, families in the 50-50 and 75-70 plans far more often chose to receive AFDC-UF than NIT benefits; while in the 50-30 and 75-70 plans, the division among those drawing benefits usually was roughly 40 percent AFDC-UF and 60 percent NIT. Thus, in only four of the eight plans did the NIT program typically dominate the AFDC-UF program. In the other four treatment groups, the AFDCUF participation rate typically was not less than half of what it was in the control group.

TABLE II-6

Percent of Families Receiving AFDC-UF, Receiving NIT, or Neither

(Wisconsin Sample, N=693)

Experi-mental Group G-t	1	2	3	4	5	6	7	8	9	10	11	12	Average All Periods
	Experimental Period												
50-30													
AFDC-UF	17[d]	17	17	17	21	21	24	21	21	28	21	28	21
NIT[b]	59	48	55	62	45	48	48	52	48	45	55	52	51
Neither[c]	24	35	28	21	34	31	28	27	31	27	24	20	28
N=27													
50-50													
AFDC-UF	9	11	20	23	20	17	17	23	23	20	20	14	18
NIT	11	9	9	6	3	3	3	0	0	9	9	6	6
Neither	80	80	71	71	77	80	80	87	87	71	71	80	76
N=32													
75-30													
AFDC-UF	3	5	8	6	5	8	10	10	11	10	11	8	8
NIT	92	90	89	87	87	84	81	81	81	81	75	73	83
Neither	5	5	3	7	8	8	9	9	10	9	14	19	9
N=60													
75-50													
AFDC-UF	14	13	11	11	14	13	14	13	13	9	9	6	12
NIT	60	50	54	46	34	36	33	34	34	39	37	34	40
Neither	26	37	35	43	52	51	53	53	53	52	54	60	48
N=65													
75-70													
AFDC-UF	12	10	16	16	16	14	22	22	24	26	24	20	19
NIT	14	12	10	8	6	14	12	8	8	8	10	10	10
Neither	74	78	74	76	78	72	66	70	68	66	66	70	71
N=48													
100-50													
AFDC-UF	9	11	13	13	13	11	11	11	13	17	17	11	13
NIT	85	80	78	74	70	76	72	74	67	63	61	72	73
Neither	6	9	9	13	17	13	17	15	20	20	22	17	14
N=44													
100-70													
AFDC-UF	7	9	11	13	15	17	17	15	17	9	11	11	13
NIT	67	54	50	48	44	41	41	41	41	44	44	43	46
Neither	26	37	39	39	41	42	42	44	42	47	45	46	41
N=53													
125-50													
AFDC-UF	6	6	6	5	8	7	6	7	7	7	9	8	7
NIT	91	91	92	92	86	87	88	87	85	84	80	82	83
Neither	3	3	2	3	16	6	6	6	8	9	11	10	6
N=96													
Experimental Groups													
(Weighted	9	9	11	11	13	12	13	13	14	13	14	11	12
Averages)	66	62	61	59	53	55	54	53	52	53	51	52	56
	25	29	28	30	34	33	33	34	34	34	35	37	32
N=425													
Control Group													
AFDC-UF	15	20	20	22	24	25	27	25	27	25	25	24	23
NIT	—												
Neither	—												
N=268													

TABLE II-7

Percent of Families Receiving Welfare, Receiving NIT, or Neither

(Friedman-Hausman Sample, N= 894)

Experimental Group G-t	Experimental Period 1	2	3	4	5	6	7	8	9	10	11	12	Average All Periods
50-30													
AFDC-UF	5[d]	5	12	14	17	17	26	31	29	43	33	29	22
NIT[b]	93	86	71	69	57	50	45	43	43	29	43	48	56
Neither[c]	2	10	17	17	26	33	29	26	29	29	24	24	22
N=42													
50-50													
AFDC-UF	8	12	25	29	27	27	27	37	33	31	31	31	27
NIT	61	62	25	12	6	6	2	0	2	6	6	2	14
Neither	31	27	50	60	67	67	71	64	65	64	64	67	58
N=52													
75-30													
AFDC-UF	0	1	7	8	11	12	15	14	18	15	17	18	11
NIT	98	94	87	83	82	78	72	73	71	72	66	62	78
Neither	2	5	6	9	7	11	13	13	12	13	18	20	10
N=85													
75-50													
AFDC-UF	3	4	8	9	11	15	19	20	22	16	16	17	13
NIT	85	78	62	49	46	40	37	34	32	37	36	32	47
Neither	12	17	30	42	44	45	45	47	47	47	48	51	40
N=92													
75-70													
AFDC-UF	5	7	13	15	16	18	21	16	25	21	16	18	15
NIT	49	43	21	18	13	18	16	13	12	11	13	13	20
Neither	46	51	66	67	71	64	62	71	64	67	71	69	64
N=61													
100-50													
AFDC-UF	2	2	7	5	10	10	13	16	18	15	18	21	11
NIT	97	95	85	84	74	79	72	72	64	67	62	62	76
Neither	2	3	8	12	16	12	15	12	18	18	20	16	13
N=61													
100-70													
AFDC-UF	2	2	5	8	14	12	12	17	18	14	15	15	11
NIT	93	82	62	56	49	44	41	39	38	42	42	42	53
Neither	6	17	33	36	38	44	47	44	44	44	42	42	36
N=66													
125-50													
AFDC-UF	3	5	6	7	7	8	8	8	7	7	7	9	7
NIT	94	93	92	90	86	86	86	85	84	82	82	82	87
Neither	2	2	2	3	7	6	6	7	9	10	10	10	5
N=125													
Experimental Groups (Weighted Averages) N=584	3	4	9	11	13	14	16	18	19	16	17	18	13
	86	81	68	62	57	55	52	51	49	51	49	48	59
	11	15	23	28	31	31	32	32	32	33	34	34	28
Control Group													
AFDC-UF	15	17	19	21	23	26	26	27	28	28	27	27	24
NIT	--												
Neither	--												
N=307													

(Footnotes are the same as those for TABLE II-6)

FOOTNOTES

TABLES II-6 and 7

a. The figures in the first row of each set of three rows is the percent of families in a given period in the respective experimental groups and control group which received an AFDC-UF payment.

b. The figures in the second row of each set of rows is the percent of families in the respective experimental groups which received NIT payments.

c. The figures in the third row of each set of rows is the percent of families in the respective groups which received neither AFDC-UF nor NIT payments.

d. The three numbers for a given group and time period should add to 100 percent. Where they do not, it is because of rounding errors.

It follows that if we were to compare the mean of a variable like hours worked for each of the treatment groups, observed differences could not necessarily be attributed to differences in the experimental treatments. The control group, rather than receiving no welfare, now may receive payments, but under a program other than the NIT. The comparison between the experimental group as a whole and the control group no longer isolates the "pure"effects of an NIT, but merely shows the differential effects of the two welfare programs. Since AFDC-UF is complicated, it is difficult even to compare program characteristics or to predict which program should most discourage work effort. Although an actual recipient of AFDC-UF is more limited in the amount he can work than an NIT recipient, it is more difficult to get on AFDC-UF in the first place. Those who do not get on will probably maintain a high level of work effort. The difference in work effort between experimental and control groups thus depends in part on the effects of each program on recipients, but also on the proportion of recipients among those eligible for each program. Similarly, it is also difficult to distinguish the effects of the various NIT treatments. For each of the NIT treatment groups now includes a combination of NIT and AFDC-UF recipients. In order to isolate the "pure" NIT effect one needs to separate NIT and AFDC-UF recipients, but also to identify what kind of individual chooses one program rather than the other. These are herculean tasks, but the Wisconsin staff devoted great energy and intelligence to the task of distinguishing the effects of an NIT.

Before proceeding with a review of the labor supply studies of the Wisconsin staff, it should be noted that comparisons between treatment groups is not the only way to study the data from the experiment. A successful experimental approach would, of course, provide several clearly distinguished transfer programs, with each family kept in an unchanging welfare environment for the duration of the experiment. Controlling the characteristics of the sample receiving each treatment through careful experimental design, observed differences in labor supply would then measure the "pure" effect of transfer program differences without relying on complicated inferences and questionable assumptions that go into the usual cross-section study. In view of the extensive AFDC-UF contamination in the first NIT experiment, one cannot rely on the treatment categories to identify the actual transfer environment faced by any family. Thus, comparisons of labor supply between treatment categories can identify the "pure" effects of various NIT plans only with the aid of complicated inferences and questionable assumptions.[21] In view of the complexities in studying the treatment categories, an alternative approach would be to ignore them in analyzing the data. Each family would be faced with the guarantee and tax rate for the program in which they actually participate. The separate effects of these would have to be deduced from a regression analysis of work effort over a cross-section of all families. The chief advantage of the experiment is thus lost, but it is lost even if the analysis continues to use the treatment groups. The remaining advantage of the experiment, other than producing a large body of data, is that it did face families with a substantial variety of welfare experience. Over the cross-section, there is much more variation in guarantees and tax rates than if AFDC-UF had been the only welfare program. This improves the chances of getting reliable estimates of guarantee and tax rate effects. We are not arguing that analysis by treatment groups is wrong. Rather, in view of the complications, it may be an inefficient use of research time when the data provides so many interesting research opportunities.

2. The First Results from the NIT Experiment

a. Response of Male Heads

Harold Watts did the analysis for the Wisconsin staff of the labor supply response of married men.[22] Having presented results which consciously ignore the AFDC-UF problem, he conducted his more sophisticated analyses after excluding all families in the 50-50 and the 75-70 treatment groups. In the latter work, there is no further effort to adjust for the AFDC-UF problem in the other six treatment and control groups.

The important results of his work are presented here. First, his findings varied with his measure of the dependent variable and by ethnic group. Labor force participation seemed to be unaffected by the several treatments. Over time, employment fell and unemployment rose, however, among whites and Spanish-surname male heads, while they moved in the opposite direction for blacks; only for the Spanish-surname were the changes statistically significant. Hours of work fell significantly in the white and Spanish-surname groups, the decreases becoming larger with the passage of time during the experiment; among blacks, hours of work, like the other measures of supply, showed a surprising, although not significant increase. Secondly, decreases in work effort were greater the lower the male head's normal level of earnings. For Watts, a pivotal example of the experiment's effect was the 4-5 percent reduction, from the mean of 35, in the weekly hours of work for white males under the 100-50 plan in the middle of the experiment.[23] Thirdly, while a statistically significant experimental effect was detected in comparing the entire experimental group with the control groups, no consistent pattern was observed among males facing different guarantees and tax rates. As between persons in the plans with 50 percent and 70 percent tax rates, for example, this should not be surprising: at the close of the experiment, only 5 of 85 persons and 26 of 86 originally assigned, respectively, to the 75-70 and 100-70 plans were still receiving NIT payments, the remainder being either on AFDC-UF or not receiving a NIT payment.

Garfinkel analyzed the impact of the AFDC-UF problem on the results of the experiment by trying to determine the sensitivity of the findings of the experiment to various assumptions about how those who received AFDC-UF during the experiment would have worked in the absence of the program.[24] He tries to test the effect of three alternative assumptions: 1) that those in all groups who received AFDC-UF would have worked in its absence as much as they did when it was available; 2) that such persons would have worked the same amount as persons in the sample that never went on AFDC-UF but had similar demographic characteristics and were in the same group; 3) that persons in control group families receiving AFDC-UF would have worked the same as those in the control group who did not, but persons in experimental families receiving AFDC-UF would not have changed their work effort in its absence. In Garfinkel's view, these assumptions will yield, in turn, low, intermediate, and high estimates of what the differences in labor supply between control and experimental groups would have been in the absence of the AFDC-UF program. For example, the third assumption implicitly imposes changes in behavior on sample members that should exaggerate the differences between control and experimental groups that otherwise would have arisen. Presumably, part of the difference within the control group in work effort arose not because some people had a stronger taste than others for welfare, but rather because they became involuntarily unemployed; and some of the experimental group persons whose work effort was negatively affected by AFDC-UF would have worked slightly more under less generous and constraining NIT plans. If so, increasing substantially (by this third assumption) the work effort of certain control families and leaving unchanged that of greatly affected treatment families maximizes the difference in work effort between the two groups.

Using average hours worked for the twelve experimental periods as his measure of labor supply, Garfinkel finds that the experimentals worked 2, 6, and 9 percent less than the controls under assumptions 1, 2, and 3, respectively; with the latter two differences statistically significant at or above the 5 percent level.[25] When he makes the comparisons by each of the distinct experimental plans, statistically significant differences between the respective groups and the control group begin to appear under the second of the three assumptions and, as might be expected, only for the more generous treatments. Recall that under the less generous treatments, there were few families who actually received NIT payments. Thus, there was a trivial number of cases in those groups among whom control-experimental differences were likely to arise; and any observed differences, when averaged over all persons in the groups, would have to be small. Garfinkel, therefore, summarizes his results by noting that the AFDC-UF program did affect the findings of the experiment, although the effects in absolute terms were not very great. Moreover, by dominating the less generous treatments, the AFDC-UF program made it virtually impossible to develop coefficients for the increase in an NIT tax rate from 50 to 70 percent.

b. Response of Married Women

In the analysis of the response of married women by Glen Cain and his colleagues, families in all plans receiving AFDC-UF always were included; and analyses also were done distinguishing between those on and off AFDC-UF as well as among those who were above from those who were below NIT breakeven levels.[26] Given the sample design, not many families with working wives could have been selected for the experiment. Again, the results of the analysis differed according to the dependent variable used, as well as by ethnic group. Among white women, in general, work effort declined to a statistically significant degree when considering labor force participation or hours worked as the measure of labor supply. Among black women, a decline was not observed in either labor supply measure, except at the lowest levels of normal income. Among Spanish-surname women, a decline was observed only in hours worked, but it was not statistically significant. As one would expect, the reduction in work effort was greater among the married women than among the men: from a mean of roughly 4 hours per week among all white wives, i.e., those working and those not working, work effort declined among women in the 100-50 plan by roughly 25 percent (compared to 4-5 percent for the males in that group).[27] Once again, a strong and consistent pattern could not be detected in the effects of rising guarantees or tax rates.

Like Watts, Cain hypothesized a relationship between normal income and the impact of a NIT plan. In the study of married women, however, the assumption was that there was a sharp discontinuity in responses when normal incomes reached NIT breakeven levels of income; families below such levels were assumed to be influenced by the relevant program parameters, while those whose incomes were above such levels were assumed to be totally unifluenced. In fact, Cain did find a relationship between the normal income of a family and the impact of an NIT plan. By facing the families whose incomes were on different sides of the breakeven levels with different program parameters, Cain and his colleagues estimated a quantitatively smaller but a statistically more significant impact of guarantees and tax rates on various measures of work effort than when all sample members were faced with similar parameters. Their findings, interestingly, were sensitive to the measure of normal income which was utilized to determine breakeven levels. By estimating a quadratic function, the authors were able to detect, especially for Black and Spanish-surname families, that work disincentives were greatest at very low levels of normal income but that they reached their peak well below breakeven levels of income.[28]

The work of the Cain group also included an attempt to estimate the impact of the AFDC-UF program on the findings for their sub-group of the sample. Cain's group concluded that the AFDC-UF program did not have a statistically significant impact on the overall findings with respect to married women. Interestingly, though, when they used average hours worked over the twelve experimental periods as their measure of labor supply, two of their four models yielded results quite comparable to those of Garfinkel for the same group.[29] Garfinkel had found that married women in the experimental groups worked 14 percent fewer hours than did women in the control group, using his second assumption on how to treat the AFDC-UF contamination problem. Estimates by Cain in his two modelswere roughly 12 percent. Garfinkel's differences also were not statistically significant. Not having come up with statistically significant results when incorporating Garfinkel's second assumption, the Cain group conducts all of its work by including all families in all eight treatment groups in their sample. (Recall that Watts dropped the two least generous plans.)

c. The Response of the Family

Robinson Hollister conducted the analysis of the impact of the NIT plans on the labor supply of the entire family unit.[30] Hollister always eliminated all families when they received AFDC-UF and sometimes eliminated in his empirical work those not receiving AFDC-UF if they were in the 50-50 and 75-70 plans. He also often distinguished the responses of families above and below breakeven levels. His findings also vary with the dependent variable used, as well as by ethnic group and experimental time. Among white families, both earnings and hours declined significantly in the experimental group. Among black families, earnings increased significantly but hours of work declined significantly. Among Spanish-surname families, earnings and hours declined signficantly. Among whites and Spanish-surname families, the declines increased with time. Lastly, as Watts and the Cain group found, theoretically expected and consistent patterns by guarantees and tax rates generally could not be detected.[31] For whites and Spanish-surname families on the 100-50 plan, the induced decline in hours worked was on the order of 10 percnet. Another interesting finding of Hollister's is that declines in work effort were greater for families with greater variance in income, other things constant. This finding, he speculates, could be attributable either to the fact that families with high variance learn about the implications of high tax rates; or that the NIT payments reduced the need for families to send secondary workers in a family into the labor force when the primary worker became unemployed.

Hollister's findings on the relationship between normal income and the impact of the NIT treatments on the labor supply response of the family appear to be at variance with those of Watts and Cain. He finds that the higher the level of normal total family earnings, the larger the negative experimental differential in family hours or earnings. His speculative explanation is that:

> "this greater responsiveness may have reflected the concentration of families with working wives at higher earnings levels. As has been found in other chapters of this report, working wives respond more than male heads to the disincentive effects."[32]

Thus, the apparent inconsistency among the three authors well may be reconcilable.

Beyond excluding all families when they received AFDC-UF, Hollister attempted to test further the impact of the AFDC-UF alternative on his findings. Hollister developed a special subsample of families in the remaining six treatment groups who typically would have received a higher NIT than AFDC-UF payment, given their normal incomes and the parameters of the two particular program alternatives facing any one of them. Using this small subsample, he concluded that differences in tax rates had a bigger impact on work effort than he previously was able to uncover, but guarantees had no impact. Given the fact that he was compelled in this subsample to consolidate all ethnic groups, his confidence in these findings is very limited.

Supplementing Hollister's analysis of the impact of AFDC-UF on the family's work effort is Garfinkel's. His findings are that family labor supply, whether measured by family earnings or family hours worked, was sensitive to the AFDC-UF program. The differences between the control and experimental groups were 6,9, and 13 percent, depending on the use of assumptions 1,2, or 3. All three of his differences were statistically significant at or above the 5 percent level.[33]

As was the case with studies using non-experimental data, relatively little energy was devoted to measuring the impact of the NIT plans on employment <u>patterns</u>. Holding constant the characterisitcs of individuals and the jobs they held, Seymour Spilerman and Richard Miller attempted to assess the effects of the generosity of the NIT plans on the rate of job turnover, the duration of unemployment, and the pattern of re-employment.[34] Spilerman and Miller did not adjust for the AFDC-UF problem, nor did they distinguish between families with and without NIT payments. Contrary to their expectations, they found that job turnover declined with the increasing generosity of the NIT plans, the latter apparently failing to induce added search in the labor market.

Again, contrary to expectations, plan generosity was not positively correlated with the duration of unemployment, although a hint of such a relationship appeared among whites. Since Spilerman and Miller detected evidence suggesting that workers at low levels of earnings increased their job attachments as a result of receiving NIT benefits, they speculated that people treat such benefits as wage increases on current jobs. Lastly, the authors found some supplort for the view that generous NIT benefits facilitated the movement of younger, more educated family heads into jobs with potential growth in earnings and satisfaction. We cannot be sure, however, that they were observing something other than normal job mobility patterns among age groups. The study is limited by failure to account for the AFDC-UF problem, as well as by the repeated inability to generate statistically significant results.

D. Conclusion

Our review of the literature on the employment of family heads in the low income population and how it is affected by the guarantees and tax rates of welfare programs has yielded several major points.

1. In the low income population most family heads, female as well as male, are employable. While only 15 percent of female heads of AFDC families are employed at any moment in time, roughly 40 percent work at some time during three years. Employment typically is part-year or part-time and at relatively low wage rates. Not surprisingly, employment is more prevalent among male heads of poor (not welfare poor) families. During the year, roughly two-thirds of such men work at some time. The extensive labor force attachments of such women and men suggests that under the existing welfare program most families will work of their own volition at some time; and thus programs which get them jobs largely will be affecting the timing of their work.

2. Studies using non-experimental data suggest that guarantees and tax rates in welfare programs should affect work effort negatively. The effects vary by sex, the work effort of women being more sensitive to welfare programs than that of men. [illegible]e effects for either group are not sizable. For example, the consensus of studies

using non-experimental data is that a $1,000 increase in the guarantee of a welfare program should lead to a decrease in work effort among male heads of no more than 5 percent during the year.

3. The first studies of the NIT experiment also suggest that work effort of low income persons is sensitive to income guarantees, but they find no effect of tax rates. Because of the surprise development of the AFDC-UF program in the state where the experiment was conducted, the design of the experiment was damaged badly. Consequently, we believe that although the data generated by the experiment are useful in studying the impact of welfare on work, the use of the original experimental groups in doing this empirical work is ill-advised. Reliance on the findings resulting from the early analyses of the NIT experiment should be limited.

4. Analyses of work effort in the low income population and how it is influenced by welfare programs concentrate on the quantity rather than the timing of work. Useful studies have been done recently on work patterns in the general population. This study analyzes work patterns in our samples and how they are affected by welfare programs.

CHAPTER II

FOOTNOTES

1. This is a title of a recent book that emphasizes the point: S. Leyitan, M. Rein, and D. Marwick, Work and Welfare Go Together (Baltimore: Johns Hopkins University Press, 1972).

2. The AFDC data are for "AFDC mothers in the home." In 1973, roughly 85 percent of AFDC units were headed by females. Thus, the data in the tables that follow largely refer to female heads of AFDC units, not simply AFDC mothers.

3. Robert E. Hall, "Why Is the Unemployment Rate So High At Full Employment?" Brookings Papers on Economic Activity, Vol. 3, 1970, p. 390.

4. Robert George Williams, "AFDC And Work Effort: The Labor Supply of Low Income Female Heads of Household," unpublished doctoral dissertation, Princeton University, April 1974, p. 13.

5. Over 90 percent of all poor female-headed families receive AFDC at some point during the year. (See: Barbara Boland, "Participation in the Aid to Families With Dependent Children Program," in U.S. Congress, Joint Economic, Subcommittee on Fiscal Policy Studies in Public Welfare, Paper No. 12, Part I.)

6. James Storey, "How Public Welfare Benefits Are Distributed In Low Income Areas," in U.S. Congress, Joint Economic Committee, Subcommittee on Fiscal Policy, Studies in Public Welfare, Paper No. 6, p. 100.

7. U.S. Department of Commerce, Bureau of the Census, Current Population Reports, Consumer Income: Characteristics of the Low Income Population, 1972, Series P-60, No. 91, December 1973, Table 30.

8. Williams, pp. 14 and 15.

9. U.S. Department of Commerce, Table 31.

10. Robert E. Hall, "Turnover in the Labor Force," Brookings Papers on Economic Activity, Vol. 3, 1972, pp. 709-56. See also: George L. Perry, "Unemployment Flows in the U.S. Labor Market," Brookings Papers on Economic Activity, Vol. 2, 1972, pp. 245-78; and Hyman B. Kaitz, "Analyzing the Length of Spells of Unemployment," Monthly Labor Review, Vol. 93, November 1970, pp. 11-20.

11. The impact of income transfers on the aged and teenagers has received attention. We do not review the findings of these studies.

12. Harold W. Watts, "Mid-Experiment Report On Basic Labor-Supply Response," in U.S. Congress, Senate Committee on Finance, Income Maintenance Experiments, February 18, 1972, pp. 117-8.

FOOTNOTES - CONTINUED

13. Irwin Garfinkel, "Income Transfer Program and Work Effort: A Review;" and Glen G. Cain and Harold W. Watts, "An Examination of Recent Cross-Sectional Evidence on Labor Force Response to Income Maintenance Legislation," in U.S. Congress, Joint Economic Committee, Subcommittee on Fiscal Policy, Studies in Public Welfare, Paper No. 13, pp. 1-32 and pp. 64-99; and Orley Ashenfelter and Ronald Ehrenberg, "Using Estimates of Income and Substitution Parameters to Predict the Work Incentive Effects of Various Income Maintenance Programs: A Brief Exposition and Partial Survey of the Empirical Literature," Technical Analysis Paper No. 1, Office of Assistant Secretary for Policy, Education and Research, U.S. Department of Labor, June 1973, pp. 6-9.

14. Having no data from AFDC-UF and Food Stamps, economists have not produced any studies on male heads of families using program data.

15. A compact description of the measures of labor supply used in the studies using non-experimental data is offered in: Cain and Watts, pp. 85-6.

16. Between July 1972 and July 1974, Food Stamp benefits have risen 34 percent to offset partially the recent inflation. The variation among male heads of families in cash plus Food Stamp benefits and in total tax rates arises because AFDC-UF is not provided in most of the smaller states. (These data are taken from a very useful study: James R. Storey, "Welfare in the 70's: A National Study of Benefits Available in 100 Local Areas," in U.S. Congress, Joint Economic Commitee, Subcommittee on Fiscal Policy, Studies in Public Welfare, Paper No. 15, July 22, 1974, pp. 8 and 53.)

17. Garfinkel, pp. 20-27.

18. Ashenfelter and Ehrenberg, pp. 21-22.

19. Storey, pp. 8 and 53.

20. The contamination of the NIT experiment by the surprise development of an AFDC-UF program in New Jersey and by the presence of two similar programs in Pennsylvania has received extensive attention in two previous papers, one by Irwin Garfinkel and the other by Henry Aaron. (See: Irwin Garfinkel, "The Effects of Welfare on Experimental Response," in H. Watts and A. Rees, editors, Final Report of New Jersey Graduated Work Incentive Experiment, Institute for Research on Poverty, University of Wisconsin-Madison and Mathematica, 1974, Part C, chapter II; and Henry Aaron, "Lessons from New Jersey-Pennsylvania Experiment," unpublished paper presented at a Brookings Institution conference on the NIT experiment, April, 1974.)

21. Cain and Watts, in assessing the non-experimental studies of the relationship between income transfers and labor supply, point to the issues on which researchers are compelled to make assumptions before they can do their work; and then indicate how sensitive these research results are to the assumptions that are made. The NIT experiments were launched to avoid such problems; but the analysts of the first experiment were forced by unanticipated developments to rely on a new set of questionable assumptions.

22. Harold W. Watts, "Labor-Supply Response of Married Men," in Watts and Rees, Part A, chapter IIa.

23. Ibid., p. 46.

24. Garfinkel's analysis extended to married women and the entire family unit. We report his results for these two groups when we discuss below the papers by Cain, et al., and Hollister. (Irwin Garfinkel, "The Effects of Welfare on Experimental Response.")

25. Using average earnings as his measure of labor supply, Garfinkel found insignificant differences for married women between the experimental and control groups. In good part, this was attributable to a special problem, described below, which arose in gathering data.

26. Glen G. Cain, Walter Nicholson, Charles Mallar, and Judith Woolridge. "The Labor Supply Response of Married Women, Husband Present, in the Graduated Work Incentive Experiment," in H. Watts and A. Rees, Part A, chapter IIIa.

27. Cain, Tables 2 and 3, pp. 5 and 6.

28. Cain, et al., pp. 54-6 and 75-6.

29. Compare the results in Tables 2 and 15 in the paper by Cain, et al.

30. Robinson Hollister, "The Labor-Supply Response of the Family," in H. Watts and A. Rees, Part B., chapter Va.

31. Hollister, Tables 4-11, pp. 16 and 25.

32. Hollister, pp. 19-24 and 54.

33. Garfinkel, Table 8, p. 39.

34. Seymour Spilerman and Richard Miller, "The Effect of Negative Tax Payments on Job Turnover and Job Selection," in Watts and Rees, Part B, chapter VII.

CHAPTER III

Work and Welfare Experience in the Two Study Samples: Some Preliminary Observations

In this chapter we look at work and welfare experience in our two data samples to see if any relationships emerge without applying complicated statistical techniques. We examine both work and welfare experience in two stages. For work experience, we first consider what distinguishes those who work at least some of the time from those who never do. Then we consider the work effort and earnings patterns of those having some work experience during periods covered by the data samples. Similarly for welfare experience, we look first at the difference between those who receive welfare at least some of the time and those who never do. We then investigate the welfare experiences of those who ever are recipients. It should be noted that our later statistical investigations of work and welfare experience are concerned only with the second stages of these two problem areas. Thus, earnings patterns are studied statistically in chapters V and VI, but only of these who at some time work. Welfare patterns are studied statistically in chapter VII, in this case only for those who at some time receive transfer payments.[1] Chapter IV is non-statistical, investigating the Wisconsin data by a case history approach. Statistical techniques have the advantage that several explanatory factors for a variable can be considered at one time in such a way that the separate effect of each can be distinguished. However, a certain amount of simplification and abstraction necessarily is involved. This chapter together with chapter IV, both non-statistical in method, are intended to fill in the picture suggested by the statistical investigations of chapters V, VI and VII, and also to serve as a check on their results.

As noted, the data from the negative inocme tax experiment are derived from a set of thirteen quarterly interviews, administered over a 36-month period, one at the outset and then at intervals of three months during the actual experiment. Originally, 1357 families were selected for the experimental and control groups. Our work on welfare patterns is done with a sample of 894 families, chosen because continuous information on their AFDC-UF and NIT experience was available to us. Our work on earnings patterns is done with a much smaller sample because of missing information. With respect to labor market activities, respondents were questioned about the nature of their labor force participation and earnings only for the last week of each quarter. Continuous labor force histories were not developed. With respect to welfare experiences, respondents were questioned about their presence on a cash welfare program at any point during the quarter and the size of their AFDC-UF payments for the quarter; records also were kept on the respondents presence on a NIT plan and on the size of their quarterly payments.[2]

Previous studies of welfare concentrated on a study of welfare status -- whether a family is on or off welfare. This gives only a partial picture of the welfare experience of a family, since the amount of its transfer payment may fluctuate frequentl even though its welfare status, "on welfare," remains constant. From another viewpoint, the cost of aiding a family depends not only on the fact that it receives a transfer payment but also on the amount of that payment. The availability of periodic information on payments permits analysis of such changes and is a major advantage of the Wisconsin data set. The data on transfer payments, moreover, comes from interviews administered at short, frequent intervals and were cross-checked with records from the AFDC-UF and NIT agencies. In spite of the fact that individual changes in welfare status were not recorded, a reasonably accurate picture of changes in welfare status can be deduced for each family. Complete on-off-on again cycles are highly unlikely to occur within any quarter. Therefore, changes in welfare status are likely to be measured fairly accurately by recording welfare status at just one point during the quarter. In contrast to the longitudinal data available for New York City, ours do not contain direct information on the reason for movements on and off AFDC-UF, on and off the NIT plans, or from one transfer program to another.[3] Nevertheless, we will attempt to infer whatever possible about determinants of changes in transfer payments in our later statistical analyses. This work is novel, since heretofore research has been confined by available data to analyzing changes over time only in welfare status, not in actual payments.

The absence of continuous work histories in the Wisconsin data implies that we are unable to measure directly the duration of jobs and unemployment. Of course, stated explanations for changes in employment status are not provided. Whereas changes in welfare status can be reasonably deduced from once-a-quarter observations, such is not the case with employment status. Hall estimated the duration of unemployment spells for low-income males, aged 30, during a period of relatively low unemployment, to be roughly five weeks.[4] Further, among those persons who experienced unemployment in 1969, 16 percent had three or more spells of unemployment.[5] Such persons are likely to be concentrated in the low-income population. With unemployment spells being both short and frequent for certain persons, assessing employment status only in the last week of a thirteen week quarter runs the risk of inaccurately measuring work experience. Although we cannot provide reasonable estimates of changes in employment status, we can offer an analysis of earnings, both their mean over time and their variability for each individual. Indeed, a family may go on welfare not only because its members are out of work, but also (depending on the welfare system) because it is poor. Poverty may come either from no earnings or from positive, but low earnings. A fuller picture of the need for welfare arisesfrom a study of earnings rather than from employment status alone.

The second data set to which we had access comes from the Panel Study on Income Dynamics of the University of Michigan. Families in this nationally representative sample were interviewed annually for five years.[6] For our analyses, we selected famil which were, except for the possible departure of any member, essentially intact; whose income, in any of the five years, was in the bottom fifth of the income distribution; a whose head was not over 60 in the first year of the study. There were 1635 such famil on the Michigan data tape. Continuous work and welfare histories also were not develop in this study. From questions about labor force activities in the past week and about weeks worked during the past year, estimates were made of hours worked and hours unemployed for the year for the family head and spouse. With respect to welfare activit respondents were asked to provide an estimate of the total amount of welfare payments received by the family during the year. It is thus even more difficult to study either welfare or employment status with the Michigan data than with the Wisconsin data. We shall again concentrate on explaining transfer payments and earnings.

The remainder of this chapter divides into a description of the work experience of persons in the Wisconsin and Michigan study samples in part A, of their welfare experience in part B, and their work experience during periods in which their families receiv welfare payments in part C.

A. Work Experience

The firm attachment to the labor force of male heads of low income families is borne out by the data we offer on the work behavior of male members of our two samples Table III-1, part A shows that 92 percent of the male heads in the Wisconsin sample worked at some time during the three year experiment. To determine the basis for the non-participation of the remaining 8 percent, the non-workers are divided in the third and fourth columns in the upper part of Table III-1 into two groups, differing in the amount of time the male head was in his original household.

TABLE III-1

Work-Related Characteristics of Workers and Non-Workers

A. Wisconsin Sample

Characteristics	MALES Workers (N=820)	MALES Non-Workers (N=74)	MALES Non-Workers, Prsnt. 6 Prds. or less (N=44)	MALES Non-Workers, Prsnt. More Than 6 Prds. (N=30)	FEMALES Workers (N=292)	FEMALES Non-Workers (N=602)
Mean No. Prds. Prsnt. (H)	11	5	--	--	10	11
Mean No. Prds. Prsnt. (FS)	12	11	12	11	12	12
Pct. With Children ≤ 5	79	45	43	46	71	79
Mean Age	36	42	42	42	33	33
Pct. HS Graduates	22	8	4	13	30	21
Pct. Unhealthy	52	43	11	90	60	56
Pct. Blk. & Span-Surname	57	66	82	43	58	58
Pct. With Training	24	11	2	23	14	8
Pct. Whose Spouse Worked	30	65	30	43	84	96

B. Michigan Sample

Characteristics	MALE HEADS Workers (N=1017)	MALE HEADS Non-Workers (N=46)	FEMALE HEADS Workers (N=442)	FEMALE HEADS Non-Workers (N=130)	FEMALE SPOUSES Workers (N=706)	FEMALE SPOUSES Non-Workers (N=296)
Mean No. Prds. Prsnt.	4.5	4.3	5	5	4.3	4.4
Mean No. Prds. Sourse Prsnt.	3.9	3.0	--	--	4.1	4.3
Pct. With Children ≤ 5	82	74	80	77	81	83
Mean Age	39	50	41	43	34	36
Pct. HS Graduates	53	33	65	55	51	32
Pct. With Disability	31	70	50	79	--	--
Pct. Disfigured	13	63	11	40	--	--
Pct. Blk. & Span.-Surname	57	48	78	81	57	55
Pct. With Training	19	15	19	9	--	--
Pct. Whose Spouse Worked	73	47	--	--	98	93

Notes to Table III-1

In the notes to each table variables will be defined only if they were not defined in the previous tables.

A dash indicates that data are not available on the particular variable for the indicated group; or that none should be available, as in the case of information on a spouse of a woman who always was the female head of the family.

(H) and (FS) stand always for "Head" and "Female Spouse," respectively.

Definitions of Variables:

1. Mn. No. Pds. Prsnt. (H) and (FS): The mean among persons in the group of the number of periods that he (she) was with the family with which he lived at the outset of the study. Periods are quarters in the Wisconsin data and years in the Michigan data.

2. Pct. With Children ≤5: The percent of the group that had at some time during the study a child in the family that was aged 5 or less.

3. Pct. HS Graduates: The percent of the group that completed 12 or more years of schooling.

4. Pct. Unhealthy: The percent of the group that was considered unhealthy as a result of having some chronic illness at some time during the experiment. This measure is the "Elesh health variable" on the Wisconsin staff's analysis tape.

5. Pct. With Training: The percent of the group that had formal job training outside of a regular school program.

6. Pct. Whose Spouse Worked: The percent of the group whose spouse ever worked during the study period.

7. Pct. With Disability: The percent of the group that ever suffered from a disabling physical or nervous condition that would impair the ability to do a certain type or amount of work during the study period.

8. Pct. Disfigured: The percent of the group that was judged by the observation of the interviewer to suffer from a disfigurement that would limit the ability to find work.

Of the 44 men who spent less than 7 of the 12 periods with their original households, 41 left between the time of their selection and the time the experiment began. Thus, since we do not know about their work behavior over the three years of the experiment, we cannot be sure that they are non-workers. Among the remaining 30 non-workers, bad health seemed to be the distinguishing characteristic. While 52 percent of the males who ever worked were over "unhealthy," i.e., had a chronic illness at some time during the experiment, 90 percent of the non-workers (present at home for more than 6 periods) were at some time unhealthy.

In the Michigan study sample, non-workers also are a small minority, 4 percent, of the male heads. Again in this group, the distinguishing characteristic of non-workers is their poor health: roughly two-thirds of them suffered from a nervous or physical disability that they asserted limited the kind or amount of work they could do; while a substantially overlapping group had a disfigurement which their interviewer felt would interfere with their ability to find work. While such disabilities and conditions were common among workers, they were far more prevalent among non-workers. For the most part, male heads of low income families stay out of the labor force entirely for long periods only if they suffer from some disabling condition.

Also consistent with the review in Chapter II is the less prevalent attachment to the labor force of female spouses and female heads. In the Wisconsin sample, only one-third of the female spouses worked at any time during the three year experiment. In the Michigan sample, over the longer period of five years, twice that fraction of female spouses worked at some time. Among women who during the study always were female heads in the Michigan sample, 77 percent worked at some time. In both study samples, female labor force participation appears to be slightly more common if the male head is absent. In the Wisconsin sample, male heads are present for an average of 10 periods in households where the female spouse ever worked; and are present roughly 10 percent more often in the households of non-workers. In the Michigan sample, among the women who always were female heads, the proportion of workers, 77 percent, is slightly higher than the 70 percent among women who were female spouses at least some of the time.

Since the group of female spouses includes women who occasionally were family heads, the contrast between the two groups in work effort resulting from a male head's absence may be blurred. Note also that working women are more highly educated than non-workers, so that they may have superior wage opportunities. Also, they are less likely to suffer from disabling conditions. Even so, disabilities are common among workers.

Having distinguished between workers and non-workers, we proceed with a discussion of work experience by using data only on workers in the two study samples. Before going further, consider how the various "means" presented in Table III-2 and thereafter are obtained. For each individual in the Wisconsin sample, we have twelve quarterly observations on many of the variables, like hours worked (measured as the hours worked in the last week of each quarter). To reduce this information to manageable form, we first take the mean over time of hours worked for each individual. Then an average of those individual means is presented in the tables for each group of individuals under consideration. Similarly, the standard deviation of a variable like earnings is calculated for each individual from his time series on earnings. The tables then present an average of these standard deviations for all of the individuals in a particular group. (The latter mean appears in the tables as "Mean Std. Dev. Earnings.")

Our discussion of the male and female workers in Tables III-2 and III-3 is directed towards making initial judgments about the determinants of low earnings. Families receive income transfers in our samples principally because their earnings are insufficient, the insufficiency resulting either from earnings that are low regularly or that are interrupted with varying frequency. Thus, we inquire as to why earnings are inadequate, knowing that earnings for the individual are equal to the product

TABLE III-2

Work-Related Characteristics and Work Experience of Male Heads of Families, By Hourly Wage Rates and Hours Worked

A. Wisconsin Sample

Characteristics	Mean Wage Rates .01-2.40 (N=206)	2.41-2.80 (N=195)	2.81-3.20 (N=183)	3.21-4.80 (N=201)	4.81+ (N=15)
Pct. With Children < 5	74	81	79	83	73
Mean Age	38	35	37	35	35
Pct. Unhealthy	56	54	51	48	40
Pct. Disfigured	--	--	--	--	--
Pct. HS Graduates	16	21	26	27	27
Pct. With Training	21	24	28	24	53
Pct. Black & Span.-Surname	54	61	57	59	20
Mean No. Pds. Empl.	8	10	10	11	9
Mean Hrs.Worked	27.7	31.3	33.1	34.2	24.1
Mean Hrly.Wage Rate	--	--	--	--	--
Mean Earnings	731	1015	1197	1447	1658
Mean Std.Dev. Earnings	308	364	406	468	886
Pct.With 3+(2+)Jobs	32	28	22	13	33

Characteristics	Mean Hours Worked 1-20 (N=155)	21-30 (N=118)	31-40 (N=324)	41+ (N=187)
Pct. With Children < 5	79	78	78	82
Mean Age	35	36	38	37
Pct. Unhealthy	65	62	50	39
Pct. Disfigured	--	--	--	--
Pct. HS Graduates	19	17	24	27
Pct. With Training	26	25	22	28
Pct. Black & Span.-Surname	65	54	59	45
Mean No. Pds. Empl.	4	9	11	12
Mean Hrs. Worked	--	--	--	--
Mean Hrly. Wage Rate	2.65	2.95	2.92	2.89
Mean Earnings	618	933	1212	1438
Mean Std.Dev. Earnings	467	562	339	325
Pct. With 3+(2+)Jobs	30	29	20	15

TABLE III-2

continued

B. Michigan Sample

Characteristics	Mean Wage Rate .01-1.60 (N=354)	1.61-2.50 (N=377)	2.51+ (N=285)	Mean Hours Worked 1-1000 (N=101)	1001-1800 (N=246)	1801+ (N=669)
Pct. With Children ≤ 5	84	82	80	85	84	82
Mean Age	42	38	37			
Pct. Disabled	43	27	22	64	43	22
Pct. Disfigured	21	10	8	41	15	8
Pct. HS Graduates	41	56	65	51	55	53
Pct. With Training	14	18	26	20	20	18
Pct. Black & Span.-Surname	63	57	49	55	58	58
Mean No. Pds. Empl.	4.3	4.2	4.2	2.7	4.3	4.4
Mean Hrs. Worked	1981	1964	1864	---	---	---
Mean Hrly. Wage Rate	---	---	---	1.91	2.20	2.06
Mean Earnings	2278	3895	5940	926	3266	4590
Mean Std.Dev. Earnings	793	1082	1664	862	1404	1062
Pct. With 3+(2+)Jobs	38	34	30	1	25	43

Notes to Table III-2

In each of the "mean"variables appearing below, that mean should be interpreted in the following way. First, the mean over time is calculated for each individual for the values of his wage, for example. Then, an average of these individual means is taken for the individuals in each group. It is these subgroup averages or means of individual means that appear in the table.

Definition of Variables

1. Mean No. Pds. Empl.: Based on the number of quarters in the Wisconsin data (years in the Michigan data) during which persons in the group were employed at some time.

2. Mean Hrs. Worked: Individual and group means based on hours of work per week in the Wisconsin data (per year in the Michigan data).

3. Mean Hrly. Wage Rate: Based on the hourly market wage rate. The mean for each individual is calculated, for periods in which he is present and for which a positive wage is available as a result of employment by dividing earnings by hours worked. Wage rates in both data sets are deflated by consumer price index (1967 = 100).

4. Mean Earnings: Based on quarterly (annual) earnings calculated over the number of periods during which an individual is present in his original home. Earnings are deflated by a price index (1967 = 100).

5. Mean Std. Dev. Earnings: Earnings used in this calculation are defined in the previous footnote. The standard deviation for each individual is calculated for earnings over the 12 quarters (5 years in the Michigan data). Then the average of these is calculated for each group of individuals.

6. Pct. With 3+(2+)Jobs: The percent of persons in the group who had 3 or more jobs in the Wisconsin data (2 or more in the Michigan data) during the course of the respective study periods.

7. Mean Other Family Income: Based on mean family income for each individual, which is income exclusive of his own earnings and income transfers from welfare or NIT; is deflated by the price index; and is calculated over the entire 12 quarters in the Wisconsin data (5 years in the Michigan data).

TABLE III-3

Work-Related Characteristics and Work Experience of Female Heads and Female Spouses, By Hourly Wage Rates and Hours Worked.

A. Michigan Sample

FEMALE HEADS

Characteristics	Mean Wage Rates .01-1.60 (N=452)	Mean Wage Rates 1.61-2.50 (N=142)	Mean Wage Rates 2.51+ (N=30)	Mean Hours Worked 1-500 (N=128)	Mean Hours Worked 501-1800 (N=157)	Mean Hours Worked 1801+ (N=157)
Pct. With Children ≤ 5	82	70	90	81	80	80
Mean Age	40	37	35	40	41	42
Pct. Disabled	39	23	30	72	48	33
Pct. Disfigured	10	9	7	22	10	4
Pct. HS Graduates	63	79	77	65	65	66
Pct. With Training	18	22	30	16	16	24
Pct. Black & Span.-Surname	73	68	57	80	73	82
Mean No. Pds. Empl.	3.3	3.7	3.1	2.1	4.3	5.0
Mean Hrs. Worked	1035	1249	1027	--	--	--
Mean Hrly. Wage Rate	--	--	--	1.33	1.37	1.44
Mean Earnings	1168	2375	2830	235	1373	2692
Mean Std. Dev. Earnings	509	970	1559	301	854	755
Mean Other Family Income	1817	2179	2327	2660	1998	1072

FEMALE SPOUSES

Characteristics	Mean Wage Rates .01-1.60 (N=508)	Mean Wage Rates 1.61-2.50 (N=150)	Mean Wage Rates 2.51+ (N=47)	Mean Hours Worked 1-500 (N=366)	Mean Hours Worked 501-1800 (N=249)	Mean Hours Worked 1801+ (N=61)
Pct. With Children ≤ 5	80	81	85	81	85	61
Mean Age	35	33	30	34	33	38
Pct. Disabled	--	--	--	--	--	--
Pct. Disfigured	--	--	--	--	--	--
Pct. HS Graduates	43	68	79	50	51	53
Pct. With Training	--	--	--	--	--	--
Pct. Black & Span.-Surname	62	49	28	42	43	38
Mean No. Pds. Empl.	2.7	2.7	2.5	2.0	3.6	4.9
Mean Hrs. Worked	585	607	509	--	--	--
Mean Hrly. Wage Rate	--	--	--	1.37	1.46	1.30
Mean Earnings	835	1528	2134	557	1562	2304
Mean Std. Dev. Earnings	1010	1359	1945	468	963	709
Mean Other Family Income	4236	4734	5517	4860	4287	3616

Note:
Variables in this table are defined in the notes to Tables III-1 and III-2.

TABLE III-3

continued

B. Wisconsin Sample

FEMALE SPOUSES

Characteristics	Mean Wage Rate .01-1.60 (N=39)	1.61-2.00 (N=109)	2.01-2.40 (N=68)	2.41+ (N=59)	Mean Hours Worked .01-3 (N=56)	3.01-6 (N=35)	6+ (N=184)
Pct. With Children < 5	67	70	69	80	73	86	68
Mean Age	35	33	34	33	33	29	34
Pct. Unhealthy	51	66	57	58	68	69	56
Pct. Disfigured	--	--	--	--	--	--	--
Pct. HS Graduates	33	27	35	32	30	26	32
Pct. With Training	5	15	18	19	14	20	14
Pct. Black & Span. Surname	41	58	68	53	46	40	63
Mean No. Pds. Empl.	5.6	4.6	6.7	7.1	1.3	2.7	7.7
Mean Hrs. Worked	11.4	10.6	18.5	17.4	--	--	--
Mean Hrly. Wage Rate	--	--	--	--	1.90	2.08	2.11
Mean Earnings	170	221	462	539	39	105	479
Mean Std.Dev. Earnings	150	232	326	385	107	229	337
Mean Other Family Income	1232	1249	1288	1461	1340	1370	1277

of hours worked and the wage rate. Consequently, to see what factors are associated with differences in wage rates and hours worked, the workers are grouped by their average hourly wage rates and their average hours worked in the two tables.

Turning to the tables, an interesting peculiarity arises for male workers in the highest wage rate categories. In the Wisconsin data, for example, note that workers whose hourly wage rate exceeded $4.81 have fewer hours worked on the average than workers in the lowest wage rate category. This could suggest the existence of a "backward-bending labor supply curve." The standard expectation is that workers will take advantage of the greater earnings opportunities open to them by increasing their work effort in response to higher wage rates. Such behavior yields a positively sloped supply of labor curve. However, it also is possible that workers reach some wage at which they decided to take advantage of the increased well-being offered by a yet higher wage by actually decreasing their hours of work. If the latter is the case, we observed a backward-bending labor supply curve. We do not believe that this is what we are observing in this instance. Rather we think that the phenomenon being observed is an artifact of the manner in which the Wisconsin and (our version of the) Michigan samples are constructed. To fall into either sample, families had to have relatively low annual incomes. High wage workers could appear in a low income sample only if they worked relatively few hours per year. Thus, the high wage workers in this sample constitute a group that is likely to be unrepresentative of all workers who work at such wage rates. What we well may be observing then in Table III-2 is the entry into the two truncated samples of only those high wage workers who experience substantial unemployment.

Excluding the peculiar high wage category, we note that low wage rates are associated with less education and formal job training, as well as with a higher incidence of bad health and disablement. Similarly, low average hours worked over time is associated with less education and training, and especially with the greater prevalence of disabling physical or nervous conditions.

Workers earning low wage rates not only suffer more from disadvantageous characteristics, but also encounter more difficulties in the labor market which, in turn, account for their low earnings. Looking at the male workers grouped by their wage rates in Table III-2, note that low wage workers are employed for fewer periods and, reflecting their periodic unemployment, average fewer hours worked per week than do high wage workers. Moreover, job stability seems to be positively associated with the wage rate: lower wage workers experience more job changes than do high wage workers. Combining the information on hours worked with that on job changes suggests that low wage workers often experience unemployment when changing jobs rather than going directly from one job to another. In sum, the data in Table III-2 suggest that the association of low wage rates, frequent job changing, and periodic unemployment together contribute to the low earnings experienced by low wage workers.

Examining the data in Table III-2 which group the male workers by their average hours of work, we note that those who work fewer hours, like those who work at lower wage rates, more frequently experience health problems and generally have more limited educations. (They have not, however, been exposed less often to formal job training) Constrasting those in the lowest hours worked category both in the Wisconsin and Michigan data with those in all of the other hours categories, we see that those who worked relatively little did so at relatively low wage rates; and also worked in very few periods. Thus, very low earnings seem to result from persons with low wage rates working very few hours, because they frequently have long stretches of no work at all.

Some caution is needed in interpreting the mean wage rate for each hours category because of the dispersion in wage rates within each category. In the Wisconsin data, for example, 9.6 percent of those averaging 21-30 hours worked had an average market wage of over $4.40 per hour; only 2.7 and 2.3 percent, respectively, of those in the two highest hours categories had hourly wage rates exceeding $4.40. Differences in mean wages from one category to another provide only an imperfect measure of differences in the distribution of wage rates within each category.

In examining the work experience of women in the two sets, we concentrate our discussion on the Michigan data, where information exists both on female heads of families and female spouses. We continue in the Michigan data to separate women who always are female heads from those who either always or sometimes are female spouses and again inquire into the sources of low earnings. In the Wisconsin data, most women were spouses for most of the period: consequently, female spouses and female heads were not separated. Thus, as in the discussion of low earnings among men, we group the working women by their average hourly wage rates and hours worked.

With respect to work-related characteristics, the data in Table III-3 indicate that women who receive low wage rates are likely to be black or Spanish-surnamed and less highly trained and educated than are high wage workers. As with males, a distinguishing characteristic of women who work fewer hours per year is the existence of a disabling physical or nervous condition. Our crude measure of child care responsibilities suggests that only among female spouses does the presence of young children differentiate women who work varying numbers of hours.

As in the case of male heads, differences in mean earnings among women are associated with differences in wage rates and hours worked. Again, though, it is not only low-wage workers who work limited numbers of hours. For female heads and female spouses in the Michigan data, the number of annual hours worked across the three wage rate categories are, respectively: 1035, 1249, and 1027; and 585, 607, and 509. This may result from the truncation of the sample by annual family income. That is, in a sample confined to low and moderate income persons, women earning very high wage rates could not enter the sample unless they work few hours; certainly high wage female spouses of working males could not enter the sample unless they worked few hours per year.

Two concluding points in this section on work experience relate to information on the impact of welfare on the work effort of females and on the composition of family income.

Shown just below are suggestive data on the effects of available income transfer programs on the work effort or women. These result from a division of the states into four groups.

State Groupings	Average Annual Hours Worked by Heads of: Male-Headed Families	Female-Headed Families
Low Guarantee-Low Tax Rate	1842	1063
Low Guarantee-High Tax Rate	1954	879
High Guarantee-Low Tax Rate	1537	707
High Guarantee-High Tax Rate	1752	598

Those having maximum annual benefits in 1967 of $2200 or less for a family of four with zero non-welfare income are called the "low guarantee" states, the remaining states being those with a "high guarantee." Those states with especially low tax rates that result from complicated benefit formulae which are designed to restrict assistance payments to low levels are labeled "low tax rate" states, whereas the remaining jurisdictions are considered "high tax rate" states. Their cross-classification yields four groups of available state welfare programs, varying crudely in the degree to which they offer recipients incentives to work. Very roughly, we would expect work effort for a given family type to decline as we go from the low guarantee-low tax rate states to the high guarantee-high tax rate states, if low guarantees and low tax rates are least likely to reduce labor supply. Families which always are female headed, those predominantly affected by the existing AFDC program for example, experience a dramatic decline in hours worked among the four groups of states. While in the low guarantee-low tax rate states, annual hours worked for female heads of families was 1,063 over the five years, it was 598, or over 40 percent less, in the high guarantee-high tax rate states. Again, though, the reader should recall that this finding fails to control for many factors, including labor market conditions, and should be regarded only as suggestive.

A last question in this non-statistical analysis of the work experience and earnings of low income families in the sample relates to the composition of family income. The data in Table III-14 indicate that within the Wisconsin sample there is an inverted U-shape relationship between total non-transfer family income and the proportion of it contributed by the male head. Male heads contribute little to the income of the poorest families, but, in addition, the contributions of other family members are small in absolute terms. Higher family income in general comes from greater earnings of the male head. But since the earnings capacities of males in our sample are limited, the highest family incomes occur when the male earnings are supplemented by earnings of other family members. The data in part B of Table III-4 reflect the large relative contributions of female earnings to family income at the extremes of family income in this sample.

B. Welfare Experience

Paralelling the discussion of work experience, we begin by distinguishing between families who sometimes and never received income transfers from AFDC-UF (or AFDC) or NIT during the study periods. Given the rules of these transfer programs,

TABLE III-4

Average Contribution of Male Head's Earnings and Female Spouse's Earnings to Non-Transfer Family Income

A. Male Heads

Mean Non-Transfer Family Income (Over 12 Quarters)	Average Ratio in Income Class of Male's Earnings to Non-Transfer Family Income
$1 - 500	47
501 - 1000	57
1001 - 1500	76
1501 - 2000	78
2001 - 2500	68
2501 - 3000	59
3001 +	48

B. Female Spouses

Mean Non-Transfer Family Income (Over 12 Quarters)	Average Ratio in Income Class of Female's Earnings to Non-Transfer Family Income
$1 - 500	13
501 - 1000	9
1001 - 1500	6
1501 - 2000	5
2001 - 2500	9
2501 - 3000	10
3001 +	13

we would expect that variables measuring family size, the head's health, the presence of young children, and family income and its variability to affect participation in the programs. As noted in Chapter I, given the characteristics of a population, the program rules themselves obviously influence the extent of participation.

By separating those who sometimes from those who never received AFDC-UF (or AFDC) or NIT, Table III-5 allows us to investigate the impact of the factors just listed. Examining first the data for the Wisconsin sample, we note that larger families, presence of young children, and ill health of the male head all are somewhat more prevalent among those ever on AFDC-UF or NIT than those who never receive welfare payments. Clearly, though, the key factor differentiating AFDC-UF (or AFDC) recipients from those never receiving payments from that program is family income (exclusive of welfare). The sharply lower and more highly variable incomes of AFDC-UF (or AFDC) families appear to be partially attributable to the more frequent absence of male heads from their families. An interesting fact is that the difference in income level between NIT and non-NIT families is small compared to the difference between AFDC-UF and non AFDC-UF units. This results from the condition that male heads either be absent or totally unemployed before their families can receive AFDC-UF (or AFDC) payments.

For both male and female-headed families in the Michigan sample in Table III-5, differences in income and family size and the prevalence of disabilities distinguish recipients from non-recipients. Here, the measure of the presence of young children suggests no effect. Only 20 percent of the male-headed families ever were recipients compared to 62 percent of those with female heads. In part this is due to the more limited availability and greater stringency in requirements of AFDC-UF programs compared to AFDC. However, the table shows substantially lower incomes for female-headed families, accounting for at least some of the difference. The effect of program

TABLE III-5

Characteristics Related to the Receipt of Income Transfers, By Potential Transfer Program

A. Wisconsin Sample

Characteristics	ON AFDC or AFDC-UF Sometimes (N=266)	ON AFDC or AFDC-UF Never (N=628)	ON NIT Sometimes (N=521)	ON NIT Never (N=63)
Mean No. Pds. Prsnt. (H)	9	11	10	11
Pct. With Children ≤ 5	84	73	80	71
Mean Family Size	6.4	5.8	6.0	5.9
Pct. Unhealthy (H)	59	48	53	49
Mean Qtly. Non-Trnsf. Inc.	957	1620	1355	1578
Std. Dev. Non-Trnsf. Inc.	520	525	447	530

B. Michigan Sample

Characteristics	MALE HEADED Sometimes (N=166)	MALE HEADED Never (N=657)	FEMALE-HEADED Sometimes (N=355)	FEMALE-HEADED Never (N=221)	HEAD CHANGES Sometimes (N=112)	HEAD CHANGES Never (N=128)
Pct. With Children ≤ 5	78	83	76	85	86	77
Mean Family Size	6	5	5	3	6	4
Pct. With Disability (H)	69	36	66	42	--	--
Pct. Disfigured (H)	25	14	23	11	13	11
Mean Annl. Non-Trnsf.Inc.	4341	6097	2002	3940	4127	6232
Std. Dev. Non-Trnsf-Inc.	1555	1966	1127	1262	2440	2846
Pct. of Families in Prgm:						
Low Guarantee-Low Tax	17		54		41	
Low Guarantee-High Tax	15		58		37	
High Guarantee-Low Tax	37		70		63	
High Guarantee-High Tax	32		70		63	

Notes to Table III-5

All earnings and income data in Tables III-5 through III-8 are not deflated by a price index.

Definitions of Variables:

1. Mean Qtly. (or Annl.) Non-Transf. Inc.: This is a mean among families in a group of mean family income over the entire study period. It excludes only AFDC-UF (or AFDC) and NIT payments from family income per quarter in the Wisconsin data (or per year in the Michigan data). It is not deflated by a price index.

2. Std. Dev. Non-Transf. Inc.: This is a mean among families in a group of the standard deviation of the non-transfer income measure just defined.

3. Pct. of Families in Prgm.: The 50 states and Washington, D.C. are grouped according to the level of benefits they offered a family of four with zero income and according to the benefit formulae they used in 1967. The states are listed by group in footnote 7 to the text of this chapter.

parameters on welfare participation can be seen from the data on participation rates in the four groups of states. In the states with low guarantees and low tax rates, for example, 17 percent of the male-headed families were recipients of cash assistance at some time. In the states with high guarantees and high tax rates, the participation rate (over time) was 32 percent. In the former group of states, mean income among all sample families averaged $5427 annually, while in the latter group of states it averaged $6509 annually, nearly $1100 more per family per year.

Restricting attention now to recipients, we consider the factors associated with extensive dependence on such transfers. Again, variables like family size, the head's health, family income, and program parameters all should be related to the degree of dependence over time on cash transfers. As noted in the introduction to the chapter, two measures of dependence are available: on and off status; and amount of welfare or NIT payments per period.

In Tables III-6, 7, and 8 families are grouped by their average income transfer payments for the purpose of determining whether the variables just mentioned are associated with the extent of dependency. Table III-6 divides the families in the Wisconsin data into two groups, one containing those in the control group and the other containing all those in the various experimental groups. Families in the experimental groups had the option of going on AFDC-UF if they preferred it to NIT. We observe in Table III-6 that families heavily dependent on AFDC-UF (or AFDC) suffer disproportionately from the absence of their male heads and are also somewhat larger than families which have slight dependence on such welfare. For broken families, apparently, AFDC-UF probably offered a more attractive package of benefits than NIT, since the former included Food Stamps and Medicaid besides AFDC-UF payments. By contrast, as seen in the Table II-7, which groups the experimental families within their various treatment groups by their average NIT payments, extensive

TABLE III-6

Extent of Dependence on AFDC-UF (or AFDC) Program,
~~By Experimental-Control Status~~

(Wisconsin Study Sample)

	AFDC-UF Payments for Experimental Group		AFDC-UF Payments for Control Group	
	$1-200 (N=55)	$200 + (N=121)	$1-200 (N=28)	$200 + (N=83)
Mean No. Prds. Prsnt. (H)	11	8	10	9
Pct. With Children < 5	91	89	64	80
Mean Family Size	6.2	6.5	5.1	6.6
Percent Unhealthy	75	52	75	61
Mean Qtly. Earnings (H)	1140	766	1005	731
Mean Qtly. Earnings (FS)	144	54	147	66
Mean Non-Trnsf. Inc.	1346	738	1341	891
Mean Std. Dev. of Inc.	608	475	574	527
Ratio of Pds. on NIT to Pds. on Both	7/9	4.11	0/2	0/10

Earnings and income measures in this table are not deflated by a price index.

Definition of Variable:

Ratio of Pds. on NIT to Pds. on Both: This is simply the ratio of mean number of periods for which families in a group received NIT payments to the mean number of periods for which families in a group received either NIT or AFDC-UF (or AFDC) payments.

TABLE III-7

Extent of Dependence on NIT Program, By Experimental Group and Mean NIT Payment

(Wisconsin Sample)

NIT Payments for Experimental Groups

	50-30		50-50		75-30		75-50		75-70		100-50		100-70		125-50	
	$1-200 (N=36)	200+ (N=4)	1-200 (N=34)	200+ (N=0)	1-200 (N=26)	200+ (N=58)	1-200 (N=70)	200+ (N=14)	1-200 (N=32)	200+ (N=5)	1-200 (N=18)	200+ (N=41)	1-200 (N=43)	200+ (N=18)	1-200 (N=19)	200+ (N=103)
Mean No. Pds.Prsnt.(H)	10	8	9	0	8	11	10	9	11	10	10	11	12	12	11	12
Mean Family Size	5.9	4.9	5.9	0	5.0	6.1	6.0	5.9	6.1	5.7	6.3	6.5	6.2	6.0	5.1	6.1
Pct. Unhealthy(H)	50	75	44	0	31	52	40	64	59	80	39	73	49	61	32	64
Mean Qtly. Erngs.(H)	1018	203	1041	0	920	1221	1248	582	1060	281	1231	952	1307	781	1429	1186
Mean Qtly. Erngs. (FS)	93	150	151	0	222	56	137	39	178	215	281	56	208	34	164	77
Mean Non-Trnsf. Inc.	1162	765	1324	0	1264	1385	1493	783	1488	666	1642	1084	1581	907	1738	1411
Std.Dev.Non-Trnsf.Inc.	521	508	530	0	486	516	577	616	598	588	648	488	570	506	524	497
Ratio, of Pds. on NIT to Pds. on Both	7/9	12/12	3/6	0	6/9	11/12	5/7	11/12	3/5	9/9	6/8	11/12	5/7	11/1.	6/9	12/12

Note to Table III-7

Earnings and income measures are calculated as described in the footnotes to Table III-2 and III-4, respectively; except that here, as with income in Table III-4, they are not deflated by a price index.

TABLE III-8

Extent of Dependence on Welfare, By State Group, Family Type, and Amount of Annual Welfare Payments

(Michigan Sample)

	State Group, Family Type, Amount of Annual Welfare					
	Low. Guar., Low Tax					
	M.Hd.		F. Hd.		Hd. Change	
Characteristics	$1-1000 (N=30)	1001+ (N=22)	$1-1000 (N=38)	1001+ (N=41)	1-1000 (N=11)	1001+ (N=22)
Mean Family Size	6	7	4	6	5	7
Pct. With Disability* (H)	70	91	76	56	--	--
Pct. Disfigured (H)	33	36	18	20	18	9
Mean Annl. Earnings (H)	2359	1456	1362	918	3896	3373
Mean Annl. Earnings (FS)	511	489	--	--	1449	978
Mean Non-Welfare Inc.	3645	3556	2117	1995	4421	3610
Std. Dev. Non-Welfare Inc.	1276	1458	1911	1083	2181	1738

	Low Guar., High Tax					
	M.Hd.		F.Hd.		Hd.Change	
Characteristics	1-1000 (N=19)	1001+ (N=28)	1-1000 (N=25)	1001+ (N=96)	1-1000 (N=9)	1001+ (N=19)
Mean Family Size	6	5	4	5	5	6
Pct. With Disability (H)	47	82	68	63	--	--
Pct. Disfigured (H)	11	39	24	18	22	5
Mean Annual Earnings (H)	3314	2606	1594	609	4422	4612
Mean Annual Earnings (FS)	709	475	--	--	959	633
Mean Non-Welfare Inc.	4919	3806	3129	1745	4748	3754
Std. Dev. Non-Welfare Inc.	1782	1513	1292	1194	2297	2720

Note

Earnings and income measures are calculated as described in the footnotes to Table III-2 and III-5, respectively; except that here, as with income in Table III-5, they are not deflated by a price index.

TABLE III-8

(continued)

(Michigan Sample)

	State Group, Family Type, Amount of Annual Welfare					
	High Guar., Low Tax					
	M.Hd.		F.Hd.		Hd. Change	
Characteristics	$1-1000 (N=7)	1001+ (N=20)	1-1000 (N=8)	1001+ (N=53)	1-1000 (N=1)	1001+ (N=21)
Mean Family Size	6	5	3	4	3	5
Pct. With Disability (H)	43	60	63	77	---	---
Pct. Disfigured (H)	0	30	25	32	0	24
Mean Annual Earnings (H)	4177	2482	1877	705	5758	4198
Mean Annual Earnings (FS)	1641	373	---	---	1810	503
Mean Non-Welfare Inc.	6146	3870	3441	1503	9545	3717
Std.Dev.Non-Welfare Inc.	946	1741	1440	940	4802	2544

	High Guar.,High Tax					
	M.Hd.		F.Hd.		Hd.Change	
Characteristics	1-1000 (N=15)	1001+ (N=25)	1-1000 (N=11)	1001+ (N=79)	1-1000 (N=2)	1001+ (N=27)
Mean Family Size	5	7	4	4	3	5
Pct. With Disability (H)	60	72	64	62	---	---
Pct. Disfigured (FS)	13	20	9	27	0	7
Mean Annual Earnings (H)	5246	2811	1503	631	7250	4795
Mean Annual Earnings (FS)	483	155	---	---	1384	941
Mean Non-Welfare Inc.	7807	3815	4404	1761	5853	4474
Std. Dev.Non-Welfare Inc.	1971	1342	1629	1143	3838	2696

receipt of NIT seems unrelated to the absence of the male head or, strangely, even to family size.

Extensive receipt of NIT payments appears to be a function, especially in the 75-30, 100-50, and 125-50 plans, both of low and relatively stable family income. The low family income usually is the sum of the low earnings of the male and female spouse; and the stability of this low income is apparent in the low mean standard deviation of family non-transfer income. From data not shown here, we know that under the most generous NIT treatments, the 75-30, 100-50, and 125-50, those who were most dependent on NIT payments were employed in as many periods as were the less dependent. At first blush, then, dependence on NIT arose both from steadily low earnings as well as from employment interruptions. From the data in Tables III-6 and 7, we may infer that extensive dependence on both AFDC-UF (or AFDC) and NIT is related to low income, which results from low earnings; but that family breakdowns were an additional factor related to extensive receipt of AFDC per se.

Male-headed families with children in the Michigan sample had the option of going on AFDC-UF, AFDC or General Assistance, depending both on the state in which they resided and on their health. Female-headed families with children typically could enter AFDC. Childless families generally are eligible for programs that aid those with serious disabilities.[8] In all three types of families greater dependence on cash welfare should be related to the prevalence of disabilities and disfigurements, family size, and family income. As can be seen from the data in Table III-8, all three factors are associated with extensive dependence. As in the Wisconsin sample, heavy dependence on welfare also is associated with regularly low income, the average standard deviation of family non-welfare income being almost uniformly lower for all three family types with lower family incomes. Data not presented here indicate that for the Michigan as well as for the Wisconsin samples, the correlates of welfare dependence were equally in evidence whether families were grouped by their average transfer payments or by the number of periods in which they were on the programs.

Though variables such as the health of the family head, family size, and family income all influence the extent of dependence on welfare programs, the impact of variations in program parameters shines through in Tables III-6, 7, and 8. Consider first the program parameters in the Wisconsin study and how, given family incomes, they affect mean NIT payments. Were one to determine the payments that a family would receive under the alternative plans if its quarterly income were at levels like those shown in Table III-7, one could compare the relative generosity of the plans. Take, for example, a family of four covered by the 100-50 NIT plan. Its quarterly guarantee in the first year of the experiment was 100 percent of the quarterly poverty line, which at that time was $825. Its quarterly benefits, W, are determined by the formula: W = 825 - .50 (Family Income). Evaluated for illustrative purposes at $800 and then $1200 of quarterly family income, the various plans would offer:

Base Year Payments at Illustrative Quarterly Family Income for a Family of Four:

	$800	$1200
50-30	173	53
50-50	13	0
75-30	379	259
75-50	219	19
75-70	59	0
100-50	425	225
100-70	265	0
125-50	631	431

At these two levels of private income, the 75-30, 100-50, and 125-50 are the three most generous plans.

Now consider the fact that, with the exception of families in the 50-30 plan, mean family incomes in each of the experimental groups varied only between $1271 and $1465. In spite of the narrow band within which family incomes appear to lie across the experimental groups, the data in Table III-7 indicate that among the families in the more generous plans, there was a far greater proportion of families who were more heavily dependent on NIT payments. Clearly, what determined great welfare dependence, measured by average NIT payments received over time, were the guarantees and tax rates of the available programs

Program parameters also had a substantial influence on the degree of program "switching" observed in the various treatment groups. In Table III-9, we see again that the total number of periods spent receiving either AFDC-UF or NIT varied among treatment groups. But the data in this table also offer insight into the extent to which families in each group switched between the AFDC-UF and NIT programs. Clearly, families in the 50-50 and 75-70 groups, faced with relatively ungenerous NIT plans, availed themselves more frequently of the AFDC-UF (or AFDC) program.

With regard to the Michigan sample, the impact of program parameters on participation per se in cash welfare programs already has been noted. In Table III-8, we can observe that for a given type of family there is a greater concentration of families in the high payment category in the more generous states -- in spite of the fact that families who received welfare in the latter states had higher incomes than did their counterparts in the less generous states. Among the male-headed families in each of the four groups of states, the proportions in the higher payment (more dependent) category are 42, 60, 74, and 63 percent, as one proceeds from the low-guarantee low tax rate to the high guarantee-high tax rate states. Similarly, among female-headed families, the figures are 52, 79, 87, and 88 percent. If welfare dependence is measured by the amount of welfare payments received over time, then clearly program generosity affects dependence quite dramatically -- even without affecting non-welfare incomes.

TABLE III-9

The Ratio of Time Spent Receiving NIT Payments to the Time Spent Receiving Either NIT or AFDC-UF Payments

	50-30 (N=40)	50-50 (N=34)	75-30 (N=84)	75-50 (N=84)	75-70 (N=37)	100-50 (N=59)	100-70 (N=61)	125-50 (N=122)	Control (N=309)
Total Periods Either on AFDC-UF or on NIT	9.4	6.4	10.7	7.8	5.9	10.7	8.1	11.3	7.7
Percent of Transfer Time on NIT	76	62	85	83	65	87	87	94	--

Definition of Variable:

Percent of Transfer Time on NIT: This is calculated by taking a ratio for each family of the number of quarters during which it received some NIT payments to the total number of quarters during which it received either some NIT or AFDC-UF payments. The percents in the second row are the averages of these ratios for the individual families.

C. Work Experience During Periods on Income Transfer Programs

It is clear that most families who receive income transfers mix their receipt with labor income, at least serially. In part A of this chapter, we noted that 92 percent of the male heads in the Wisconsin sample worked some time during the three year experiment; and 96 percent of the male heads in the Michigan sample worked some time during the five year study period. Limiting attention to just those families who received AFDC-UF or NIT payments during the experiment, 89 percent had a male head who worked some of the time. Among the Michigan sample families that ever had a male head and ever received welfare payments, 90 percent had an employed male head at some time during the study. Also noted in part A is the extensive labor force participation over time of female heads: 77 percent of those women who always were female heads during the Michigan study worked at some time during the five years; while 66 percent of the female heads who ever received welfare also worked at some point during the five year study.

The focus in this part of the chapter is on the simultaneous receipt of transfers and earnings, and how that varies by transfer program, family type, and race. Neither the Wisconsin nor the Michigan data lend themselves perfectly to an analysis of this matter. Since a family in the Wisconsin study was recorded as being on AFDC-UF or NIT if it had a positive payment at any point during a given quarter, and as working if it had earnings in the last week of the quarter, we cannot be sure that transfers and earnings are received simultaneously. The problem is much more serious in the Michigan sample, since we have data on total payments and total earnings only for each of the five entire years. Our attention in this section, therefore, is directed mostly to a discussion of the Wisconsin data.

The data in Table III-10 indicate the extensive "simultaneous" receipt of transfers and earnings. Since only a small fraction of the sample ever received AFDC-UF during any of the 12 quarterly periods (see Table II-6), only a very small fraction of all families in the sample could simultaneously receive both AFDC-UF payments and earnings

TABLE III-10

Average Percentages Over Twelve Quarters of Male Heads Working and Receiving AFDC-UF or NIT Payments in Same Quarter, by Experimental Group

(Wisconsin Sample)

Experimental Groups	Those Working and Receiving NIT: As Pct. of Entire Group in Cell	Those Working and Receiving NIT: As Pct. of NIT Recipients in Cell	Those Working and Receiving AFDC-UF: As Pct. of Entire Group in Cell	Those Working and Receiving AFDC-UF: As Pct. of AFDC-UF Recipients in Cell
50-30 (N=42)	35	62	7	31
50-50 (N=52)	10	70	10	38
75-30 (N=85)	61	78	3	26
75-50 (N=92)	30	64	4	33
75-70 (N=61)	13	63	7	47
100-50 (N=61)	54	71	5	45
100-70 (N=66)	37	71	3	30
125-50 (N=125)	69	80	4	53
Control Group (N=309)	--	--	11	46

Definition of Variable:

Each set of two columns is similar. In the first column and first row, for example, the figures are obtained by averaging over 12 quarters the ratios of those male-heads employed in the last week of a particular quarter to the total number of families in that experimental treatment group in that quarter. In the second column, the figures are obtained in a similar manner, but the denominator in each period is the number of families which are receiving NIT payments.

from their male heads. In column 4 of Table III-10, we see, though, that over one-third of all families actually receiving AFDC-UF (or AFDC) payments also received labor income in the same quarter. Column 2 shows twice that fraction, or nearly two-thirds, of all families receiving NIT payments simultaneously received earnings. Since full-time workers are ineligible for AFDC-UF and most work is full-time, the figures in column 4 probably overstate simultaneous receipt of AFDC-UF and earnings. The incompatibility of full-time work and receipt of AFDC-UF is brought out by Table III-11, where we relate mean hours worked to periods on NIT and AFDC-UF. There we see that male workers were not likely to receive AFDC-UF if, on the average over the course of each of the last weeks of the experimental quarters, they worked 31 hours or more per week. The receipt of AFDC-UF for any substantial number of periods was very uncommon among Black and Spanish-surnamed full-time workers. Among whites and where it did occur in the two other groups, its receipt may have ended before the work began (though both occurred in the same quarter). Still, though, substantial numbers of workers frequently appear to mix work and AFDC-UF.

We can observe the impact of program characteristics on the simultaneous receipt of transfers and earnings in Table III-12. The first set of three columns compares the average of mean earnings in families receiving NIT payments for different numbers of quarters. We note that such earnings do not necessarily decline with the number of periods on the program. Consider average quarterly earnings for the families under the 125-50 plan. The 32 families who received NIT benefits for from 1 to 11 of the 12 periods had average earnings of $1040; whereas the 90 families who had NIT benefits in all 12 periods had average quarterly earnings of $1071. Table III-13 is organized just as Table III-12 except that it presents information on the standard deviation of earnings. As can be seen in its first three columns, what distinguishes permanent NIT ("welfare") recipients from those who move on and off the NIT program is not their average <u>level</u> of earnings, but rather their average

TABLE III-11

Percentage Distribution of Male Heads by Mean Weekly Hours Worked, by Number of Periods on AFDC-UF or NIT, by Race

(Wisconsin Sample)

A. Whites

Average Number of Hours Worked of Male Head	No. of Periods Receiving NIT					No. of Periods Receiving AFDC-UF				
	0	1-6	7-11	12	Total	0	1-6	7-11	12	Total
0 (N=32)	56	22	6	15	100	59	3	19	19	100
1-20 (N=56)	39	23	11	27	100	50	18	25	7	100
21-30 (N=56)	48	20	7	25	100	59	16	14	11	100
31-40 (N=134)	57	13	10	21	100	78	10	5	7	100
41+ (N=102)	54	15	9	23	100	83	7	3	7	100

B. Black

Average Number of Hours Worked of Male Head	NIT 0	NIT 1-6	NIT 7-11	NIT 12	NIT Total	AFDC-UF 0	AFDC-UF 1-6	AFDC-UF 7-11	AFDC-UF 12	AFDC-UF Total
0 (N=49)	35	35	10	20	100	51	16	29	4	100
1-20 (N=72)	32	32	18	18	100	43	29	25	3	100
21-30 (N=44)	32	32	23	14	100	68	25	7	0	100
31-40 (N=105)	39	16	13	31	100	85	13	2	0	100
41+ (N=55)	28	26	11	26	100	98	0	2	0	100

C. Spanish-Surname

Average Number of Hours Worked of Male Head	NIT 0	NIT 1-6	NIT 7-11	NIT 12	NIT Total	AFDC-UF 0	AFDC-UF 1-6	AFDC-UF 7-11	AFDC-UF 12	AFDC-UF Total
0 (N=14)	7	50	14	29	100	36	14	43	7	100
1-20 (N=32)	22	47	19	13	100	31	34	28	6	100
21-30 (N=22	23	18	41	18	100	55	32	9	5	100
31-40 (N=91)	36	25	14	24	100	84	11	4	1	100
41+ (N=30)	40	23	13	23	100	87	13	0	0	100

Note: The average of weekly hours worked is calculated for each individual over the 12 experimental quarters.

TABLE III-12

Mean Quarterly Earnings Over Time Of Male Heads, by Number of Periods Receiving Income Transfers and by Experimental Group

(Wisconsin Sample)

Experimental Group	No of Periods Receiving NIT			No. of Periods Receiving AFDC-UE		
	0	1-11	12	0	1-11	12
50-30 (N=42)	979 (2)[a]	949 (28)	719 (12)	893 (26)	856 (14)	979 (2)
50-50 (N=52)	1141 (18)	943 (34)	0 (0)	1188 (30)	743 (20)	1062 (2)
75-30 (N=85)	161 (1)	939 (42)	1102 (42)	1089 (63)	786 (22)	0 (0)
75-50 (N=92)	1452 (8)	1089 (71)	538 (13)	1096 (65)	898 (25)	1113 (2)
75-70 (N=61)	1167 (24)	841 (36)	1458 (1)	1066 (40)	852 (19)	440 (2)
100-50 (N=61)	1503 (2)	935 (30)	928 (29)	1002 (45)	796 (15)	927 (1)
100-70 (N=66)	1355 (5)	1088 (47)	752 (14)	1115 (49)	862 (16)	0 (1)
125-50 (N=125)	1122 (3)	1040 (32)	1071 (90)	1111 (112)	617 (11)	923 (2)
Control (N=309)	973 (309)	---	---	1119 (197)	736 (83)	659 (29)

Note:

a. 2 is the number of people in this group, i.e., in the 50-30 group and never on NIT. Earnings are not deflated by a price index.

TABLE III-13

Standard Deviation of Earnings of Male Heads, by Number of Periods Receiving Income Transfers and by Experimental Group

(Wisconsin Sample)

Experimental Group	No. of Periods Receiving NIT			No. of Periods Receiving AFDC-UF		
	0	1-11	12	0	1-11	12
50-30 (N=39)	23 (2)	336 (27)	324 (10)	346 (24)	310 (13)	23 (2)
50-50 (N=49)	368 (17)	422 (32)	0 (0)	402 (29)	428 (18)	182 (2)
75-30 (N=79)	355 (1)	374 (39)	368 (39)	385 (58)	332 (21)	0 (0)
75-50 (N=85)	344 (8)	464 (66)	297 (11)	416 (61)	477 (22)	400 (2)
75-70 (N=58)	391 (24)	404 (33)	132 (1)	374 (37)	469 (19)	56 (2)
100-50 (N=58)	503 (2)	439 (27)	404 (29)	442 (44)	373 (13)	292 (1)
100-70 (N=63)	406 (4)	422 (46)	354 (13)	389 (47)	459 (16)	0 (0)
125-50 (N=120)	289 (3)	417 (29)	353 (88)	376 (108)	261 (10)	399 (2)
Control (N=290)	360 (290)	0 (0)	0 (0)	335 (188)	412 (76)	388 (26)

Note:

Earnings are not deflated by a price index.

variability. These findings hold in all of the more generous NIT plans. Under AFDC-UF, long-term receipt of benefits is associated with a drop in average earnings. This contrast is a simple consequence of the high guarantees and low tax rates in some of the NIT plans, coupled with the absence of an hours test, like that in the AFDC-UF program, for continued eligibility. Clearly, while the simultaneous receipt of transfers and earnings may occur with some frequency under existing programs, under a universal NIT program with generous guarantees and moderate tax rates it would be the rule.

Recalling that the Michigan data do not allow for an investigation of the incidence of simultaneous receipt of transfers and earnings, we examine briefly Table III-14 to see how "within-year" receipt of both varied over time, as well as by family type, race, and potential AFDC program.

Parts A and B of Table III-14 compare work and welfare patterns in 1967 and 1971, the first and last years for which the Michigan Panel Study obtained income data. In 1967, 24 percent of the families in the sample received some cash welfare payments. Of the latter, 39 percent also had some earnings during the year. Five years later, 29 percent of the identical group of families received some welfare aid.[9] In this group, 44 percent had some earnings during the year. These data are consistent with the general impressions that welfare participation rates have risen; and that work while on welfare has become only slightly more common since the implementation of the 1967 Amendments to the Social Security Act (see Table II-1).

Parts C and D of Table III-14 compare work and welfare pattens of white and non-white male-headed families, while parts E and F make the same comparison for female-headed families, all for 1971. The previous studies, discussed in part A of Chapter II, suggest that between 40 and 50 percent of female heads of families which receive AFDC also work at some time during the year. In contrast, the data in Table III-14, parts E and F, offer an estimate closer to one-third of the group.

TABLE III - 14

Incidence of Receipt Within One Year of Income Transfers and Earnings, Over Time, By Family Type, Race, and AFDC Program

(Michigan Sample)

A. All Families Receiving Welfare During 1967

Annual Cash Welfare Payments($)	Percent of Families With Annual Family Earnings($) 0	1-1000	1001-2000	2001+
1-100 (N=90)	28	24	21	27
1001-2000 (N=129)	64	24	7	5
2001-3000 (N=94)	71	17	6	5
3001 + (N=61)	75	16	8	3

All Families in Sample : 1621
Families Receiving Welfare : 397
Families With Welfare & Earnings: 153

B. All Families Receiving Welfare During 1971

Annual Cash Welfare Payments($)	Percent of Families With Annual Family Earnings($) 0	1-1000	1001-2000	2000+
1-1000 (N=101)	41	12	14	34
1001-2000 (N=137)	54	21	4	21
2001-3000 (N=100)	58	19	4	19
3001 + (N=128)	70	17	4	8

All Families in Sample :1635
Families Receiving Welfare :466
Families With Welfare & Earnings :204

C. Male-Headed White Families Receiving Welfare During 1971

	0	1-1000	1001 +
1-1000 (N=12)	50	0	50
1001-2000 (N=7)	43	29	29
2001 + (N=8)	38	25	38

All Male-Headed White Families in Sample :351
Such Families Receiving Welfare :27
Such Families Receiving Welfare and Earnings :15

D. Male-Headed Non-White Families Receiving Welfare During 1971

	0	1-1000	1001 +
1-1000 (N=21)	29	14	57
1001-2000 (N=24)	29	25	46
2001 + (N=23)	48	13	39

All Male-Headed Non White Families in Sample : 472
Such Families Receiving Welfare: 68
Such Families Receiving Welfare and Earnings : 44

E. Female-Headed White Families Receiving Welfare During 1971

	0	1-1000	1001 +
1-1000 (N=5)	80	20	0
1001-2000 (N=14)	79	21	0
2001 + (N=18)	67	22	11

All Female-Headed White Families in Sample : 122
Such Families Receiving Welfare : 37
Such Families Receiving Welfare and Earnings : 10

F. Female-Headed Non-White Families Receiving Welfare During 1971

	0	1-1000	1001 +
1-1000 (N=51)	41	12	47
1001-2000 (N=71)	63	23	14
2001 + (N=130)	73	18	9

All Female-Headed Non-White Families in Sample : 450
Such Families Receiving Welfare : 252
Such Families Receiving Welfare and Earnings : 91

On the other hand, among male heads of low income families which receive welfare, previous data have suggested that under 50 percent receive welfare and work within one year, while the Michigan study sample data suggest an estimate closer to 60 percent. The reader should note that data for families with different heads during the five year study are not shown in Table III-14; but, as a group, they combine welfare and labor income less commonly than continnously male-headed families and more commonly than continuously female-headed families.

Besides indicating that the combining of welfare and labor incomes is more frequent in male-headed than in female-headed and in non-white rather than white families, the data in Table III-14 also show that male-headed (as opposed to female-headed) and non-white (as opposed to white) families have greater earnings during their years on welfare. Note also that perhaps two-thirds of female-headed families combining work and welfare, either serially or simultaneously, earn less than $1,000 during such years. Data similar to that just discussed,,but not presented in Table III-14 reveal that, as one would expect, welfare participation rates are higher in the two groups of high benefit states than in the low benefit states, while the combining of work and welfare is more common in the low than in the high benefit states.

D. Conclusion

As background for the later analyses of work and welfare experience in our two samples, this chapter has summarized the work and welfare data and has suggested explanations for the observed behavior. The major points with respect to work, welfare, and work while on welfare experience are these:

1. Overwhelmingly, male heads of families in both samples work at least some of the time. Among men with their families, the major reason for non-participation in the labor force is bad health. Remarkably, in the Michigan sample, we found that 77 percent of those who always were female heads of families worked at some time during the five year study period.

2. From a tabular analysis, we found that low earnings among male heads

results typically from the low earners working at low wage rates and experiencing great job instability. These men seem to experience periodic unemployment as they go from one job to another. When they work, it is usually at a full-time job.

3. Whether prople ever go on welfare depends both on their income and on the programs which they face. Comparing those ever going with those never going on AFDC-UF, the differences in family income are great. Comparing those who ever received with those who never received NIT payments, the difference in family income is small. Families with lower incomes are more likely to go on welfare under both AFDC-UF and NIT but the AFDC-UF rules require far greater drops in income to establish eligibility. Examination of the Michigan data also suggests the powerful effect of program structure on welfare participation: even though families generally have higher incomes in the more (welfare) generous states, they are much more likely than families in the less generous states ever to participate in welfare.

4. Like the fact of participation itself, the extent of welfare dependence depends on family income and program structure. Family structure also affects the extent of dependence. A most interesting feature of the Wisconsin data is that they allow the analyst to examine how program parameters affect the degree of welfare dependence, measured by the amount of payments received over time, among families with the same income.

5. The simultaneous receipt of welfare and earnings varies, again, by program. In the Wisconsin data, "simultaneous" receipt is extensive and twice as common under the NIT as under the AFDC-UF program. While the simultaneous receipt of transfers and transfers may occur with some frequency under existing programs, under a universal NIT program with generous guarantees and moderate tax rates it would be the rule.

CHAPTER III

Footnotes

1. We argue later that the earnings of workers can be explained by a linear model and thus easily are subjected to statistical analysis. However, the behavior of non-workers is not explained by the same linear relationship. Similarly, transfer payments of recipients can be explained by a linear relationship which does not hold for non-recipients.

2. The reader ought to be mindful of the fact that our discussion here relates to the data made available to us in the fall of 1973. The experiment generated other data which may be "cleaned up" and released at other times. Also, some data exist only for certain sub-groups of the combined sample. For example, monthly data on AFDC payments are available for families in the eight treatment groups, but not for families in the control group. Our discussion of income transfer experience pertains only to data available for families in all groups.

3. Families were not allowed to receive both AFDC-UF (or AFDC) and NIT payments simultaneously. When they did, in general, it was fraudulent. Some households did have a member receiving Old Age Assistance or Aid to the Blind while others received NIT payments.

4. Robert Hall, "Turnover in the Labor Force," Brookings Papers on Economic Activity, Vol. 3, 1972, Table 1, p. 16.

5. Vera C. Perrella, "Work Experience of the Population," Monthly Labor Review, Vol. 93, February 1970, Table 2, p. 57.

6. Data for a sixth and seventh year were compiled and made available to the public while this study was in process.

7. The groups of states are:
LOW GUARANTEE-LOW TAX RATE: Alaska, Arizona, Arkansas, Florida, Georgia, Indiana, Maine, Miss., Missouri, Nevada, New Mexico, South Carolina, Tennessee, Vermont, Virginia.
LOW GUARANTEE-HIGH TAX RATE: Alabama, Colorado, D.C., Illinois, Iowa, Kentucky, Louisiana, Maryland, Nebraska, North Carolina, Ohio, Oklahoma, Texas, W. Virginia.
HIGH GUARANTEE-LOW TAX RATE: California, Delware, Washington.
HIGH GUARANTEE-HIGH TAX RATE: Connecticut, Hawaii, Idaho, Kansas, Mass., Michigan, Minnesota, Montana, Hew Hampshire, New Jersey, New York, North Dakota, South Dakota, Utah, Wisconsin, Wyoming.

8. Recall that families whose heads were 60 or over in the first year of the Panel Study were eliminated from the sample. Therefore, outside of General Assistance, AFDC, and AFDC-UF, families in our sample should have been eligible only for Aid to the Blind and Permanently and Totally Disabled. In effect, all welfare payments from any of these sources were lumped together in the Michigan data. Efforts were made by the Michigan staff to distinguish AFDC from other welfare payments, but they were totally unsuccessful. Moreover, in the first two years of the study, non-AFDC welfare payments were printed on the Michigan data tape, not in

CHAPTER III

Footnotes

8. continued

single dollar amounts, but rather in bracketed quantities, like $1-1000, etc. Through a very complicated procedure, we converted these bracketed amounts of "other welfare" payments to estimates of actual dollar amounts; and the combined the latter with the dollar amount of AFDC payments. Therefore, the sum of AFDC and these "other welfare" payments in each of the five years is the amount of welfare to which we refer in all of our work with Michigan study sample families who received welfare.

9. Moreover, if welfare participation rates in the low income population appear low in Table III-14, it is because our sample includes families who have been in the bottom 20 percent of the income distribution in any of the five years of the study. The poverty population, which normally comprises the denominator in measures of welfare participation rates, has averaged roughly 12 to 14 percent of the income distribution in recent years.

CHAPTER IV

Earnings Patterns and Welfare Patterns: A Case History Approach

Prior to presenting the statistical analyses of earnings patterns in Chapter VI and of welfare patterns in Chapter VII, this chapter offers a "case history" approach to the study of work behavior and welfare experience over time in the Wisconsin sample. Because we have neither week-by-week employment histories nor, as noted, stated reasons for movements in and out of the labor force, unemployment, and jobs, we are not actually offering "case histories." We only mean to imply by use of the term that we have traced for each individual that part of his employment history on which we have information; and then have grouped these individual histories into several types of "patterns." By discussing these patterns, we hope to add to the picture of work histories later characterized only by the mean and standard deviation of earnings.

As a complement to the later analysis of the variability in earnings through time, we begin by assigning the male heads of families to one of six groups, each characterized by a different type of earnings pattern. These are distinguished from each other on the basis of casual examination of plots of earnings for individual male heads. Then we attempt to find individual characteristics that are associated with the differences in earnings patterns, before examining in closer detail how work experience varies among the earnings pattern groups. Similarly with welfare experience, our purpose is to complement the statistical analysis of variations among families in income transfer payments by assigning each family to one of four groups based on types of welfare pattern, again distinguished from each other by visual examination of plots of NIT and AFDC-UF payments. In this analysis of welfare patterns, we seek to complete the picture of how welfare experiences differ among families. The chapter concludes with a brief discussion of the job histories of the male heads, thus providing the reader with an understanding of the occupational and industrial mobility associated with changes in earnings.

Earnings patterns were distinguished after plotting quarterly earnings for each of the 12 experimental periods for a very large fraction of the male heads in the Wisconsin sample.[1] Six types of patterns were distinguished and then assignments of males to the six groups were made. The patterns, the labels given to them, and the numbers of male heads assigned to them are:

Earnings Patterns	Number in Group	Mean Quarterly Earnings ($)	Mean Std. Dev. of Earnings[2] ($)
Stable Low	62	144	191
Unstable Low	279	879	551
Stable High	150	1232	140
Unstable High	193	1469	374
High, 1 Dip	90	1151	384
Missing Data	116	560	355

A person had a Stable Low pattern if he never worked -- non workers amounted to 50 percent of the 62 cases in this group -- or had regularly low earnings. Thus, it turned out that even among the 31 workers in the Stable Low group mean quarterly earnings averaged only $288. Males were assigned to the Unstable Low group if they were workers who had low but fluctuating earnings. As is shown above, this group had a higher mean than the Stable Low group but also had a much higher standard deviation of earnings on the average. Those assigned to the three high earnings groups are characterized by their relatively high mean earnings while those with the stable and unstable high earnings patterns are distinguished from each other by a significant difference in the standard deviation of their earnings. The reader should be mindful that our label "Unstable High" does not imply that workers in this category necessarily were in and out of jobs. One of the more interesting aspects of this chapter is an examination of how the fluctuations in earnings arose in this category. Lastly, males assigned to the Missing Data group largely were persons who separated from their families early in the experiment. Nearly 40 percent his group left their families between the time of selection and the start of the experiment. Ninety-four percent of this group was with their families for 6 or fewer of the 12 periods.

Welfare patterns were distinguished after plotting NIT and AFDC-UF payments for a very large fraction of the families in the study sample for each of the 12 experimental periods.[3] Four types of patterns were distinguished and then assignments of the families to the four groups were made. The patterns, the labels given to them, and the numbers in each group are:

Welfare Patterns	Number in Each Group	Mean Quarterly Transfer Payment ($)	Mean Standard Deviation of Transfers ($)
Stable Low	433	70	37
Low 1 Jump	63	190	199
Stable High	60	694	81
Unstable High	334	545	260

Once again, we can observe that our assignment by visual judgment of families to the welfare patterns groups is consistent with the data on average payments and the variability of payments. The two low groups have lower means than the two high groups. Moreover, families in the two low groups differ from each other by the degree of variability in their transfer payments as do those in the two high groups. Thus, we can proceed to determine the factors associated both with earnings and welfare patterns as they have been defined here.

The reader may want to pause, however to examine Figures 1-4, where the earnings pattern and other characteristics of four different male heads are plotted, each chosen as a typical example of a different earning pattern. Associated with the male heads' earnings patterns are their families' welfare patterns.

FIGURE 1

KEY

Family Size. . . . . .
Jobs Held . . . . . .
Unemployment IB
Male Earnings
Family Non-Transfer Income
NIT and AFDC-UF
NIT

EARNINGS PATTERN: STABLE LOW
WELFARE PATTERN : STABLE HIGH

(For a family with a Spanish-Surname male head, 31 years of age, 12 years of education.)

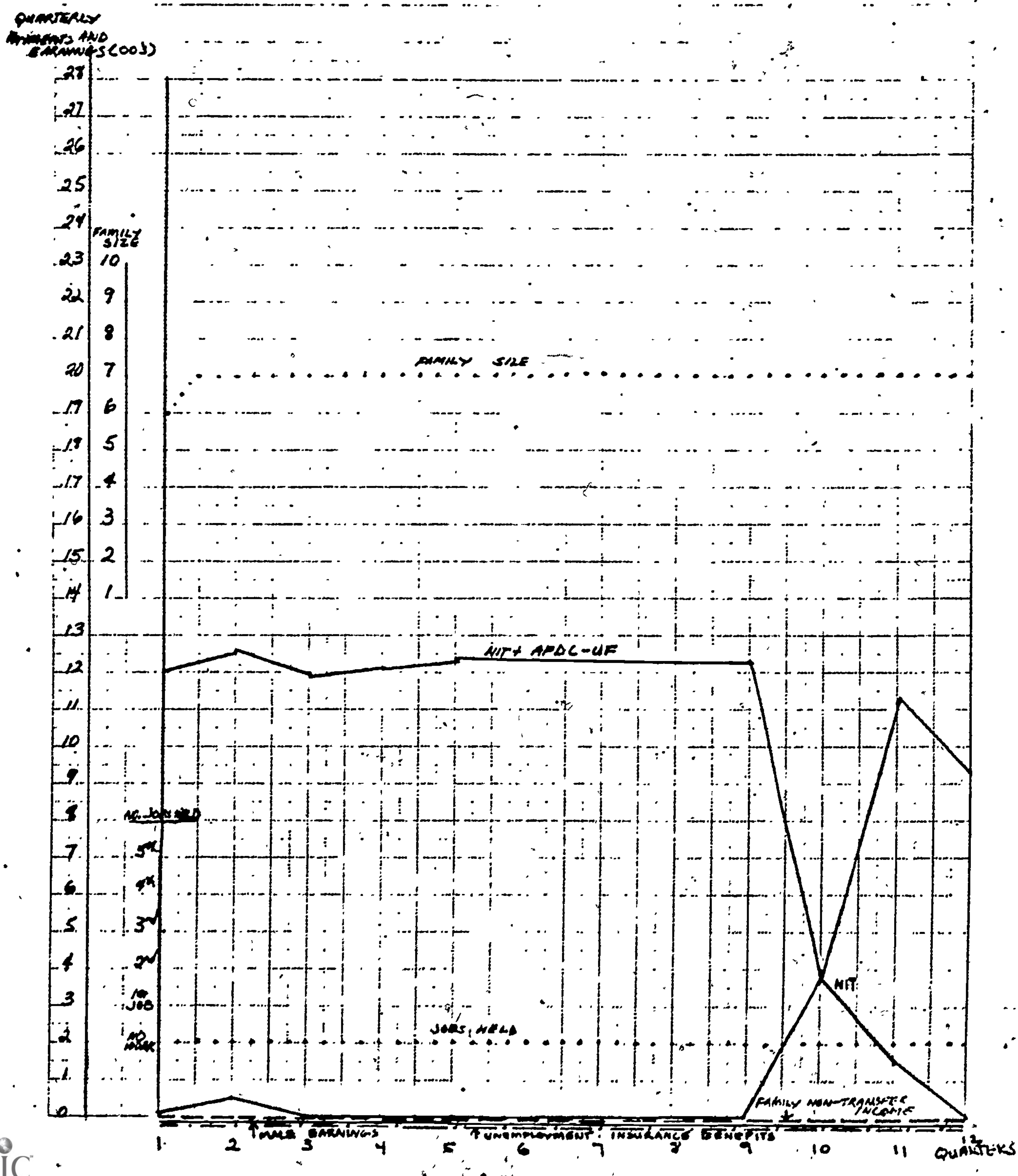


FIGURE 2

EARNINGS PATTERN: UNSTABLE LOW
WELFARE PATTERN : STABLE LOW

(For a family with a white, male head of 26 years of age, 8 years of education.)

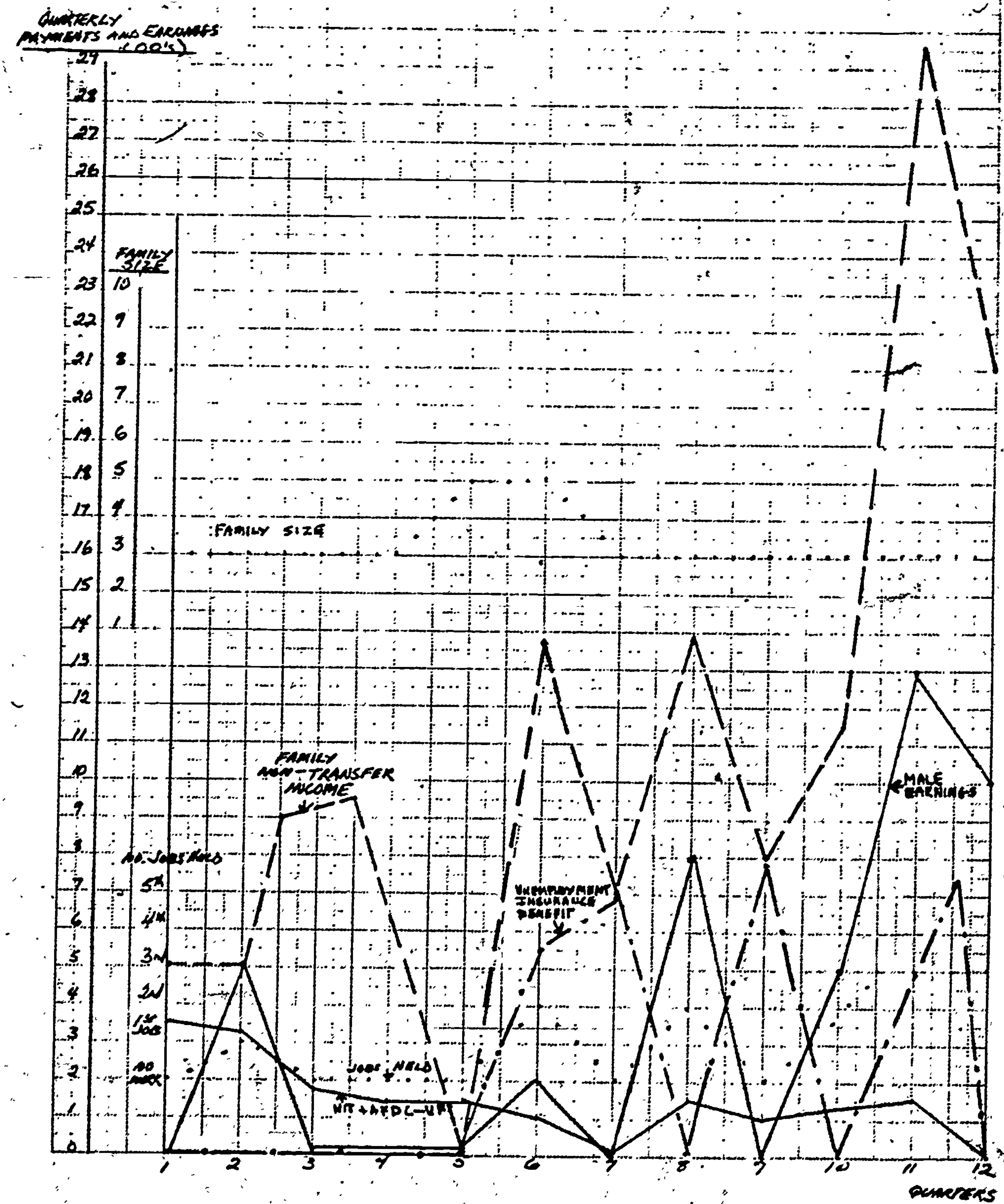


FIGURE 3

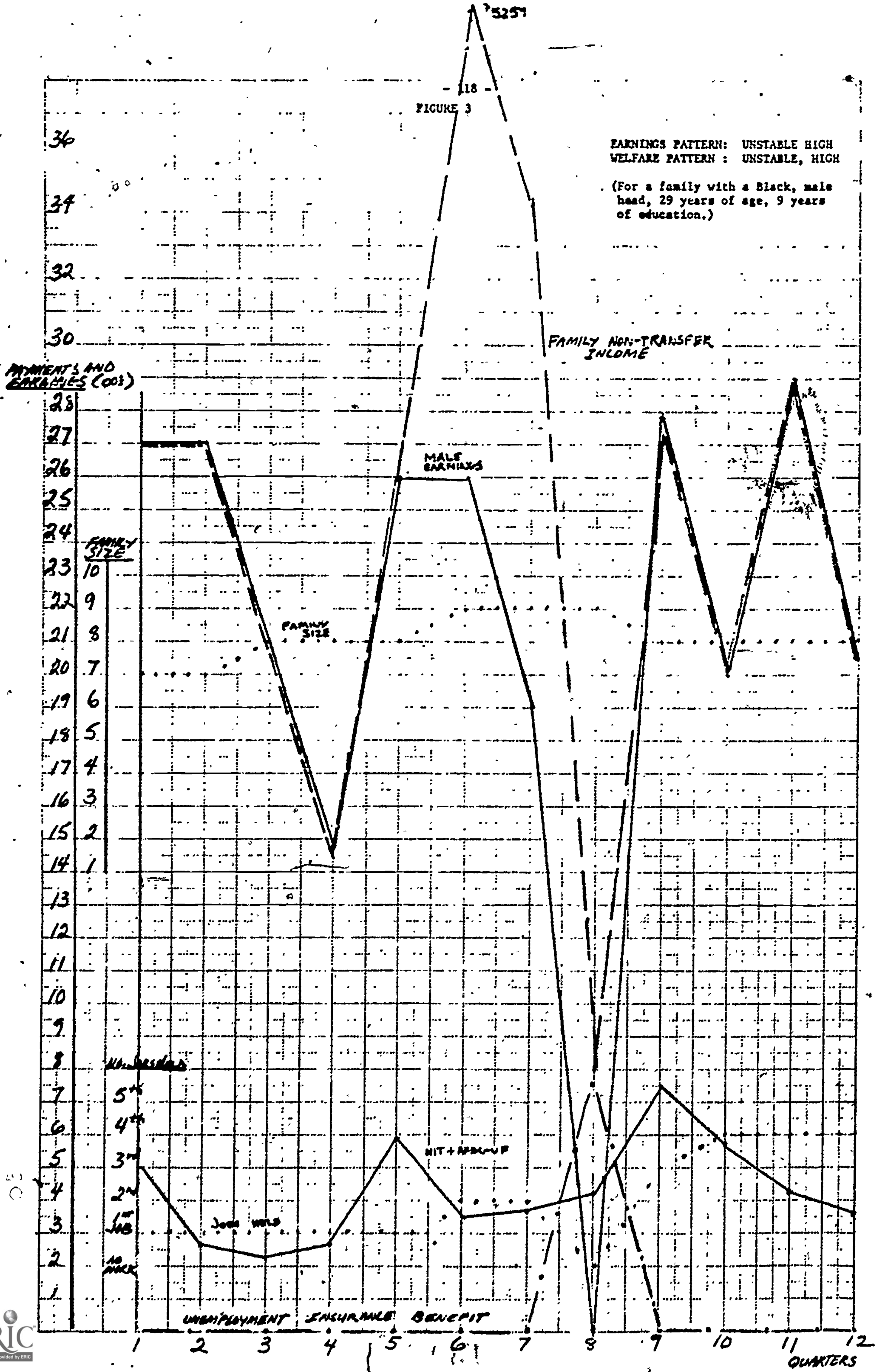
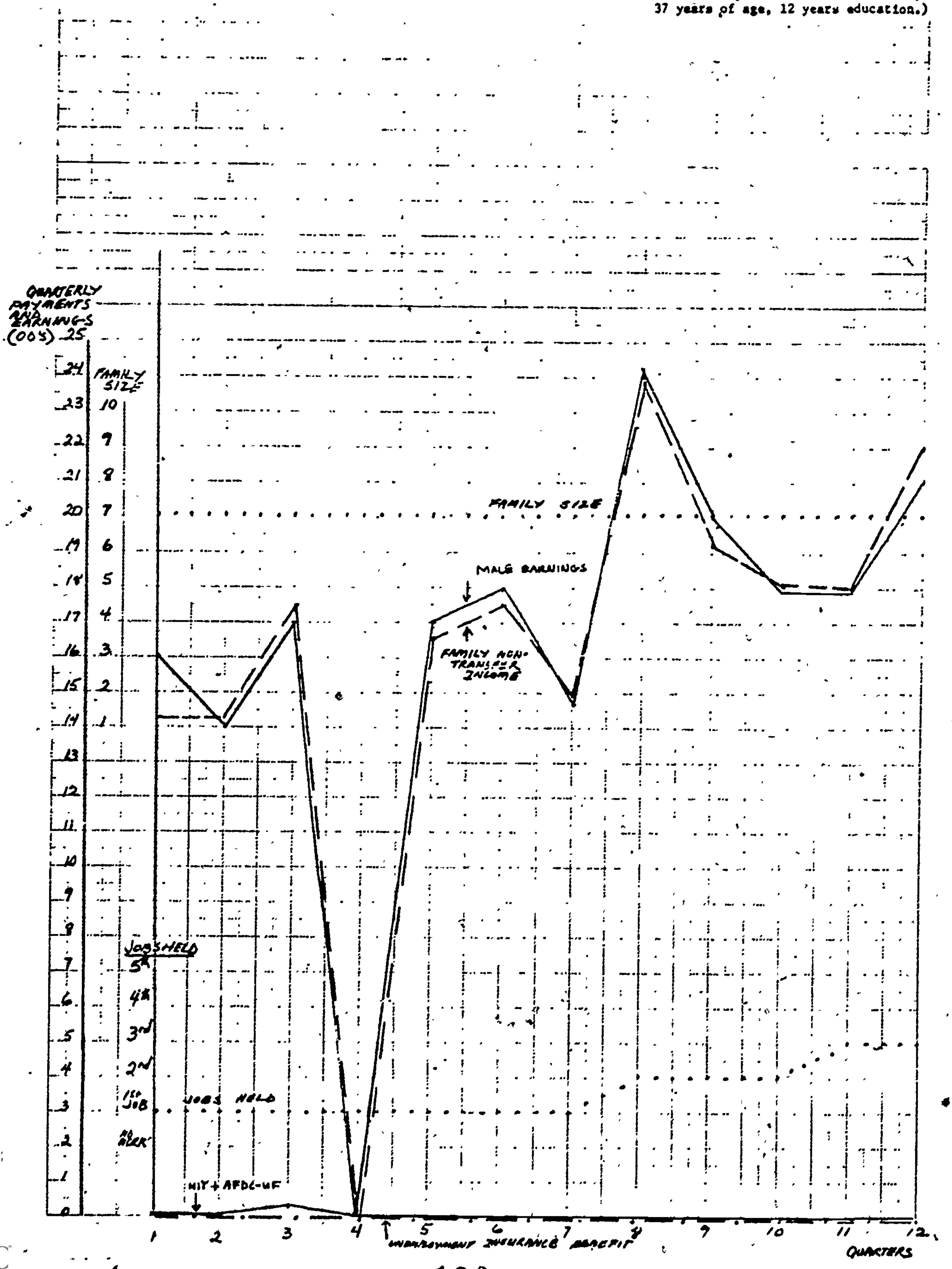

EARNINGS PATTERN: UNSTABLE HIGH
WELFARE PATTERN : UNSTABLE, HIGH
(For a family with a Black, male head, 29 years of age, 9 years of education.)
$5257
FAMILY NON-TRANSFER INCOME
PAYMENTS AND EARNINGS (00's)
MALE EARNINGS
FAMILY SIZE
UNEMPLOYMENT INSURANCE BENEFIT
QUARTERS

FIGURE 4

EARNINGS PATTERN: HIGH, 1 DIP
WELFARE PATTERN : STABLE LOW

(For a family with a white male head, 37 years of age, 12 years education.)



A. Work Experience and Other Characteristics of Males With Different Earnings Patterns

1. Demographic Characteristics of Earnings Groups

Table IV -1 displays the demographic factors associated with the six earnings patterns. The data there indicate some discernable differences between the people who experience the various work patterns. Males in the Stable Low group, for example, generally are older, have slightly smaller families with fewer young children, and are less educated than those who receive higher earnings. The higher incidence of formal job training in this group may indicate that they were attracted by a government training program which does not seem to have gotten them out of poverty. As noted, a majority of the men in this group always are absent from the labor force, their absence apparently associated with the extensive prevalence of poor health in this group. Thus, the Stable Low group can be considered the chronically poor, handicapped both by ill health and limited formal education.

While the chronically low earners are slightly older and white, those in the Missing Data pattern are somewhat younger and largely Black. Disproportionately, then, in this sample young Blacks with limited education left their families, typically at an early stage of the experiment. Their families, while not as large as those in the other groups, averaged 5 persons at the time of their departure.

Among the remaining four earnings pattern groups in Table IV-1 it is more difficult to find distinguishing characteristics. High earners stay with their families slightly more than low earners, and also have somewhat larger families. Also, those in the unstable groups are slightly younger than those with the stable patterns. Higher average instability in earnings in this group simply may reflect attempts by young men to improve their position in the labor market. As will be clear when we review the data on their work experience, the insufficiency of income among the high earners which results in dependency relates as much to their large families as to their earnings levels or patterns.

TABLE IV-1

Characteristics and Employment Experience of Male Heads
By Earnings Pattern

(Wisconsin Sample)

Characteristics	Earnings Pattern (1) Stable Low (N=62)	(2) Unstable Low (N=279)	(3) Stable High (N=150)	(4) Unstable High (N=193)	(5) High 1 Dip (N=90)	(6) Missing Data (N=116)
Mean No. Pds. Prsnt(H)	10.9	11.4	11.9	11.8	11.8	2.6
Mean Family Size	5.6	6.0	6.4	6.3	6.0	4.9
Pct. With Children < 5	60	80	78	81	73	71
Pct.Black & Span.Surn.	47	59	55	49	53	78
Mean Age (H)	41	36	38	36	39	34
Pct.Unhealthy(H)	81	66	41	40	56	29
Pct.H.S. Graduates(H)	15	18	25	31	18	13
Pct. With Training(H)	29	24	22	29	21	.16
Mean No.Pds.Empl.(H)	2.0	8.8	11.9	11.6	11.2	1.8
Mean No.Pds.NLF(H)	7.0	.9	0	.1	.1	.4
Mean No.Pds.Unempl.(H)	1.8	1.6	0	.1	.4	.3
Mean Wkly.Hrs.Wrkd.(H)	4.6	25.7	40.3	40.4	35.5	5.1
Mean Hrly.Wage Rate(H)	3.69	3.79	3.75	3.87	3.75	4.12
Mean Qtly.Earnings(H)	144	879	1232	1469	1151	560
Mean Std.Dev.Earnings(H)	191	551	140	374	384	354
Pct.With 1 Job(H)	39	35	81	63	62	36
Pct.With 3+ Jobs(H)	3	40	6	18	19	15
Mean Qtly.Earnings(S)	183	89	84	77	85	208
Mean Fam.Non-Trnsf.Inc.	351	276	231	212	251	197

NOTES

The first eight variables listed in this table are defined in the notes to Table III-1. Eight of the remaining eleven variables are defined in the notes to Table III-2. The other three variables are defined here.

Definitions of Variables

1. Mean No. Pds. NLF(H): The mean number of quarters during which persons in group were not in the labor force in the last week of the quarter.

2. Mean No. Pds. Unempl. (H): The mean number of quarters during which persons in the group were unemployed in the last week of the quarter.

3. Pct. With 1 Job (H)): The percent of persons in the group who had 1 and only 1 job during the course of the study period.

2. Work Patterns of Different Groups

A number of questions remain about work experience. Do the male heads move in and out of the labor force frequently? Or does instability arise from movements in and out of unemployment? When employed, do the men work part-time or full-time? Are differences in earnings patterns associated with differences in wage rates? Clearly, differences in the pattern of earnings can arise in a number of ways, which we now seek to identify.

Beginning with the Stable Low group again, which makes up 7 percent of the total group, we note from Table IV-1, that this pattern primarily is associated with extended absence from the labor force and secondarily is related to the high incidence of unemployment. In the group as a whole, 60 percent of the periods in which the men were with their families were spent out of the labor force. This absence from the labor force was concentrated, undoubtedly, among the 30 men who never worked during the three years. But while the men in this entire group spent three-fifths of their time out of the labor force, another one-sixth of their total time was spent being unemployed. They were unemployed for 40 percent of the time during which they were in the labor force. While these men averaged only 4.4 hours worked during the last weeks of each of the 12 experimental periods, they averaged roughly 30 hours of work when they were employed. Thus, only a few of them were part-time workers when employed. And lastly, we note in Table IV-1 that the average hourly wage rate of persons in the Stable Low group was only slightly below that for the other groups. Stable Low workers, then, are chronically poor either because their health keeps them out of the labor force or because they experience unusually excessive unemployment when they are in the labor force.

By contrast with the Stable Low group, males in the Unstable Low group, constituting just over 30 percent of the entire sample, experienced low but highly variable earnings only secondarily because of excessive absence from the labor force. Their primary problem was high unemployment, amounting to a substantial 14 percent of the periods during which they were with their families. Absence from the labor force equaled 8 percent of their periods at home. Again in this group, work largely was full-time when it existed, averaging 35 hours per week of work. What is especially noteworthy is that 40 percent of the males in this group had 3 or more jobs in a 36 month stretch. This large degree of job turnover occurred when the average length of a job for persons in the national labor force at that time was roughly 27 months.[4] So we have a second variety of work experience, one involving periodic unemployment and very frequent job changes. These workers do not seem to return to their initial jobs when they become unemployed. Thus, the Unstable Low group may seem, at first glance, to contain the types of person who works in the "secondary labor market." But note that their average wage when working is about at the average for the entire sample. Their problem appears to be employment instability, not necessarily low wage rates.

Now we may consider the three high earnings pattern groups together. Any one of them constitutes a sizable part of the sample; all told, they form nearly half of the sample. In all three high earnings pattern groups, there was little absence from the labor force. In two of the three, unemployment of the head was virtually non-existent for periods which he was with his family, which typically was nearly the entire time of the experiment. Only in the High, 1 Dip group did unemployment rise to 3 percent of the periods. Quarterly earnings in these three groups basically were high because of continuous attachment to the labor force and because of full-time work. Most probably, the instability in the Unstable High group resulted from changes in the amount of overtime and moonlighting work, as well as from fairly frequent changes in jobs.

The high degree of regular work effort in two of the three high earnings patterns groups merits further exploration, especially to see whether there are other explanations for the instability in the Unstable High group. In that group, 57 percent of the 193 men averaged 41 hours or more of work per week during the three year study period. (In the Stable High group, 37 percent of the men worked so much so regularly.)

Thus, the Unstable High group contained the greatest proportion of excessive or "overemployed" workers. From this we infer that some of the variability in earnings in the Unstable High group results from the men working more or less overtime or from moving in and out of the moonlighting jobs.

It is of great interest to note that one-fifth of the men in all six earnings pattern groups combined averaged 41 hours or more of work during the experiment. In contrast to the small group of males who spend all of their time out of the labor force, there is a large group that spends all of its time working very hard. This group is young, relatively healthy, and relatively well educated. Their mean wage on the average is at the same level as the mean for other workers averaging fewer hours. Also, as a group they exhibit a markedly higher degree of attachment to their jobs -- although the overemployed just in the Unstable High group do exhibit a somewhat smaller degree of job attachment.

3. Earnings Patterns and Welfare Experience

Lastly, we may examine the relationship between earnings patterns and welfare experience. Table IV-2 contains the data on welfare experience for the men grouped by their earnings patterns. What is clear is that although the families of men in the low earnings patterns groups collect NIT payments more often and in higher amounts than the families of men in the high earnings patterns groups, the big difference between the high and low earnings groups in welfare experience emerges in the AFDC-UF (or AFDC) programs. It is only the non-workers or the fully unemployed who can get either AFDC-UF (or AFDC). Full unemployment being uncommon in any of the three high earnings groups, receipt of AFDC-UF (or AFDC) in those categories is relatively rare. When we examine the proportions of men in each earnings pattern group that fall into the various welfare pattern groups, we note that roughly tw-thirds of all the high earners are in the Stable Low welfare pattern group, whereas roughly two-thirds of the low earners are heavily dependent on welfare, regularly or irregularly.

TABLE IV-2

Welfare Experience and Welfare Patterns of Families of Male Heads, by Earnings Pattern

(Wisconsin Sample)

	Earnings Pattern					
	Stable Low (N=62)	Unstable Low (N=279)	Stable High (N=150)	Unstable High (N=193)	High 1 Dip (N=90)	Missing Data (N=116)
Mean No. Pds. on NIT	5.5	5.2	4.6	4.3	4.3	3.7
Mean No. Pds. on AFDC-UF	3.7	2.2	1.0	.8	1.2	4.5
Mean NIT Payment	420	255	231	180	200	144
Mean AFDC-UF Payment	299	172	53	50	90	360
Std. Dev. Total Trnsf.	162	190	65	74	95	210
Welfare Patterns Column Totals	100	100	100	100	100	100
Pct. Stable Low	24	36	66	67	64	28
Pct. Low, 1 Jump	5	9	4	5	10	9
Pct. Stable High	18	3	10	5	6	10
Pct. Unstable High	53	52	20	23	20	54

NOTES

Definition of Variables:

1. Mean No. Pds. on NIT (AFDC-UF or AFDC): This is the mean for the group of the number of quarters during the experiment in which the families of the male heads received some NIT (or AFDC-UF or AFDC) payment.

2. Mean NIT (AFDC-UF or AFDC) Payment: This is the mean for the group of the average NIT (AFDC-UF or AFDC) payment received by each family over the 12 quarters, including the quarters in which each received no NIT (AFDC-UF or AFDC) payment.

3. Std. Dev. Total Trnsf.: This is the mean for the group of the standard deviation for each family of the quarterly sums of NIT and AFDC-UF or AFDC payments, including periods in which each received neither NIT nor AFDC payments. In other words, for each family a standard deviation was calculated for its twelve quarterly transfer amounts. Then, a mean of those individual standard deviations was computed for each earnings pattern group. The mean standard deviation appears in the table.

B. Welfare Experience and Other Characteristics of Families with Different Welfare Patterns

The data in Table IV-3 reflect the characteristics of families and the work experience of their male heads when present in their original family units. As between the two groups receiving low and the two receiving high average welfare payments, we can observe a difference in average family size and in the presence of young children. The heavily dependent have large families and more frequently have young children. In contrast to the tabular analysis in Chapter III, Table IV-3 shows no relationship between absence of the male head and the welfare pattern of his family. Of course, we have not distinguished between AFDC and NIT payments in this chapter, while in Chapter III, we noted that families more heavily dependent on AFDC were more likely to be without their male heads; the degree of dependence on NIT was not associated with the presence of the male head. While family size and the presence of young children distinguish the less from the more dependent, bad health clearly seems to be associated with the stability of welfare dependence within both the less and more dependent welfare pattern groups. A family may have low income relative to its family size and composition, but a stretch of poor health may precipitate a change in the degree of dependence by interrupting the income flow that obtains. Bad health thus leads to unstable welfare patterns.

The interruption of income -- sometimes resulting from bad health and sometimes from unemployment -- which leads to a sudden increase in the degree of welfare dependence is reflected by the data on employment experience associated with the four welfare patterns in Table IV-3. Both for those who receive low payments and for those receiving high payments, those with unstable welfare histories have fewer periods of employment than those with stable welfare histories. Data not in Table IV-3 indicate that the periods of non-employment in each of the welfare pattern groups is split evenly between periods of unemployment and periods of non-participation in the labor force. It is also the case that, when they are employed, the men in all four columns work an average number of hours

TABLE IV-3

Characteristics, Employment, and Welfare Experience of Male Heads, By Welfare Pattern

(Wisconsin Sample)

	Welfare Patterns			
	(1) Stable Low (N=433)	(2) Low, 1 Jump (N=63)	(3) Stable High (N=60)	(4) Unstable High (N=334)
Mean No. Pds. Prsnt. (H)	11	10	10	10
Mean Family Size	5.6	5.8	6.8	6.4
Pct. With Children ≤ 5	71	68	87	83
Pct. Unhealthy (H)	42	56	48	64
Mean No. Pds. Empl. (H)	10.1	8.6	7.9	7.3
Mean Wkly. Hrs. Worked (H)	33	28	25	22
Mean Qtly. Earnings (H)	1326	1241	952	902
Std. Dev. Earnings (H)	353	464	245	422
Pct. With 3 + Jobs (H)	18	27	8	27
Mean Qtly. Earnings (FS)	152	153	62	92
Mean Non-Trnsf. Family Inc.	1688	1530	1079	1084
Std. Dev. Non-Transf. Inc.	504	627	333	557
Mean No. Pds. on NIT	2.9	3.6	8.7	6.4
Mean No. Pds. on AFDC-UF	.4	1.6	2.9	4.0
Mean NIT Payment	75	113	583	318
Mean AFDC-UF Payment	30	122	217	300
Std. Dev. Total Transfer	37	199	81	260

NOTES

Definition of Variables:

1. Mean Non-Trnsf. Family Inc.: This is the mean for the group of each family's averaged (undeflated) quarterly income, excluding its NIT and AFDC-UF payments but including all other components of cash income like earnings and UI benefits.

2. Std. Dev. Non-Trnsf. Inc.: This is the mean for the group of the standard deviation for each family of the income measure defined in (1) above.

equal to full-time work, so that the instability in earnings observed among both the less and more heavily welfare dependent seems to reflect movement in and out of employment as opposed to movements between part-time and full-time employment. The heavily dependent have lower incomes than the less heavily dependent, but within each of these groups we can see that fluctuations in welfare payments are related to fluctuations in the male heads' earnings and, thus, in family income. Those with fluctuating transfer payments also change jobs more frequently than those with stable transfer payments. Further, while female spouses seem to work more in this sample, as in general, when their husband's earnings are low, there is only suggestive evidence that they work more to offset the fluctuations in their husband's earnings. At low levels of welfare dependency, the mean earnings of female spouses are equal for those with more or less stable earnings of the male head. At high levels of dependency, female spouses do seem to compensate for fluctuations in their husbands' earnings by working more.

From the data on welfare experience by welfare pattern groups in Table IV-3, we can see that our welfare pattern groups do pick up differences in the level of NIT and AFDC-UF payments, as well as differences in their variability. Note that the more highly dependent families not only have lower incomes than the less dependent, but that those with more unstable patterns have, on the average, a larger standard deviation of transfer payments over time.

The impact of program parameters on welfare experience can be seen in data not presented in our tables. While only 56 percent of the families in the two low payment welfare pattern groups are eligible for NIT payments, 78 percent of the families in the two high payment welfare pattern groups are in the experimental groups. Allowing for some negative impact of guarantees and tax rates on work effort, this heavier concentration of NIT-eligible families in the more dependent pattern groups is strongly suggestive of the importance of program parameters -- guarantees, tax rates, and eligibility rules -- in determining welfare patterns, even where behavior is uninfluenced by the program.

C. Occupational and Industrial Changes Associated With Work Patterns

Though the variability of earnings results from factors in addition to job changes, here we inquire briefly into how job changing varied by occupation and industry to provide an additional insight into these potential sources of the variability of earnings through time.

Job changes are defined in the Wisconsin data as changes in employers. If a person is temporarily out of work and returns to the same employer upon re-employment, there is no job change. If a person goes from one employer to a second and then returns to his initial employer, he is credited with two job changes. So job changes, as defined in this data set, have nothing to do with intra-firm mobility, but do reflect inter-firm movement of workers in the labor market.

Table IV-4 indicates how the amount of job changing varies wth occupation, Table IV-5 by industry. The ~~number~~ of jobs held is associated for each worker with his first known occupation and industry. Since the sample contained only 894 workers, we collapsed data from the 3 digit U.S. Census Bureau occupational and industrial codes into nine occupational and seven industrial groupings. (In Tables IV-4 through Table IV-7, "unknowns" and "others" largely denote periods of non-employment.) In fact, under the original 3 digit codes, a very large proportion of workers were placed by the Wisconsin staff in the "not elsewhere classified" categories within broad classifications like operatives. Thus a finer breakdown by occupations and industries would hardly have been more informative than the one used.

In Tables IV-4 and Table IV-5, no indication of an association between job changes and occupation or industry is apparent. Most of the male heads originally were in four occupational categories, craftsmen, operatives, services workers, and laborers.

In each of these four groupings, roughly two-fifths of the workers changed jobs one or more times during the three year experiment, while roughly one-fourth of the workers in each of the four categories changed jobs two or more times. Workers changing jobs

TABLE IV-4

Percentage Distribution of Male Heads By Number of Jobs Held During Experiment and By Occupation

(Wisconsin Sample)

Occupation	(1) None (N=89)	(2) One (N=462)	(3) Two (N=148)	(4) Three or More (N=195)	(5) Total in Row (N=894)	
	Number of Jobs Held					
Professional, Technical And Kindred Workers	0	57	29	14	100%	(N=7)
Managers	15	39	15	31	100%	(N=13)
Clerical And Kindred Workers	5	54	7	16	100%	(N=43)
Sales Workers	0	100	0	0	100%	(N=3)
Craftsmen, Foremen, And Kindred Workers	6	51	18	24	100%	(N=115)
Operatives and Kindred Workers	2	57	20	22	100%	(N=405)
Service Workers, Including Household	2	55	18	25	100%	(N=91)
Laborers, Farm and Non-Farm	2	57	14	25	100%	(N=139)
Unknown	83	12	3	3	100%	(N=78)

NOTES

Definition of Variables:

1. Professional, Technical, and Kindred Workers: In the 3 digit occupational code of the US Bureau of the Census, this group includes those coded 001 through 195.

2. Managers: This group includes persons with codes 200-295.

3. Clerical and Kindred Workers: Persons with codes 301-375.

4. Sales Workers: Persons with codes 380-395.

5. Craftsmen, Foremen, and Kindred Workers: Persons with codes 401-495.

6. Operatives and Kindred Workers: Persons with codes 801-895.

7. Service Workers, Including Household: Persons with codes 801-895.

8. Laborers, Farm and Non-Farm: Persons with codes 901-994.

9. Unknown: Persons with codes 996 to 999, as well as persons who were assigned no positive code on the data tape.

TABLE IV-5

Number of Male Heads By Number of Jobs Held
During Experiment and By Industry

(Wisconsin Sample)

Industry	(1) None (N=36)	(2) One (N=462)	(3) Two (N=147)	(4) Three or More (N=194)	(5) Total in Row (N=839)	
	Number of Jobs Held					
Construction	12	46	10	32	100%	(N=29)
Manufacturing, Durables	2	62	17	19	100%	(N=162)
Manufacturing, Nondurables	1	54	20	25	100%	(N=184)
Transportation, Communication and Utilities	7	51	12	30	100%	(N=59)
Wholesale and Retail Trade	4	56	17	23	100%	(N=94)
Services, Private and Govt.	8	51	19	21	100%	(N=127)
Others	0	75	25	0	100%	(N=238)

NOTES

Definitions of Variables:

1. Construction: In the 3 digit industry codes of the US Bureau of the Census, this group includes those with codes 190-199.

2. Manufacturing, Durables: This group includes persons coded 206-296.

3. Manufacturing, Nondurables: Persons coded 306-459.

4. Transportation, Communication, and Utilities: Persons coded 506-579.

5. Wholesale and Retail Trade: Persons coded 606-696.

6. Services, Private and Govt: Persons coded 706-936.

7. Others: Persons coded 996-999, persons assigned no positive code on the data tape, and persons not falling within the first six groupings.

TABLE IV-6

Numbers of Male Heads (Who Changed Jobs) Moving Between Original and Subsequent Occupations, By Original Occupation (Wisconsin Sample)

	Subsequent Occupations								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Original Occupation	Professional, Technical, And Kindred Workers	Managers	Clerical & Kindred Workers	Sales Workers	Craftsmen, Foremen, And Kindred Workers	Operatives & Kindred Workers	Service Workers, Including Household	Laborers, Farm & Non-Farm	Unknown
Professional, Technical And Kindred Workers (N=3)	3	0	0	0	0	0	0	0	2
Managers (N=6)	0	6	0	0	0	3	0	1	5
Clerical And Kindred Workers (N=18)	1	1	15	1	1	9	2	2	13
Sales Workers (N=0)	-	-	-	-	-	-	-	-	-
Craftsmen, Foreman, And Kindred Workers (N=49)	2	1	3	0	41	19	6	10	37
Operatives And Kindred Workers (N=169)	4	1	17	3	27	161	12	40	119
Service Workers, Including Household (N=39)	1	1	1	1	7	11	37	12	28
Laborers, Farm and Non-Farm (N=55)	0	1	2	1	8	34	9	45	42
Unknown (N=4)	0	0	1	0	0	1	0	1	3

Note: The numbers in each cell indicate the number of workers in an original occupation who entered a particular occupation for at least 1 period upon changing jobs. For example, the 19 in the fourth row, sixth column indicates that 19 men who were craftsmen in their first jobs entered operatives jobs for at least one period after they left their first jobs.

two times in a three year period would average only twelve months on each of three jobs, if they experienced no gap in employment between jobs. Although the proportions having two or more jobs were similar among industries, the proportions holding three or more jobs were higher in construction and transportation.

When changing employers, the men in this sample frequently also changed occupations and industries. The data in Tables IV-6 and IV-7 are intended to indicate the occupational and industrial mobility of the workers in this sample over the relatively short period of three years. From the data in Table IV-4 (or IV-5), we know that two-fifths of all the men had two or more jobs during the experiment. Tables IV-6 (and IV-7) show where the part of the sample that changed jobs went when they took their subsequent jobs. The numbers in the cells of these two tables represent the number of job changers from a given original occupation (or industry) who at some time during the three years worked in any particular subsequent occupation (or industry), i.e., excluding the first one in which they worked. Thus, the table does not contain information on those who never changed jobs. Also, men changing jobs may have entered the same or a different occupation after their first job.

Looking first at Table IV-6, note that there is substantial inter-occupational movement in the four categories in which most of the men originally are located. Although it is hard to combine these data, it seems to be the case that workers initially in the laborers category do the most moving, ususally upwards to the operatives category. Excluding the unknown category, only 45 percent of the subsequent jobs taken by laborers who do change fit into the laborers category; 34 percent are in the operatives category. Operatives and service workers are less likely to enter new occupations when they change jobs, and neither enter any other occupational category as fequently as laborers enter that of operatives. While operatives and craftsmen do change occupational categories with some frequency, there is no obvious evidence in these data that persons in either group engage in marked upward movement in the labor market.

Comparing the data in Table IV-7 with that in IV-6, we see that industrial identification or attachment, as might be expected, is somewhat less prevalent than occupational attachment as these men move through the labor market. Whereas 44 percent of job changes (into a known occupational group) result in changes in occupational category, 47 percent of all changes (into a known industry group) result in changes in industry category. The job changers in each original industry category show a marked tendency to move widely among industries as they change jobs.

In sum, these data on the incidence of job changing and on occupational and industrial changes associated with job changes reflect limited attachment among two-fifths of the workers to their jobs. Among the latter, there are many workers who also have limited attachments to their occupations and industries. Such lack of attachment, if sustained over longer periods, should lead periodically to income interruptions and at least occasional dependence on income transfers. Income interruptions would be more likely for such workers if their job search procedures are complicated by a lack of occupational and industrial attachment.

D. Conclusion

Our analysis of employment and welfare histories by a "case history" approach has yielded insights not provided by the statistical analysis that follows in Chapter VI.

1. Within the Wisconsin sample of male heads of families, several distinct earnings pattterns have been distinguished. The men first may be divided into "regular" and "irregular"workers, constituting, respectively, 39 percent and 48 percent of the total sample (which also includes the 13 percent who make up the Missing Data cases). Regular workers further are subdivided into those with Stable High and Unstable High earnings, respectively representing 17 percent and 22 percent of the total sample. Within both of these groups, observed unemployment or absence from the labor force is negligible. There may be some short-term unemployment that we cannot detect because our data cover only the last week in each of 12 quarters. Instability in earnings in the second of the two groups must derive from fluctuations in moonlighting and overtime work. We deduce from the very low unemployment and overall regularity of earnings which we observe that if unemployment strikes, these men try to return quickly to work.

TABLE IV-7

Numbers of Male Heads (Who Changed Jobs) Moving Between Original and Subsequent Industry, by Industry

(Wisconsin Sample)

Original Industry	Subsequent Industry (1) Construction	Mfg., Durables	Mfg., Non Durables	Trns. Comm. and Util.	Wholesale & Retail Trade	Services, Private and Govt.	Others
Construction (N=21)	19	4	5	5	2	5	15
Manufacturing, Durables (N=71)	5	62	22	8	14	15	51
Manufacturing, Nondurables (N=99)	3	17	90	10	14	17	69
Transportation, Communication and Utilities (N=34)	4	2	3	34	8	2	26
Wholesale, and Retail Trade (N-44)	5	5	5	9	40	8	35
Services, Private and Govt. (N=71)	7	12	13	12	17	59	46
Others, Including Unknown (N=1)	0	1	0	0	0	0	2

NOTE:

The numbers in each cell indicate the number of workers in an original industry who entered a particular industry for at least 1 period upon changing jobs. For example, the 4 in the first row, second column indicates that 4 men who were in construction in their first jobs entered a durable manufacturing sector job for at least one period after they left their first jobs.

Irregular workers may be subdivided further into those with Stable Low, Unstable Low, and High-1 Dip earnings patterns. They constitute, respectively, 7 percent, 31 percent, and 10 percent of the total sample. Persons in the Stable Low work very little, often as a consequence of poor health. What is most interesting about the earnings patterns of the latter two groups is that where breaks in employment arise, we can observe from the plottings of individual earnings that re-employment is bound to result in the overwhelming proportion of cases. By and large, then, among irregular and regular workers, attachment to the labor force is the rule -- even in the presence of generous welfare programs.

2. One-fifth of the men in the entire sample worked very hard, averaging 41 or more hours of work per week during the experiment. Of those with Unstable High earnings, 57 percent averaged 41 or more hours of work per week over a three year period.

3. Not surprisingly, those with low earnings generally were more dependent on welfare than those with high earnings. Besides earnings, though, program structure also affected welfare patterns: those who were eligible for more generous welfare benefits were more dependent on welfare.

4. Trying to detect an affect of occupation or industry on earnings patterns, we examined the association between the frequency of job changes and those two variables. No indication of an association between job changes, defined as changes in employers, and occupation or industry is apparent in our data.

5. The data on job changes suggest a seemingly low degree of occupational and industrial attachment among the workers in this sample. Among those -- roughly 40 percent of the sample -- who ever changed employers during the experiment, 44 percent of all job changes resulted in changes in occupational category and 47 percent of all job changes resulted in changes in industrial category.

CHAPTER IV

FOOTNOTES

1. Recall that our earnings data actually are for the last week in each quarter. Quarterly earnings simply are weekly earnings multiplied by 13. Also, in the discussion of earnings patterns we use earnings deflated by the consumer price index (1967 = 100).

2. The numbers of persons for whom the standard deviations could be computed were smaller than those for whom patterns were determined because of missing information. The cell sizes for the standard deviations were, respectively: 39, 150, 90, 278, 193, 80 and 10.

3. Recall that our transfer payments data, unlike the earnings data, actually are quarterly amounts. In the discussion of welfare patterns, they are not deflated.

4. Robert E. Hall, "Why Is the Unemployment Rate So High at Full Employment?" Brookings Papers on Economic Activity, No. 3, 1970, p. 390.

CHAPTER V

A Model of Earnings

In the remaining chapters, we want to delve more deeply into the interactions between income and transfer payments and into the full range of other variables affecting these two quantities. Since a number of variables operate on income and transfers simultaneously, statistical techniques are required to isolate the separate effects of the exaplanatory variables. In order to formulate statistical tests, we must formulate hypotheses reflecting both the nature of transfer programs and the likely behavioral response of recipients. In this chapter, we will formulate our earnings model while in the next we will present our statistical tests on earnings. Investigations of transfer payments are presented in Chapter VII. Part A of this chapter gives a general description of the model, Part B presents a more technical discussion of the theoretical model, and Part C discusses estimation techniques. Parts B and C may be omitted by a reader anxious to see the empirical results.

A. General Description of the Model

In order to study income, we must examine its parts separately. Some of the income of a household may come from a government sponsored transfer program (e.g., AFDC-UF or NIT). Let W represent the transfer payments to a household while I represents other or non-transfer income (which, henceforth, we will call income). One difficulty for analysis is that W and I are closely interrelated. The transfer programs we are studying -- AFDC-UF and NIT -- are both income-conditioned so that the amount of W is adjusted on the basis of the amount of I. The effect of I on W is thus determined by aspects of program structure like the guarantee level, the tax rate, and the income accounting system. But there may also be a causal relationship running the other way, from W to I. This arises if, for example, the transfer payment induces a family member to reduce work effort, thereby reducing his earnings. It is useful to break down the parts of I further by writing

$$I = E_M + E_F + E_O + Q$$

where E_M, E_F, and E_O are the earnings, respectively, of the husband, wife, and other family members, while Q measures unearned, non-transfer income. W will affect I to the extent that it affects the parts of I, E_M, E_F, and E_O, the earnings of various family members. A family might even try to reduce its Q in order to qualify for more benefits but we will not try to account for this effect here. We must thus be prepared for causal effects running both from I to W and from W to I. In the presence of mutual causation, it is known that any direct estimate of the effect of I on W or W on I will be statistically biased.

We can begin to disentangle these mutual effects by considering the typical formula for an income transfer payment, W:

$$W = G - tI$$

where G is the guarantee level and t the tax rate. (We ignore the complications introduced by the income accounting system.) G and t are both program characteristics fixed independently of the behavioral response of the family. The effect of W on I is thus a consequence of the separate effects of G, t, and I on I. We could elminate the problem (of, in effect, explaining I in part on the basis of itself) if we could legitimately replace W as an explanatory variable by just G and t, variables not determined by the behavior of the family. Economic theory tells us that this is appropriate. To apply the usual economic model, we must analyze the earnings of each family member separately. The standard model of the work effort of an individual explains his work effort on the basis of his wage rate, the unearned income of the family, and some term accounting for the earnings of other family members. Since the earnings of an individual are equal to his work effort (hours worked) multiplied by his average wage, his earnings also will depend on the same variables. It follows from the standard model that the guarantee level has the same effect on earnings as unearned income, while the tax rate modifies the wage rate. Some simple manipulations shown in Part B allow us to incorporate the tax rate and the guarantee level into a new family unearned income variable, N, combining the effects of Q and the exogenous program parameters of the welfare system. The assumption of a Cobb-Douglas utility function leads to the convenient result that earnings are a linear function of the previously mentioned variables. Standard theory thus suggests equations for male and female earnings, respectively, in the following form:

$$(1a)\quad E_M = \alpha_1 W_M + \alpha_2 W_F + \alpha_3 N + u_M$$

$$(1b)\quad E_F = \beta_1 W_M + \beta_2 W_F + \beta_3 N + u_F$$

W_M and W_F measure, respectively, the husband's and wife's wage rates. The α's and β's are constant terms to be estimated while the u's are error terms.

The standard model of earnings discussed above has the apparent drawback for our purposes that it is a static model. It explains earnings for a single period, but gives little direct insight into our principal interest -- the pattern of earnings over time. We will concentrate on two aspects of earnings patterns -- their mean and standard deviation. The mean provides a measure of the variability about that level. We have found a very convenient and fruitful way to use the standard model of earnings in studying the mean and standard deviation. Suppose that in equations (1a) and (1b) we assume that the error term has an expected value of zero. Then those equations give the expected values of E_M and E_F, respectively, conditional upon given values of W_M, W_F, and N. In other words, we may think of the standard model as explaining expected earnings. Now actual earnings in any period are likely to deviate from the expected value because of the error term, u_M or u_F. But we have two sets of longitudinal data, from Michigan and from the New Jersey experiment, both having a series of observations on each variable for each family over time. It is known that the mean of a random variable approaches the expected value as the sample size becomes larger. Although our time series is not especially long, we will take the mean over time as an approximation to the expected value of a variable for an individual and explain mean earnings using the standard earnings model on the basis of the mean values of W_M, W_F, and N.

The standard model of earnings can also be useful in deciding how to define the standard deviation. Since we explain mean earnings on the basis of W_M, W_F, and N, changes in these variables lead to changes in earnings. Some of the variation in earnings can then be explained directly by W_M, W_F, and N in the equations for mean earnings. But even if an individual had unchanging values of W_M, W_F, and N, earnings might fluctuate because of variations in the error term. Thus, variations not already explained by the mean earnings equations are measured by the standard deviations of the errors, u_M and u_F, respectively. We might expect different individuals to have different patterns and hence different standard deviations, s_M and s_F. These differences should vary systematically depending on factors related to the individual's tastes and abilities and on his situation in the labor market. For example, some individuals might work in industries with seasonal fluctuations in demand and employment.

The difficulty with our proposed measure of the standard deviation is that u_M and u_F are not directly observable. In Part C of this chapter, we present the technique that we will use to estimate these standard deviations and how we will then use our estimates to refine our mean earnings equations. Chapter VI, part A presents a discussion of what variables might systematically affect the standard deviation, followed by empirical estimates; while part B of chapter VI, presents empirical results for the mean earnings equations. The reader not wanting to follow a further elaboration of the technical details of the model now may proceed directly to Chapter VI.

B. The Theoretical Model

1. Cobb Douglas Variant of the Work-Leisure Model.

Consider a family which consumes a bundle of goods measured by the index X. Let L_M be the leisure time of the male and L_F that of the female. (We will ignore the work-leisure choices of other family members.) Then assuming a Cobb-Douglas utility function we have

$$(2) \quad U(L_M, L_F, X) = L_M^{\alpha_M} L_F^{\alpha_F} X^{(1-\alpha_M-\alpha_F)}$$

In the absence of the male, $\alpha_M = 0$; and, in the absence of the female $\alpha_F = 0$.

The hours worked by the male and female are H_M and H_F, respectively. Let W_M and W_F measure the male and female wage rates, and let the price of the index of goods be unity, so that in effect W_M and W_F measure the real wage rates in terms of goods. The income of the household may come from earnings, $W_M H_M$ and $W_F H_F$, from unearned, non-transfer income, Q, and from transfers, W. Transfer income equals an income guarantee level, G, minus a correction or tax based on non-transfer income. Letting t_E be the tax rate on earnings and t_Q the tax rate on unearned income,

$$W = \begin{cases} G - t_E(W_M H_M + W_F H_F) - t_Q Q, \text{ if } > 0 \\ 0, \text{ otherwise} \end{cases}$$

A family receives an income transfer only if its income is small enough so that W remains positive. The budget constraint of the family is thus

$$\begin{aligned} X &= W_M H_M + W_F H_F + Q + G - t_E(W_M H_M + W_F H_F) - t_Q Q \\ (3) \qquad &= (1-t_E)W_M H_M + (1-t_E)W_F H_F + t_Q)Q + G, \text{ if } W > 0 \\ \text{or,} \qquad X &= W_M H_M + W_F H_F + Q, \text{ if } W = 0 \end{aligned}$$

The variables H_M and H_F of the budget constraint can be related to the variables L_M and L_F of the utility function, if we introduce the constants $\overline{H}_M$ and $\overline{H}_F$, the maximum number of hours the male and female, respectively, can work. While $\overline{H}_M$ and $\overline{H}_F$ cannot exceed the total number of hours in the period under consideration, they should be interpreted as behavioral parameters rather than as technical constants.[1] In part, they measure the maximum number of hours that the male or female could be induced to work under the conditions most encouraging to work effort. In addition they may reflect economic constraints on the amount of work the person can do -- constraints beyond the control of the individual resulting perhaps from market forces like insufficient aggregate demand or possibly from discrimination in hiring certain kinds of workers. It follows then, that the leisure quantitites L_M and L_F measure only that part of leisure displacing potential work effort. We have

$$(4) \qquad L_M = \overline{H}_M - H_M, \; L_F = \overline{H}_F - H_F$$

Using (4) we can express the budget constraints in terms of leisure time instead of work time. Using (4) in (3) and rearranging, we obtain

$$\text{(5a)}\quad X + (1-t_E)W_M L_M + (1-t_E)W_F L_F = (1-t_E)W_M \overline{H}_M + (1-t_E)W_F \overline{H}_F + (1-t_Q)Q + G, \text{ if } W > 0$$

or

$$\text{(5b)}\quad X + W_M L_M + W_F L_F = W_M \overline{H}_M + W_F \overline{H}_F + Q, \text{ if } W = 0$$

We thus have two alternative budget constraints, (5a) and (5b). If the family is receiving a transfer benefit, the parameters of the transfer system enter the constraint (5a), while if no transfer benefit is received, the transfer parameters do not constrain choice and the family can move along (5b).

We assume that the family seeks to adjust L_M, L_F, and X in order to maximize utility, (2), subject to its budget constraint, and given W_M, W_F, $\overline{H}_M$, $\overline{H}_F$, Q, and the parameters of the transfer system. As part of this choice, the family must decide whether or not to receive income transfers. The decision to go on a transfer program can be formalized in two steps. First, let the family matrix utility with respect to (5a), leading to optimal quantities, H_M^*, H_F^*, X^*, and $U^* = U(\overline{H}_M - H_M^*, \overline{H}_F - H_F^*, X^*)$. Note that there is a restriction on the attainable values of these variables, for if the optimal quantities produce so much income that W cannot remain positive, the individual cannot remain on the transfer program. In the second step, let the family maximize utility with respect to constraint (5b), finding optimal quantities

$$H_M^{**},\ H_F^{**},\ X^{**}, \text{ and } U^{**} = U(\overline{H}_M - H_M^{**}, \overline{H}_F - H_F^{**}, X^{**}).$$

Comparing the results in the two steps, if $U^* > U^*$, the family chooses to go on the program while if $U^{**} > U^*$, the family chooses not to do so.

Applying the usual first order conditions for a maximum, we find after tax earnings euations for the husband and wife. If $U^* > U^{**}$ (so that (5a) is the relevant constraint),

$$(6a)\quad \begin{aligned} (1-t_E)W_M L_M &= \alpha_M[(1-t_E)W_M\bar{H}_M + (1-t_E)W_F\bar{H}_F + (1-t_Q)Q + G],\ W > 0 \\ (1-t_E)W_F L_F &= \alpha_F[(1-t_E)W_M\bar{H}_M + (1-t_E)W_F\bar{H}_F + (1-t_Q)Q + G],\ W > 0 \end{aligned}$$

When $U^{**} > U^{*}$ (with (5b) the relevant constraint),

$$(6b)\quad \begin{aligned} W_M L_M &= \alpha_M[W_M\bar{H}_M + W_F\bar{H}_F + Q],\ W = 0 \\ W_F L_F &= \alpha_F[W_M\bar{H}_M + W_F\bar{H}_F + Q],\ W = 0 \end{aligned}$$

The value of the leisure time chosen by each family member is a constant fraction α_M or α_F of "full income," the expression in brackets in each case, which is what income would be if every family member worked the maximum amount. Using (4) and dividing each equation by $(1-t_E)$ when $U^{*} > U^{**}$, we obtain pre-tax earnings:[2]

$$(7a)\quad E_M = W_M H_M = \begin{cases} (1-\alpha_M)\bar{H}_M W_M - \alpha_M \bar{H}_F W_F - \alpha_M Q, & \text{if} > 0 \text{ and } W = 0 \\ (1-\alpha_M)\bar{H}_M W_M - \alpha_M \bar{H}_F W_F - \alpha_M \dfrac{(1-t_Q)\ Q + G}{1-t_E}, & \text{if} > 0 \text{ and } W > 0 \\ 0, & \text{otherwise} \end{cases}$$

$$(7b)\quad E_F = W_F H_F = \begin{cases} (1-\alpha_F)\bar{H}_F W_F - \alpha_F \bar{H}_M W_M - \alpha_F Q, & \text{if} > 0 \text{ and } W = 0 \\ (1-\alpha_F)\bar{H}_F W_F - \alpha_F \bar{H}_M W_M - \alpha_F \dfrac{(1-t_Q)\ Q + G}{1-t_E}, & \text{if} > 0 \text{ and } W > 0 \\ 0, & \text{otherwise} \end{cases}$$

Data are available on the variables W_M, W_F, Q, and the parameters of the transfer programs. The first two cases of both equations (7a) and (7b) are thus linear equations in W_M, W_F, and an income term. The coefficients to be estimated, given our Cobb-Douglas model, can be interpreted as behavioral parameters. The coefficient of the income term, $-\alpha_M$ or $-\alpha_F$, measures (the negative of) the fraction of "full income" that the respective family member chooses to consume in the form of leisure. The coefficient of the own-wage, $(1-\alpha_M)\bar{H}_M$ in (7a) and $(1-\alpha_F)\bar{H}_F$ in (7b), must be positive since α_M and α_F are both less than one. Given estimates of α_M and α_F from the income terms, these own-wage coefficients can be unscrambled to obtain estimates of $\overline{H}_M$ and $\overline{H}_F$ which we assumed to to be behavioral parameters. The cross-wage coefficients, $-\alpha_M\overline{H}_F$ and $-\alpha_F\overline{H}_M$, are both negative. Significant coefficients not satisfying these theoretical restrictions would cast serious doubt on the Cobb-Douglas formulation of the theory of work effort.

Notice finally that we have three variants of both (7a) and (7b). The first two are identical except in the way income is measured. When the family receives transfer benefits, the parameters of the transfer system must be introduced into the income term in the way derived in our previous calculations. The third variant is different. The difficulty with the first two is that if either the appropriate income measure or the spouse's wage is too large relative to the own-wage, a negative value of earnings is predicted. Since negative earnings (or work effort) make no economic sense, we conclude that earnings would be zero when either of the first two variants predict negative earnings. This raises the important problem that the zero earnings of those who do not work are not explained by the same linear relationship that explains positive earnings. The two groups must be treated separately. The non-workers do not satisfy the first order equality conditions that the workers do. They can be shown to satisfy inequality conditions and lie at a "corner solution."

2. Choice of Income Transfer Program

As has already been noted, the introduction of an AFDC-UF program in New Jersey not only provided a welfare program to the control group under the experiment, but also provided a second option to those covered under the experimental plans. If eligible, they could receive either a NIT payment, or they could switch to the AFDC-UF program. Since equations (7) include an income term involving parameters of the transfer program, some principle is needed to determine which parameters to use for a family facing a choice of two transfer programs. A simple extension of the argument on whether a person chooses to be on or off a transfer program provides such a principle. Given the parameters of each transfer program we can construct a separate budget constraint corresponding to each program. Confronting the family first with one budget constraint, we calculate the maximized value of utility. The operation is then repeated with the second constraint. The family chooses the program that leads to the highest utility (provided that this level of utility is higher than that obtained receiving no transfer benefit). To achieve this highest utility, the family must be on the budget constraint determined by the parameters of the chosen program.

To apply this principle, we observe which transfer program the family chooses. Assuming that it makes the choice maximizing its utility, we use in equations (7) only the guarantee and tax rate parameters of the chosen program.[3]

3. Fluctuations in Earnings

Equations (7) predict changes in earnings in response to changes in wage rates or in the appropriate income measure. However, fluctuations in earnings also may be caused by changes on the demand side of the labor market or by additional aspects of individual behavior not incorporated into equations (7). Consider first problems related to the demand for labor. With wages inflexible, fluctuations in the demand for labor will tend to lead to fluctuations in employment. The extent of these fluctuations will differ by labor market, by industry, by occupation, and perhaps by worker characteristics of significance to employers, like seniority. Whatever the source of the fluctuations, they mean that in the short run, at least, the actual earnings of an individual will deviate from his desired level. Earnings will be less than desired when a person is fired or laid off temporarily and higher than desired when there are required increases in overtime. If we interpret equations (7) as giving the level of earnings desired by the individual, it thus provides only a partial explanation of actual earnings.

However, equations (7) may not even provide an adequate representation of desired earnings. In addition to the obvious simplifications resulting from the Cobb-Douglas form of the utility function, equations (7) do not consider the timing of the decisions.

One person may prefer steady employment while another prefers work interspersed with frequent breaks. Over a sufficiently long time horizon, equations (7) could explain the difference in some cases, perhaps by a difference in α. The person desiring frequent breaks over a long period prefers to devote a larger fraction of "full income" to leisure. But we are interested in short term fluctuations. Moreover, steady part-time work could yield the same α as intermittent full-time work. The standard model in itself explains a desired level of earnings, but provides no way of distinguishing a preference for a particular pattern..

For purposes of analysis we find it convenient to consider separately the determination of the normal or desired level of earnings and the fluctuations about this normal level. The earnings model given by equation (7) provides a convenient explanation of the normal level of earnings. Then the demand side factors together with individual preferences for patterns determine fluctuations. To be more specific, rewrite equation (7a) expressing the first two cases (when earnings are positive) in the same form, and including an error term u_t.

$$(8) \qquad E_{Mt} = b_1 W_{Mt} + b_2 W_{Ft} + b_3 N_t + u_t.$$

This relationship holds for a given period, t, indicated by the subscript attached to each variable. Here $b_1 = (1-\alpha_M)\overline{H}_M$, $b_2 = -\alpha_M \overline{H}_F$, $b_3 = -\alpha_M$, N_t is the appropriate measure of other income depending on the kind of transfer payments received. (The argument is identical for female earnings so we present only the case of male earnings.) We assume that the parameters b_1, b_2 and b_3 remain constant from period to period over the time interval considered. We assume that for this individual u_t is randomly distributed over time with expectation, $\varepsilon(u_t) = 0$, and standard deviation s. The expected value of earnings, $\varepsilon(E_{Mt})$, is thus

$$(9) \qquad \varepsilon(E_{Mt}) = b_1 W_{Mt} + b_2 W_{Ft} + b_3 N_t .$$

We thus assume that earnings of an individual over time can be analyzed as if they were randomly distributed with conditional expectation given by (9) and standard deviation of the conditional distribution given by s. The distribution of the fluctuations in earnings (other than those explained by W_M, W_F and N) is thus determined by s. But s is likely to vary from one individual to another depending on his situation in the labor and on his preferences concerning pattern. Although we do not try to explain every change in a person's earnings, we do study the overall pattern (or at least that aspect of it measured by the standard deviation) in a way that allows us to make predictions of individual behavior and comparisons between individuals.

In Part C, we discuss our techniques for studying the conditional expectation of earnings on the one hand, and the standard deviation, s, on the other. Before proceeding to that, consider some general aspects of our approach. Although equation (9) can be viewed as an equation for normal or long run desired earnings $\varepsilon(E_{Mt})$ is not observable. We know from equation (8) that actual earnings in any period will differ from its expected value by u_t, which may be large. Since we are hypothesizing that it is the long run or normal level of earnings that is explained by equation (9), it is desirable to measure the mean of earnings over time rather than the actual level in any one period, and to use the mean in explaining the normal level of earnings. To see the advantage of working with the mean, take the average over time of both sides of equation (8), letting bars denote averages over time.

$$(10) \qquad \overline{E}_M = b_1\overline{W}_M + b_2\overline{W}_F + b_3\overline{N} + \overline{u}$$

Since we assumed that $\varepsilon(u_t) = 0$, it follows that $\varepsilon(\overline{u}) = 0$. Therefore, the mean $\overline{E}_M$ is explained by the means of the independent variables in exactly the same way that E_{Mt} is explained by equation (8), except that the standard deviation of the error term is now reduced. Whereas u_t has standard error s, $\overline{u}$ has a standard error $\frac{s}{\sqrt{T}}$ where T is the number of observations used to calculate each of the means. We will frequently refer to the normal or long run level of earnings as mean or average earnings. Equation (10) is the one we will estimate in our empirical investigations of mean earnings.

Many factors besides W_M, W_F, and N may explain period-by-period earnings, but our argument suggests that they should not enter equations (8) or (10) as additive dummy variables. Indeed, the Cobb-Douglas model implies that equation (8) should have no constant term. Consider the example of a variable like education which is usually expected to have an effect on earnings. In terms of our model, the principal effect of education on earnings would be transmitted through the wage rate, assuming that more education allowed the individual to face a higher wage. In addition, a more educated person might have different tastes than a less educated one. But differences in tastes lead to differences in the coefficients b_1, b_2, and b_3, not to differences in the constant term. The only other effect of education might be on the pattern of earnings. If more educated people either faced more stable market opportunities or preferred greater stability, they would have a smaller value of s. Similarly, any variable affecting earnings outside of the channels through W_M, W_F, and N either must change one of the coefficients b_1, b_2, or b_3 in which case it affects mean earnings, or it must change s, in which case it affects the pattern of earnings.

4. The Treatment of Unemployment.

Equations (7) presented expressions for earnings in three cases. Only the first two, involving positive earnings, entered the discussion on fluctuations. The third case, with zero earnings, must be considered separately. Zero earnings could arise in two situations. First, if equation (9) predicted an expected value of earnings of either zero or less than zero, actual earnings would no doubt equal zero. Given the person's tastes and values of W_M, W_F, and N that he faces, he chooses not to work. Equations (8) - (10) do not apply in this situation. Second, his actual earnings in some period may be zero because of a large negative value of u_t, even though his normal desired level is positive. The negative u_t could result from an involuntary layoff or from a desire for periodic breaks in employment, combined with a long run desire for positive earnings. As long as desired earnings are positive, equations (8) - (10) hold. The zero earnings are simply part of a temporary fluctuation.

In principle the two situations are distinct. The difference depends on whether or not equation (9) predicts a positive value. In practice, the situations are hard to distinguish because an unemployed worker does not have an observable wage rate. The worker may have a good idea of what wage rate he can normally expect but such information is probably not available to the analyst.[4] We will assume that any person with zero earnings for all periods studied does not have positive normal earnings. We will exclude these individuals in developing a mean earnings equation. On the other hand, any individual who has positive earnings for at least one period has positive mean earnings. We will assume that these individuals are unemployed only because of temporary fluctuations and include them in developing both a mean earnings equation and an equation for the standard deviation.

C. Techniques of Estimation

1. The Mean Earnings Equation

In estimating the coefficients of equation (10), one problem is that of heteroscedasticity. The previous argument suggested that the standard deviation of the error in equation (10) would differ from person to person because of differences in labor market situations and tastes. Let σ_i now represent the standard deviation of the error in equation (10) for individual i [which was previously denoted by $\frac{s}{\sqrt{T}}$]. T, the number of time periods, is the same for all individuals, so that variations in σ_i result strictly from variations in s. Then the variance-covariance matrix ($\sigma^2 V$) of the error terms of equation (10) in a cross section of n individuals is a (nxn) diagonal matrix on the assumption that the errors of different individuals are uncorrelated. The i^{th} diagonal element is $\sigma^2\sigma_i^2$, where σ^2 is a common scale factor. Given the matrix V, the appropriate estimation technique for equation (10) is generalized least squares (GLS). The GLS technique in this case requires that each variable for individual i, dependent and independent, be divided by σ_i. Least squares is then applied in the usual way to this transformed set of data. The only difficulty in using this technique is that the elements of V are not known. It is thus essential to produce estimates of σ_i not only to study individual patterns, but also to obtain the GLS estimate of the coefficients of the mean earnings equation.

2. Estimation of the Standard Errors

In the mean earnings equation (10), we have already made use of the time series available on each individual by calculating the mean over time of each variable for each individual and then using these means in the cross-section estimation. By so doing, we reduced the standard deviation of estimate from what it would have been had we used cross-section data from a single period. Now we propose to exploit again our time series data to calculate the standard error of the unexplained fluctuations in earnings for each individual. We need estimates of the coefficients in equation (10) to calculate the unexplained fluctuations. However, the best linear unbiased GLS estimates of the coefficients can be calculated only once the standard deviation of the unexplained fluctuations is known. There is nevertheless a way around these difficulties. Although the coefficient estimates using GLS are best linear unbiased, estimates from ordinary least squares (OLS) are also unbiased, even in the presence of heteroscedascity. As a first step, we will estimate equation (10) using OLS. For convenience, the estimated earnings equations for a cross-section of n individuals may be written in matrix form as

(11) $E = Xb + v$

where E is the (nxl) vector of mean earnings, X is the (nxk) observation matrix of the individual means of the k independent variables, b is the coefficient vector estimated by the use of OLS, and v is the (nxl) vector of errors resulting from the OLS estimation. Equation (11) gives a single error, v_i, for each individual.

We assumed earlier that the coefficients explaining mean earnings for an individual also explain the expected value of period-by-period earnings. We may therefore use the coefficients estimated for the mean earnings equation to predict period-by-period earnings based on the period-by-period values of the independent variables for an individual. For individual i, let E_i^* be the (Txl) vector of his actual earnings for T periods. Similarly, let X_i^* be the (Txk) matrix of observations on the k independent variables faced by individual i in each of the T periods. The asterisks denote that these variables were not used directly in calculating the regression; only the E and X without asterisks (whose elements are all means) were so used. Using the OLS coefficient vector b, we may calculate the (Tx1) vector v_i^* of prediction errors over time for individual i.

$$(12) \quad v_i^* = E_i^* - X_i^* b$$

We propose to use the standard error of the T elements of v_i^* (which we denote as $\hat{\sigma}_i$) as an estimator of σ_i, the standard deviation of unexplained fluctuations for individual i.

To evaluate the properties of our estimator, let β be the true coefficient vector so that the true vector of unexplained fluctuations u_i^*, is defined by

$$(13) \quad E_i^* = X_i^* \beta + u_i^*$$

What we really want to measure is the standard deviation of the elements of u_i^*, which is the i^{th} element of the diagonal of $\sigma^2 V$, i.e., $\sigma^2\sigma_i^2 = c\varepsilon(u_i^{*\prime}u_i^*)$. The prime (′) denotes the transpose and the c is a proportionality factor accounting for the correction for degree of freedom. (The σ_i entering the GLS estimation need be determined only up to a scale factor. Since, with the same number of time series observations for each individual, the degrees of freedom will be the same for all, so will c. Henceforth, we will assume that c is absorbed into σ^2 and write $\sigma^2\sigma_i^2 = E(u_i^{*\prime}u_i^*)$.) Now consider the sum of squared prediction errors using the OLS coefficient estimates.

$$\begin{aligned}(14) \quad v_i^{*\prime}v_i^* &= (E_i^* - X_i^* b)'(E_i^* - X_i^* b) \\ &= [X_i^*(\beta - b) + u_i^*]'[X_i^*(\beta - b) + u_i^*]\end{aligned}$$

where the last step uses (13). Our proposed estimator $\hat{\sigma}_i$, is proportional to $v_i^{*\prime}v_i^*$. Take the expected value of both sides of (14), remembering that in our model, $\varepsilon(u_i^*) = 0$.

$$(15) \quad \varepsilon(v_i^{*\prime}v_i^*) = \varepsilon[(\beta - b)'X_i^{*\prime} X_i^*(\beta - b)] + \varepsilon(u_i^{*\prime}u_i^*)$$

Therefore, $(v_i^{*\prime}v_i^*)$ is not an unbiased estimator of $\varepsilon(u_i^{*\prime}u_i^*)$. The expected value of the squared prediction error, $\varepsilon(v_i^{*\prime} v_i^*)$, equals the true variance of earnings fluctuations, $\varepsilon(u_i^{*\prime} u_i^*)$, plus a term accounting for the error in the estimation of b. Indeed, if we are to construct an estimator, $\hat{V}$, of the variance-covariance matrix $\sigma^2 V$ based on the measured errors v_i^*, the covariances would not all equal zero.

$$(16) \qquad \varepsilon(v_i^{*\prime} v_j^*) = \varepsilon[(\beta - b)' X_i^{*\prime} X_j^* (\beta - b)] + \varepsilon(u_i^{*\prime} u_j^*)$$

Although the true errors are uncorrelated, i.e., $\varepsilon(u_i^{*\prime} u_j^*) = 0$, $i \neq j$, the error in the estimation of b again contributes a non-zero term.

Now examine the term leading to the bias in equation (15), noticing that the matrix in brackets turns out to be just a scarlar.

$$(17) \qquad \varepsilon[(\beta - b)' X_i^{*\prime} X_i^* (\beta - b)] = \varepsilon[\mathrm{tr}(\beta - b)' X_i^{*\prime} X_i^* (\beta - b)]$$

$$= \varepsilon[\mathrm{tr}(X_i^{*\prime} X_i^*)(\beta - b)(\beta - b)'] = \mathrm{tr}(X_i^{*\prime} X_i^*)\varepsilon[(b-\beta)(b-\beta)']$$

Here tr stands for trace and the last step follows since X_i^* is a set of given independent variables. The expression $\varepsilon[(b-\beta)(b-\beta)']$ is the variance-covariance matrix of the OLS coefficient vector obtained in the cross-section estimation of equation (10). Although the OLS estimator is not best linear unbiased in the presence of heteroscedasticity, it is unbiased and consistent, with the property that every term in the variance-covariance matrix approaches zero as n (the size of the cross-section sample) gets very large.[5] Therefore,

$$(18) \qquad \operatorname*{plim}_{n\to\infty} \varepsilon[(\beta - b)' X_i^{*\prime} X_i^* (\beta - b)] = \mathrm{tr}(X_i^{*\prime} X_i^*) \operatorname*{plim}_{n\to\infty} \varepsilon[(b-\beta)(b-\beta)']$$

$$= \mathrm{tr}(X_i^{*\prime} X_i^*)[0] = 0.$$

The matrix $(X_i^{*\prime} X_i^*)$ is unaffected by the probability limit since it measures the variables for a single individual, which are unaffected as more individuals are added to the sample.

From this it follows that

$$(19) \qquad \operatorname*{plim}_{n\to\infty} \varepsilon(v_i^{*\prime} v_i^*) = \varepsilon(u_i^{*\prime} u_i^*)$$

Our estimator is thus an asympototically unbiased estimator of the true variance as the sample size of the cross-section becomes very large. In a similar way, we could demonstrate that all the co-variances given by equation (16) approach zero in the probability limit.

We know that the variance-covariance matrix of the u_i^* is given by

$$\varepsilon(u_i^* u_i^{*\prime}) = \sigma^2 V.$$

Another way of looking at this result is that the expected value of the variance-covariance matrix of the v_i^*, $\varepsilon[\hat{V}]$, is a consistent estimator of $\sigma^2 V$. Our sample estimator of the standard deviation for individual i, $\hat{\sigma}_i$, is proportional to the square root of the i^{th} diagonal element of $\hat{V}$. It can be easily established that GLS estimation of the mean earnings equation, based on the matrix $\varepsilon[\hat{V}]$, provides consistent estimates of all parameters.

We are still not prepared for the GLS procedure since consistent estimation requires us to use $\varepsilon(\hat{\sigma}_i^2)$ as our estimate of individual variance, not $\hat{\sigma}_i^2$ which we can calculate. Indeed $\hat{\sigma}_i^2$ is estimated from a time series for each individual, but the time series is short. There should thus be substantial error in the measurement of $\hat{\sigma}_i^2$, so its actual value will often be far from its expected value. Our measure of $\hat{\sigma}_i$, however, does allow us to proceed with one of our chief purposes, which is to investigate what determines the standard deviation of unexplained fluctuations in earnings. To do this, we will identify a series of variables, some related to the labor market situation of each individual and others related to his tastes and abilities. Denote these variables by $Z_{1i}, \ldots, Z_{ri}$. We hypothesize that

$$(21) \qquad \hat{\sigma}_i = a_1 Z_{1i} + \ldots a_r Z_{ri} + w_i$$

where w_i is an error term. Assuming that the appropriate assumptions are satisfied, we apply OLS to equation (21). First, this allows us to study the determinants of the standard error of fluctuations. Second, using the estimated coefficients $\hat{a}_1,\ldots,\hat{a}_r$, we can calculate an estimate of $\hat{\sigma}_i$ free of the error w_i:

$$\hat{\hat{\sigma}}_i = \hat{a}_1 Z_{1i} + \ldots \hat{a}_r Z_{ri}$$

Given the properties of OLS, we know that $\hat{\hat{\sigma}}_i = \varepsilon(\hat{\sigma}_i)$. This step finally provides the standard errors that we use in the GLS estimation of the mean earnings equation.

In the summary, our procedure has the following steps:

1. Estimate the parameters of the cross-section mean earnings equation (10) by OLS.
2. Use the resulting coefficients to predict the period-by-period earnings for each individual; calculate the prediction error for each period; and take the standard deviation of these errors, $\hat{\sigma}_i$, for each individual.
3. Analyze the determinants of $\hat{\sigma}$ by means of OLS.
4. Use the coefficient estimates from step 3 to calculate $\hat{\hat{\sigma}}_i = \varepsilon(\hat{\sigma}_i)$.
5. Re-estimate the parameters of the mean earnings equation (10) using GLS by dividing each variable for individual i by $\hat{\hat{\sigma}}_i$.

CHAPTER V

FOOTNOTES

1. In a recent paper, Hall tried to account for differences in tastes by differences in the α's and in the $\overline{H}$'s. Although we do not investigate differences in tastes in these ways, we do use the interpretation of the $\overline{H}$'s as behavioral parameters rather than as technical constants. See Hall, Robert E., "Labor Supply and the Negative Income Tax Experiment," presented as part of the Brookings Panel on Social Experimentation, April 29 and 30, 1974.

2. Division by $(1-t_E)$ in the transfer program case converts an equation for after-tax earnings to one for pre-tax earnings. This is reasonable in that earnings are measured before the tax. In fact, the tax appears to the family as a reduction in G rather than as a sum removed from its income. It is only for analytical purposes that we speak of it as a tax on earnings. Note that the behavioral consequences of the model are completely unchanged by this transformation.

3. Actually, data are not available on all relevant aspects of the AFDC-UF program. For example, a large work expense allowance can provide a sizable benefit even to a person with earnings, making AFDC-UF more attractive to this person than NIT, which does not have a similar allowance. However, the work expense allowance is calculated on a case-by-case basis. We do not have data on it for our families. There may also be subjective factors affecting the relative attractiveness of programs, like more inconvenience or stigma attached to one program or the other. All we can do is observe the actual choice of the family and use only the readily calculable parameters of the program -- the guarantee level and tax rate.

4. Some unemployed workers may have an unreasonable expectation, holding out for a wage they cannot attain. At a realistic wage, they might prefer not to work. Such individuals are hard to classify even in principle.

5. The variance-covariance matrix approaches zero as n approaches infinity, provided the matrix X continues to have rank k; if all second-order moments of the independent variables remain finite; and provided the true diagonal matrix V remains finite. See Theil, Henri, Principles of Econometrics, 1971, pp. 362-3.

CHAPTER VI

Empirical Results on Earnings

As Chapter II demonstrated, there have been numberous studies of what determines the level of work effort or earnings, and also how this level may differ from one group to another, or may change in response to a stimulus like the negative income tax experiment. However, there has been little attention to how earnings fluctuate over time (although there was the important Hall contribution on fluctuations in employment[1]). Both the Michigan and Wisconsin data sets are longitudinal and thus provide a basis for investigating the pattern of earnings over time. We thus begin in Part A with our results on fluctuations in earnings. We then present the results of our approach to the previously studied problem of explaining the average level of earnings in Part B of this chapter.

A. Fluctuations in Earnings

1. Specification of the Fluctuation Model

We showed in Chapter IV that the mean earnings of an individual will depend on his own wage, the unearned income of his family, and possibly the wage of his spouse. Therefore, a change in any of these variables leads to a change in his mean or expected earnings. But then fluctuations in earnings resulting from these variables are accounted for fully by the mean earnings equation. Fluctuations are worthy of separate study only to the extent that they are caused by factors not accounted for in the mean earnings equation. To review our measure of fluctuations, we first calculate preliminary estimates of the coefficients of the mean earnings equations (either (10) or (1) of Chapter IV). These coefficients are then used to predict the earnings of each individual for each time period. These predicted earnings are our measure of the "explained" portion of actual earnings accounted for by the wage rates and unearned income. The "error" in prediction -- the difference between actual and predicted -- measures the remaining part of earnings which must be explained by other variables. We assume that, on the average, predicted earnings will equal actual earnings, so that the prediction errors can be thought of as fluctuations about the mean. For each individual, we then take the standard deviation of the period-by-period prediction errors. Our measure of earnings variability for individual i is thus the standard deviation, σ_i, of those fluctuations in his earnings not explained by the mean earnings equations. Although we cannot explain each fluctuation, we assume that there is some regularity to them over time which can be detected by our measure σ_i. (Our preliminary OLS estimates of the mean earnings equation used to calculate this standard deviation are presented in Part B together with our later refined estimate of that equation.)

Given the measure of variability, σ_i, we proceed to investigate what determines it and how it differs from one individual to another by using regression analysis. Thus we must specify the independent variables that explain $\hat{\sigma}_i$. We expect variability to differ from one individual to another, first because of differences in labor market situation, and second because of differences in preferences concerning the timing of work effort. The labor market situation of an individual may depend on factors external to himself, like unexpected fluctuations in the demand for the product he produces. On the other hand, his skills, ability, or personality might affect his labor market opportunities. A person may have an unstable work record because successive employers found undesirable traits in him. Unfortunately, if we observe individual characteristics that significantly affect earnings variability, these characteristics may matter either because they reflect the individual's tastes for variability or because they influence an employer's judgment of the individual. Thus it will often be impossible to determine the reason a variable matters or whether the earnings variability is voluntary or not. It should be clear that our equation to explain fluctuations in the end can provide only a statistical description of what variables account for $\hat{\sigma}_i$. We cannot deduce much about causality. What we can discover is how $\hat{\sigma}_i$ differs systematically between people with different measurable characteristics.

The distinction between labor market and taste factors is useful primarily in motivating the search for measurable explanatory variables. Consider first involuntary factors resulting from the labor market situation of the individual. Since wages are often inflexible, changes in the demand for labor tend to lead to changes in employment. Different industries face different conditions in the markets for their own outputs. Some industries operate seasonally, while others operate throughout the year; some indutries are strongly influenced by the ups and downs of the aggregate economy, while others are only mildly so. These patterns in output are likely to influence the demand for labor and thus also actual employment. To test for such effects, we will use a set of industry dummy variables in our regression equation. These variables will have a value of 1 if the individual works in the industry, 0 if he does not. A positive, significant coefficient for some industry would mean thata workerwho works in that industry would have a significantly higher value of $\hat{\sigma}$ than a person who did not. The magnitude of the coefficient would give a measure of how much higher it would be. Since each industry (except for one used as a standard of comparison) requires a separate dummy variable, and since there is a limit to the number of variables that can be successfully introduced into an equation, we are not able to use the three digit classification of industries, but must rely on a limited number of broad industrial categories which will be described in conjunction with our results.

Given fluctuations in the demand for its product, a firm will adjust its use of labor, but different occupations will be affected in different ways. A firm may be particularly reluctant to give up certain skills and then have to rehire qualified workers again. In some occupational categories, the firm may try to minimize fluctuations, while letting them proceed apace in others. In addition, it simply may be easier to economize on one kind of labor than another. To account for differences in $\hat{\sigma}$ due to occupation, we introduce a set of occupational dummy variables similar to the industry dummy variables. In a similar way, also, we must rely on a limited number of broad occupational categories. It is conceivable that a given occupation may have more fluctuations in one industry than another, but we do not attempt to detect such industry-occupation interaction effects.

Although we introduced the industry and occupation variables as measures of involuntary effects from the demand side of the labor market, there is some ambiguity in interpreting these variables. For the individual had to choose his industry and occupation. It is conceivable that a person with a taste for variability might choose an occupation with a variable employment pattern. More serious is the possibility that inherently unstable workers are pushed out of more stable occupations or industries (if they even gain employment there in the first place), and can find employment only in unstable jobs. Doeringer and Piore carry such an argument further by claiming that the labor market opportunities open to some people are severely limited to begin with. These limits, together with other social conditioning factors, may help make the person an unstable worker.[2] Our tests cannot distinguish to what extent the Doeringer and Piore hypothesis is true.

The industry and occupation dummy variables are based on the first recorded industry and occupation for each individual. Over time, as the person changed jobs, he may also have changed industries or occupations. Without detailing the individual changes, we characterize a person's job history by measuring the number of his job changes. (In Chapter IV we did attempt to fill in more of the details in the circumstances of job changing.) Although job changes are not the only source of earnings fluctuations, they are likely to be related to $\hat{\sigma}$. What we want to discover is how much of the variability in earnings is due to an unstable job history. Of course, interpreting the job change variable is difficult, for we cannot distinguish whether the changes are voluntary or involuntary. The job change variable measures the observed fact, but does not convey information concerning the reasons.

Labor market conditions vary by locality. To discover the extent that such differences affect the pattern of earnings, we introduce location dummy variables, in the Michigan data for regions, and in the Wisconsin data for cities. Even here there is some possibility of ambiguity in interpretation since tastes could differ by region or more people in one locality could have labor market problems than in another.

Another set of variables reflects individual characteristics of the worker. These characteristics may affect the ease of finding work or the kind of job attainable. On the other hand, individual tastes may also vary with these factors. We include the age of the worker, measures of his education and training, as well as of health, disability, and disfigurement. We treat male and female fluctuations separately, and for each, we further separate people by race. We thus distinguish the effects of race and sex by running separate regressions for each race-sex combination. Some variables may reflect the family situation of the worker. We include the presence of the spouse and the number of children. Especially in the case of females, these variables could affect the desire of the woman to remain at home. We want to check whether the effect is strong enough to affect the standard deviation of earnings.

Finally, we include a few variables to discern the effects of the economic situation of the family on members' earnings patterns. Greater family income or assets could induce one family member to seek more leisure. Previous writings suggest that female workers are likely to be influenced more than male workers in reducing work effort. We want to oberve whether the pattern of earnings also is affected by the presence of other family income. We thus include unearned income, a measure of assets and the earnings of the spouse. One of the larger components of unearned income for families with spells of unemployment is unemployment insurance, which we introduce separately.

2. Results for the Variations in Male Earnings

a. Wisconsin Data

Regression results for the standard deviation of unexplained fluctuations, $\hat{\sigma}$, in male earnings are presented in Table VI-1, by race. The variables used are explained in the notes to the table. In this and all other tables presenting regression results, t ratios are given in parentheses below the appropriate coefficient. The table reports the results of stepwise regressions, where variables are added only as long as they increase the value of R^2 by .01. Thus, of all the variables tried, only those contributing to the explanatory power of the regression are reported. The dependent variable, $\hat{\sigma}$ measures in effect the average deviation of real earnings above and below the mean level in 1967 dollars. The coefficients indicate how changes in the independent variables will affect $\hat{\sigma}$. For example, the wage coefficient for whites in line 2 means that an increase in the real wage of $1 an hour will increase the average fluctuation in earnings by $116.9 per quarter.

One of the more interesting results is the significance of the wage rate in the equation for whites. Although it is not significant for the Spanish-surname or Black males, we will find it significant for both whites and Blacks and other minorities in the Michigan sample. Also the positive coefficient means that a higher wage rate is associated with more variability in earnings. There are several possible explanations of this result. First, it could be a statistical error. For $\hat{\sigma}$ is calculated from the prediction errors of the mean earnings equation. But the prediction of mean earnings depends on the wage rate. Thus, misspecification of the mean earnings equation could produce a positive relationship between $\hat{\sigma}$ and the wage rate if, for example, mean earnings actually increased with the wage rate at an increasing rate rather than linearly. The wage effect could also be explained in the absence of any statistical error.

For example, suppose that work effort diminshes as the wage rate rises (backward bending supply curve.) Since the work week often has a standard length, the easiest way to diminish work effort may be to take periodic breaks in employment, thus increasing the variability in earnings. We have already seen in Chapter III that in our sample, those with the highest wages tend to have a lower average of hours worked, a relationship attributable mainly to the truncation of the sample rather than to a backward bending supply curve. Actually, the likeliest explanation of the positive coefficient is probably a mechanical one. When a worker loses his job, the fall in earnings is larger the higher his wage rate. If high wage workers lose their jobs as frequently as low wage workers, there will be a positive relationship between the wage rate and the variability in the earnings.

The strongest variable for each race is transfer payments other than NIT and AFDC-UF. This includes social security, pensions, unemployment insurance, workman's compensation, veteran's disability, etc., although we have no breakdown of these separate components. The partial correlation coefficient of this term is .064 for whites, .092 for those with Spanish-surname, and .143 for Blacks. Although we tried several other income and wealth terms, none emerged significantly in the regressions. We tried at various times assets, earnings of the spouse and a measure of unearned income excluding the types of transfers just enumerated. (All were means over time). It is likely that the importance of the coefficient of other transfers stems from the inclusion of unemployment compensation. Unfortunately, there is reason to believe that the coefficient is biased. Coefficients will be biased if an explanatory variable is correlated with the error term of the regression equation. To see that the coefficient of transfer payments is likely to be correlated with the error term, trace the effect of an increase in error. With given

TABLE VI-1

Male Equations for the Variability of Earnings ($\hat{\sigma}$):
Regression Results for the Wisconsin Data

Variable	White	Spanish-surname	Black
1. Transfers (except NIT, AFDC-UF)	.6486 (5.02)***	.9451 (4.09)***	1.1470 (6.42)***
2. Wage	116.8881 (4.18)***		
3. Construction	136.9939 (2.74)**		
4. Transportation, Communications, Utilities	123.7470 (3.23)**		
5. Service Industries		-62.9351 (-1.70)	
6. White Collar			-244.2823 (-3.36)***
7. Self-Employed			298.4485 (3.03)**
8. Unknown Occupation			-436.3242 (-2.92)**
9. One Job Change	79.8185 (2.26)*	71.9583 (2.12)*	
10. Two or More Job Changes	99.3646 (3.33)***	124.2673 (3.42)***	137.5688 (3.67)***
11. Health	66.8510 (2.73)**		
12. Trenton		140.6244 (2.64)**	
13. Jersey		39.5500 (1.34)	-70.4193 (2.39)*
14. Scranton			-194.3154 (-1.60)
15. Control		-113.5678 (-3.73)***	
16. Age			-4.8808 (-2.96)**
17. Constant term	-179.563	304.031	559.048
R^2	.29	.39	.35
Number of Observations	288	119	196
F ratio	16.40	10.00	12.77

TABLE VI-1

NOTES

Numbers in parentheses are t ratios.

***denotes signficance at .001 level; ** at .01 level; * at .05 level.

The OLS coefficients of the earnings equation used in calculating $\hat{\sigma}$ appear in Table VI-7.

Only males with positive $\hat{\sigma}$ included in regressions.

Definitions of Variables

1. Transfers: mean over time of work conditioned transfer income (social security, pensions, unemployment insurance, workman's compensation, veteran's disability, etc.), but excludes welfare or public assistance, food stamps, and NIT; in 1967 prices.
2. Wage: mean over time of male "predicted wage" (calculated by Poirier and Watts and included on Analysis Tape of Wisconisin Graduate Income Experiment) deflated by price index (1967 = 100). Mean calculated over periods for which male present and for which positive "predicted wage" available. The "predicted wage" is used throughout this chapter rather than the actual wage in the hope that any endogenous component is thereby removed.

3-15. Dummy variable equal to one in indicated circumstance, 0 otherwise. First recorded industry or occupation used for 3-8.

5. Service Industries: financial services, business and repair services, peronsal services, entertainment and recreation services, professional and related services, public administration.

6. White Collar: professional and technical; managers, officials and proprietors; clerical and sales workers.

11. Health: equals one if Elesh health variable on Analysis Tape ever indicates unhealthy.

15. Control: family assigned to control group.

Variables included in stepwise regression, but not appearning in table because of insignificance:

Trade (wholesale and retail); service workers; years of schooling; participant in training programs; number of children under 5; days of work lost due to illness; unearned income of the family (excluding the transfers in variable (1), but including an exogenous measure of welfare or NIT); earnings of the wife.

values of the independent variables, the increased error increases $\hat{\sigma}$. A higher variability might arise from loss of a job. If that is the case, the person may be eligible for unemployment insurance so its amount will rise. Therefore, an increase in the error term may lead to an increase in unemployment compensation. It follows that the coefficient of transfer payments will be biased upward, overstating the true effect of this variable. In other words, the problem is one of mutual causation. If unemployment insurance is more or less a mechanical response to unemployment, the fluctuations cause the UI so that our entire effect is spurious. UI should not appear as an explanatory variable for the variation in earnings. If, on the other hand, fluctuations in earnings are larger because people know they can fall back on a UI system, then it must play a role in the explanation of earnings fluctuations. There has been some recent literature arguing that the design of the current UI system encourages instability in earnings.[3] To the extent that this is true, our coefficient, although exaggerated because of the bias, has some validity. We have not been able to measure the extent of the bias in the coefficient. It seems clear that a prime task for future studies of earnings fluctuations will be to elucidate the effects of the unemployment insurance system on earnings stability.

Now consider the effects of the industry and occupation dummy variables. Whites in construction and the grouping of transportation, communications, and utilities have significantly higher variability in earnings than other workers. Among Spanish-surname workers, there may be less variability in the service industries, a result significant only at the 9% level, while other industries and occupations do not seem to differ significantly. Blacks working in white collar industries have significantly less variability, while self-employed Blacks have significantly higher values of $\hat{\sigma}$. There is, in addition, significantly less variability among Blacks of unknown occupation. These are for the most part people with recorded earnings at some time, but who did not work most of the time. The standard deviation of a series of zeros and a couple of small positive elements will be small. It should be remembered that all of the Wisconsin data come from cities in the same region of the country. In different areas, people might work in different industries with differing patterns of employment. Thus, our specific results concerning occupation and industry should not be generalized to males living in other areas. In spite of all the peculiarities possible in individual earnings patterns, we, nevertheless, do find some regularities among individuals, even though we have used very broad industrial and occupational categories. It may be concluded that industry and occupation do help explain differences between individuals in earnings patterns, but the specific features of the relationship are likely to depend on the particular sample studied.

Lines 9 and 10 of Table VI-1 show that individuals with a history of job changes have significantly more earnings variability. For all races, those with two or more job changes have significantly higher values of $\hat{\sigma}$, but the effect is stronger for Spanish-surname and Black individuals than for whites. Among white and Spanish-surname males, there is also a significant effect from just one job change, although the addition to $\hat{\sigma}$ is smaller than for those with two or more job changes. From line 11, unhealthy whites have significantly more variability than healthy ones, but a similar effect is not detected for other groups. Unhealthiness could affect $\hat{\sigma}$ in two ways: a person who is ill much of the time may seldom work, giving him a low $\hat{\sigma}$ while a person afflicted by illness intermittently may have a high $\hat{\sigma}$. Apparently, the two types of effects may counterbalance each other among Spanish-surname and Black individuals. The age variable in line 16 is signficant only for Blacks. The negative coefficient means that earnings variability is high amoung young Blacks, but tends to decline with age. Lines 12 through 14 indicate significant differences in variability between the cities of the experiment for Blacks and those with Spanish-surname, possibly reflecting differences in overall labor market conditions facing these two groups.

There remains the important problem of whether or not the transfer system affect the variability of earnings. We tried using parameters of the transfer system as explanatory variables. For example, we used the guarantee level facing each individual.[4] Introducing it with various combinations of other variables, it was never significant. We also tried the breakeven level -- the guarantee divided by the tax rate faced by each individual -- but with similar results. We tried dummy variables based on some of the NIT experimental treatment groups (which differ by tax rate and guarantee level) and again found no effect. Of course, differences in rules and administrative procedures of welfare programs could have important effects. There are significant differences between NIT and AFDC-UF in that it is easier as we have noted, both to get on and to stay on NIT. We thus introduced a dummy variable equal to one for members of the control group and zero for others. This variable was strongly significant for those with Spanish-surname implying a lower value of $\hat{\sigma}$ for Spanish-surname members of the control group. One interpretation is that the various restrictions in the AFDC-UF program made it less satisfactory compared to NIT as a cushion of support in the face earnings variability; AFDC-UF recipients thus tried to avoid variations in their earnings. It is not clear why the other groups were not similarly affected.[5] For whites and Blacks, then, we have not been able to detect an effect of the transfer system on the variability of earnings. For the Spanish-surnamed we find an effect, although the interpretation is uncertain.

Now consider the overall quality of our estimates. The values of R^2 are perhaps somewhat low, but the F ratio reveals significance for each regression at less than .001 level. Indeed, it is not surprising that much unexplained variance remains. The $\hat{\sigma}_i$ used as the dependent variable for each individual is the standard deviation of a time series of only thirteen elements. Moreover, there is considerable error in measuring the period-by-period earnings figures. Our earnings data are earnings for the last week in the quarter, multiplied by thirteen (the number of weeks in a quarter) to put them on a quarterly basis. Weekly earnings are only imperfectly related to quarterly earnings, which is what we are trying to explain.

Much of the unexplained variance is thus a consequence of errors in measurement. But, in addition, there are unique aspects to the earnings pattern of each individual which will never be explained by a few easily measurable variables. Given the intricacies in studying earnings patterns, the problems with measurement, and the unique elements of each individual's situation, the surprising thing is perhaps that we explained as much of the variance as we did.

To show the consequences of these results, Table VI-2 presents predicted values of $\hat{\sigma}$ for various types of individuals. We take as our standard of comparison in line 1 the value of $\hat{\sigma}$ for a laborer in a manufacturing industry in Patterson earning $3.80 (the mean wage for whites) who has never changed jobs, is healthy, 40 years old, in the experimental group, and who has not received unemployment insurance. Each other line presents the value of $\hat{\sigma}$ for an individual identical with the standard individual except in the one characteristic listed on that line. Each number in the table should be interperted as the average deviation per quarter, measured in dollars, of a person's actual earnings from his mean earnings. For example, a standard Black male (line 1) has a $\hat{\sigma}$ of $364, indicating that in the average quarter, the fluctuation in his earnings is $364 above or below the mean level of this earnings. Notice that except for construction workers and those with health problems, whites generally are predicted to have the lowest variability. On the other hand, Blacks tend to have the highest predicted variability, except for older workers and white collar workers.

b. Michigan Data

The Michigan data include five annual observations on most variables for each household. Each annual earnings figure covers the entire year, not, as in the case of the Wisconsin earnings series, just the last week of the period. This reduces one type of measurement error. The Michigan data permit the study of annual fluctuations in earnings rather than the quarterly fluctuations studied with the

Wisconsin data. This could be a disadvantage if fluctuations are of short duration. Consider, for example, a person who has several short fluctuations each year. If the pattern is the same each year, annual earnings will show little change even though the person has a volatile earnings pattern. There is a better opportunity to detect short run fluctuations using quarterly rather than annual data, but the annual data should uncover longer lasting fluctuations. As one final element of comparison, the Michigan data are based on a national sample, while the Wisconsin data are all from the same area. The national sample is likely to include more varieties of experience. On the other hand, if earnings patterns differ by locality, a nationwide cross-section which portrays the "average" pattern may not give a satisfactory picture of what determines earnings variability in any particular locality. It is necessary to remember these points in comparing results from the two data sets.

Regression results for the variability in male earnings are presented in Table VI-3. In the Michigan data, earnings are measured in hundreds of dollars so the standard deviation has the same units. For the Michigan data, we present separate results for two categories, 1) whites and 2) Blacks and other minorities. The variables are explained in the notes to the table.

As with whites in the Wisconsin sample, the wage rate is strongly significant, now for both racial categories. Unemployment insurance is now measured separately and is also a strongly signficant variable for both groups just as it was in Wisconsin. Again, there is reason to believe that an unknown part of its effect is the result of statistical bias. In contrast to the Wisconsin results, we now find, for whites at least, a significant impact of the economic situation of the family on the pattern of earnings of the male. For example, line 3 shows a significant positive effect of unearned income other than UI on the variability of earnings. This means that more unearned income in the household encourages greater variability in male earnings. The unearned income measure includes the guarantee level of the transfer payment system for recipients, so this result suggests that the welfare system may encourage

TABLE VI-2

Predicted Values of $\hat{\sigma}$ for Males, Wisconsin Data
(Based on coefficients in Table VI-1)

Each individual is identical with standard individual, except in the one characteristics listed.

	White	Spanish	Black
1. Standard	265	304	364
2. Construction Worker	401	304	364
3. White Collar Worker	265	304	120
4. Changed Jobs Twice	364	428	502
5. Unhealthy	331	304	364
6. Age 20	265	304	461
7. Age 60	265	304	266
8. Average UI Benefit over time of $100	329	399	479
9. Control group	265	190	364

Standard individual: laborer in manufacturing in Patternson, 40 years old, healthy, no job changes, experimental group, never received UI, earned $3.80 an hour.

TABLE VI-3

Male Equations for the Variability of Earnings:
Regression Results for the Michigan Data

Variable	White	Blacks and Other Minorities
1. Wage	2.8448 (6.82)***	2.2504 (6.54)***
2. Unemployment Insurance	.8718 (3.82)***	1.1469 (6.36)***
3. Other Unearned Income	.0802 (3.75)***	
4. Earnings of Spouse	.0906 (2.44)*	
5. Number of Periods Spouse Present		-.5322 (2.88)**
6. Age	-.1163 (-3.85)***	
7. Number of children	-.7406 (-4.26)***	
8. Transportation, Communications, Utilities	4.2346 (3.05)**	2.4549 (-2.31)*
9. Farm Work	3.1212 (2.88)**	
10. Army	6.0835 (2.33)*	
11. Durable Manufacturing	-2.2999 (-2.14)*	
12. White Collar		5.2876 (4.22)***
13. Disability		1.8894 (3.13)**
14. Constant	7.212	3.482
R^2	.32	.25
Number of Observations	335	454
F ratio (d.f.)	15.20(10;324)	24.33(6;447)

TABLE VI-3

NOTES

The OLS coefficients of the earnings equation used in calculating $\hat{\sigma}$ appear in Table VI-8.

Only males with positive $\hat{\sigma}$ included in regressions.

Definitions of Variables

1. Wage: mean over time of average hourly earnings for each year, deflated by consumer price index (1967 = 100). Mean calculated only over those periods for which male was present and had positive earnings.

2. Unemployment Insurance: mean over time of UI benefits, deflated by price index.

3. Other Unearned Income: calculated in same way as Unearned Income, Table VI-8, except UI excluded.

4. Earnings of spouse: mean real earnings of spouses. Value of zero assigned in each period when spouse did not work or was not present.

8-13. Dummy variables.

12. White Collar: professional, technical; managers proprietors; self-employed; clerical; sales.

13. Disability: equals one if ever any indication of disability.

Variables included in step-wise regression, but not appearing in table because of insignficance: Craftsmen and foremen, operatives; non-durable manufacturing; construction; trade (retail and wholesale); government worker; high school graduate; recipient of job training; disfigured; number of job changes; North central region; South; West; mean over time of guarantee level in state of residence, corrected for family size.

instability in white male earnings in the Michigan sample. A possible interpretation is that white males tolerate (or choose) more variability in their earnings as long as they know there is other income available to the household. Consistent with this view is the significantly positive coefficient on line 4 for the earnings of the spouse, again, just for whites.

Whereas family income may encourage variability in male earnings, other aspects of the family situation of a male seem to limit that variability. For Blacks and other minorities, variability is lower the longer the wife is present, shown by the negative coefficient on line 5. For whites, the number of children seems to impose a similar pressure in restraining variability, indicated by the negative coefficient on line 7.

For whites, variability declines with age. Various industries and occupations affect variability. Variability is higher among whites working in transportation, commmunications, utilities, agriculture, and among those who were in the Army at an early time in the survey. It is lower in durable manufacturing. Among Blacks and other minorities, variability is lower in transportation, communications, and utilities, but higher for white collar workers. Disabilities increase variability among non-whites.

All regressions are significant at the .001 level. The values of R^2 are similar to those from the Wisconsin equations.

3. Female Equations

a. Wisconsin data

In both the equations for mean earnings and for the variability in earnings, we include only individuals who at some time had positive earnings. Since so many women in the Wisconsin sample never worked, we have a fairly small number of observations for our regressions. The number of Spanish-surname females who worked was too small to give significant results, so we combined Spanish-surname and Black females in a single category. Thus all female results using Wisconsin data are presented for just two categories. Results for the equations for the variability of earnings appear in Table VI-4.

TABLE VI-4

Female Equations for the Variability of Earnings:
Regression Results for Wisconsin Data

Variable	Whites	Blacks and Spanish-Surname
1. Wage	46.1328 (3.35)**	101.1981 (7.93)***
2. Unearned Income (Excluding UI)	-6.4588 (-4.01)***	
3. Assets		-4.3740 (-2.39)*
4. OneJob Change		68.1229 (2.13)*
5. Two or More Job Changes	71.7067 (1.99)	80.0956 (2.27)*
6. Operative	40.3200 (1.42)	79.6514 (3.26)**
7. Service Occupation	-62.7064 (-1.78)	
8. Control Group	-50.7284 (-1.69)	38.4936 (1.52)
9. Trenton	-150.9746 (-1.69)	
10. Jersey City		-64.4770 (-2.45)*
11. Scranton	-138.1891 (-4.56)***	
12. Constant	283.329	-12.744
R^2	.56	.48
Number of Observations	82	92
F ratio	11.43	10.87

TABLE VI-4

NOTES

The OLS coefficients of the mean earnings equation used to calculate $\hat{\sigma}$ appear in Table VI-10. Only women with positive σ are included in regression.

1. Wage: mean over time of female "predicted wage" (from Wisconsin Analysis Tape) deflated by Consumer price index. Mean calcualted over periods for which female present and for which positive "predicted wage" available. Calculated only for women who at some time during the survey worked.

2. Unearned income: excludes UI benefits but otherwise calculated in same way as unearned income in Table VI-10 (see notes to that table).

3. Assets: financial assets, in 1967 prices.

4-11. Dummy variables equal to one in indicated situation, otherwise zero.

Variables included in stepwise regression, but not appearing in table because of insignificance: Non-durable manufacturing; trade; service industries; unknown occupation; number of children under 5; received job training; number of work-days lost due to illness; UI; mean earnings of the husband.

As with males, the wage rate is strongly significant, with a positive coefficient for both racial categories. (Table VI-4, line 1). But there is a difference from the males in the effect of other family income. Whereas white males in the Michigan sample had positive coefficients for measures of other family resources, the Wisconsin female equations show negative coefficients for similar variables. For white females, unearned income (excluding UI) is strongly significant with a negative coefficient (line 2), while for Black and Spanish-surname females it is an assets variable that has the negative significant coefficient (line 3). Both of these results show that as the resources of the family increase, the variability of female earnings decreases. Since unearned income includes the guarantee level of the transfer system, apparently welfare may reduce the variability of female earnings. To understand this result, note that the standard deviation of a series will be small, either if the series is quite stable at a positive level or if the series stays at zero most of the time. Our theory of mean earnings (to be tested in Part B) suggests that an increase in unearned income will reduce mean earnings. The white males of the Michigan sample apparently accomplish the reduction in mean earnings by taking more breaks in employment. This shows up as greater variability, but it does so because they continue to work, only more sporadically. On the other hand, the results for Wisconsin females suggest that the discouragement from higher unearned income tends to induce them to leave the labor market altogether. For if earnings are reduced to zero much of the time, the variability in earnings becomes very small. Indeed, of the 82 white females entering the regression, 50 had positive earnings for less than six quarters out of thirteen, while the similar number out of the 92 Blacks and those of Spanish-surname was 54.

We find no effect of unemployment insurance in these female equations. The only other significant variable for whites is the dummy variable for Scranton. Among Blacks and those of Spanish-surname operatives have significantly higher variability as do those who change jobs, while residents of Jersey City have less.

b. Michigan Data, Female Heads

The negative income tax experiment in New Jersey and Pennsylvania covered only male-headed families, so the only families in our sample are those initially having a male head. In contrast, the Michigan data are based on a sample of the entire population and so include a sizable number of female-headed families. In studying female earnings in the Michigan sample, we treat two groups separately: (1) females who were family heads for the entire duration of the survey; (2) females who at some time during the survey were spouses. For each group, we separate further whites from Blacks and other minorities. We proceed now to examine the regression results presented in Table VI-5, for the female heads.

Note first that the explanatory power of the equations for this group, measured by R^2, is substantially lower than that for all other groups. The results are, nevertheless, interesting because of the strong contrast they show between the female heads of the Michigan sample and the female spouses of the Wisconsin sample in Table VI-4. First, the wage term is not significant for female heads in the Michigan sample. Second, no measure of other family income or assets is significant either. Thus the economic variables which might have reflected voluntary instability or withdrawal from the labor force for the Wisconsin female spouses do not seem to affect the patterns of the Michigan female heads. Unemployment insurance has a significant effect on Blacks and other minorities. The variables that do matter are some of the dummy variables for industry, occupation, and other personal characteristics.

c. Michigan Data, Female Spouses

The results for the female spouses of the Michigan sample are presented in Table VI-6. They are more similar to the Wisconsin female spouses than to the Michigan female heads. The wage is again strongly significant for both whites and Blacks and other minorities. There is no significant effect of a variable for other family income. However, the variable measuring the number of periods the head is present

TABLE VI-5

Female Head Regression Results for the Standard Deviation Of Unexplained Earnings, Michigan Data

Variables	Whites	Blacks and Other Minorities
1. Unemployment Insurance		1.1115 (2.99)**
2. Professional, managerial worker or self-employed	1.8129 (1.11)	6.1195 (4.01)***
3. Clerical Worker	2.2392 (2.04)*	2.9699 (3.63)***
4. High School Graduate	1.1979 (1.18)	
5. Job Training	3.1639 (2.63)*	1.2745 (2.53)*
6. Disability		-.7591 (-1.90)
7. West	1.4413 (1.43)	
8. Constant	2.800	4.669
R^2	.18	.14
Number of Observations	97	345
F ratio (d.f.)	3.96(5;91)	10.56(5;339)

NOTES

The OLS coefficients of the mean earnings equation used to calculate $\hat{\sigma}$ appear in Table VI-13. A woman is excluded from the regression unless its $\hat{\sigma}$ is greater than zero.

1. Unemployment Insurance: as in Table VI-3.

2-7. Dummy variables equal one in indicated circumstances, 0 otherwise.

Variables included in stepwise regression, but omitted due to insignificance: Operative; durable manufacturing; non-durable manufacturing; trade; government workers; age; number of children; disfigurement; number of job changes; unearned income excluding UI; North Central region; South; mean guarantee of welfare program in state of residence.

TABLE VI-6

Female Spouse Equations for the Variability of Earnings:
Regression Results for the Michigan Data

Variables	Whites	Blacks and Other Minorities
1. Wage	3.2483 (8.48)***	2.8823 (8.33)***
2. Number of Periods Head Present	-.7020 (-2.96)**	-.5866 (-3.89)***
3. High School Graduate		1.1036 (2.59)*
4. Professional, Managerial, Self-Employed	3.3403 (3.64)***	
5. Manufacturing	2.3332 (2.72)**	3.0672 (4.94)***
6. Trade	1.4652 (1.89)	
7. Clerical		-1.4243 (-2.01)*
8. Constant	3.022	2.905
R^2	.35	.29
Number of Observations	265	365
F ratio	28.43	29.77

NOTES

The OLS coefficients of the mean earnings equation used to calculate $\hat{\sigma}$ appear in Table VI-11.

1. Wages: mean over time of average hourly earnings in 1967 prices for years in which the woman worked.

3-7. Dummy variables equal to one in the indicated circumstance, zero otherwise.

Variables included in stepwise regression, but omitted due to insignificance: Operative; agriculture, forestry; government worker; age; number of children; unearned income, husband's earnings; North Central region; South; West; mean guarantee level faced by family.

is strongly significant, with a negative coefficient for both racial categories. This variable may reflect the same phenomenon as the unearned income term of the Wisconsin female equations. We argued there that other income in the household tends to induce women to leave the labor force, leading to low variability. If the husband is the source of other income, the more he is present, the more the wife can withdraw from employment. In contrast to the Wisconsin result, the fact that the presence of the husband rather than his earnings is the significant variable suggests the possibility that the inducement to remain home may be chiefly social or psychological rather than only economic.

B. Mean Earnings

In Chapter V we developed our equation for mean earnings which depends on the wage rate of the individual, the wage rate of his spouse, and a measure of unearned income. In the tests we performed of the mean earnings equations, the wage rate of the spouse never emerged as a signficant variable. Usually, it at least had the right sign in female equations, but not in the male equations. In contrast to the mean earnings equations, our results for the variability of earnings suggest the possibility of important effects of the husband on the variability of the earnings of the wife. Others, like Gronau,[6] have attempted to design more sensitive tests of intra-family effects. In view of our experiences with our mean earnings equations, we will present only results with the spouse's wage excluded from the regression. In section 1, we discuss the results of our mean earnings equations for each group studied and, in section 2, we examine the implications of the results for the question of how a transfer payemtns system affects earnings.

1. Discussion of Regression Results

a. Males

From the Cobb-Douglas formulation of the utility function in Chapter V, we deduced that in the mean earnings equation the coefficient of unearned income should

TABLE VI-7

Mean Earnings Equations for Males: Regression Results, Wisconsin Data

Variable	White OLS	White GLS	Spanish Surname OLS	Spanish Surname GLS	Black OLS	Black GLS
Male Wage	302.6792 (5.77)***	494.1040 (21.34)***	382.1943 (4.34)***	365.2786 (15.81)***	336.9751 (3.77)***	383.0920 (25.10)***
Unearned Income	-.1345 (-5.48)***	-.1041 (-4.74)***	-.0325 (-1.70)	-.0232 (-1.19)	-.0668 (-2.52)*	-.0430 (-2.13)*
Constant	167.93	-1.550	229.442	-.479	82.681	-.635
R^2	.18	.61	.15	.68	.09	.85
F ratio	31.75	227.81	10.97	131.08	9.91	582.14
Number of Observations	294		128		211	

OLS: ordinary least squares   GLS: Generalized least squares

TABLE VI-8

Mean Earnings Equations for Males: Regression Results, Michigan Data

Variable	White OLS	White GLS	Blacks And Other Minorities OLS	Blacks And Other Minorities GLS
Male Wage	16.1201 (19.65)***	19.8278 (44.92)***	17.3019 (30.06)***	22.9522 (45.91)***
Unearned Income	-.1125 (-2.57)*	-.1576 (-3.54)***	-.2160 (-7.17)***	-.0970 (4.12)***
Constant	8.682	.186	6.112	-.895
R^2	.49	.83	.64	.79
F ratio	149.11	1024.98	467.88	1056.45
Number of Observations	411	439	334	577

TABLE VI-7

NOTES

Only males with positive mean earnings included in regression.

Description of Variables

Mean earnings: mean over time of male earnings (including zero earnings) in 1967 prices. Mean computed over all quarters for which the man was present in the surveyed household.

Male wage: Same as Wage, Table VI-1.

Unearned Income: calculated for each quarter according to formula,

$$\frac{(1-t_Q)\ Q + G}{1-t_E} \qquad \text{(see equation (7), Chapter V).}$$

Then, the mean over time of these figures, is the variable used. Q is the income of the household, less earnings of the husband and wife, and less NIT or AFDC-UF payments, in 1967 prices. (It includes the earnings of other family members since we do not try to explain them, but treat them as if they were exogeneous.)

t_Q is the tax rate on unearned income, t_E is the tax rate on earned income, G the guarantee level of the transfer system, in 1967 prices.

For a person receiving neither NIT nor AFDC-UF, we assume $t_Q = 0$, $t_E = .05$ to allow for the social security tax on earnings, and G = 0. (We ignore the positive tax system since most of our sample will be little affected by it.)

For a person receiving NIT, $t_Q = t_E$ is determined by the experimental treatment group to which his family is assigned. However, the earnings tax is applied to earnings after deducting the social security tax. We thus use a denominator of .95 $(1-t_E)$. The guarantee as a percent of the poverty line is determined by the experimental treatment group. The poverty line is determined by the family size and number of spouses present. (See Wisconsin staff memo, "Key to Plans.").

For a person receiving AFDC-UF, $t_Q = 1$. Since we cannot measure work related expenses, we assume that the average effective tax on earnings is $t_E = .5$, which includes a deduction for social security taxes. The guarantee levels are those prevailing each period in the New Jersey and Pennsylvania AFDC-UF program, with adjustments for family size and number of spouse present. (Our information on AFDC-UF guarantees was obtained from The Home Economic Advisory, State of New Jersey, Division of Public Welfare, and from the Bureau of Policy, State of Pennsylvania, Department of Public Welfare.)

For a person receiving both in a quarter, we used the NIT parameters.

Note on price deflation: since the experiment ran over different periods of calendar time in the four cities, it is necessary always to deflate for each city separately.

TABLE VI-8

NOTES

Only males with positive mean earnings included in regression.

Description of Variables

Mean earnings: mean over time of male earnings (including zero earnings) in 1967 prices. Mean calculated over all years for which the man was present.

Male wage: mean over time of average annual earnings in 1967 prices. Mean calculated only over years for which average earnings positive.

Unearned income: calculated for each year according to formula,

$$\frac{(1-t_Q)\,Q + G}{1-t_E} \quad \text{(see equation (7), Chapter IV).}$$

Then, the mean over time of these figures is the variable used. Q is the income of the household, less earnings of the husband and wife, and less welfare payments, in 1967 prices.

For a person not receiving welfare, we assume $t_Q = t_E = G = 0$.

For welfare recipient, $t_Q = 1$. We assume that the average effective tax rate, allowing for work expenses, set-aside, and social security tax is $t_E = .5$. We obtain estimates of the guarantee level for a family of 4 for each state each year from Gertrude Litwin, "States' Methods for Determination of Amount of Grant for an AFDC Size of Four (1 Adult and 3 Children)," unpublished table, Department of Health, Education, and Welfare, January 1972; and from U.S. Department of Health, Education and Welfare, National Center for Social Statistics, NCSS Report Series D-2, "OAA and AFDC: Standards for Basic Needs for Specified Types of Assistance Groups."

To correct the guarantee for family size, we assumed in the first year a $40 extra payment for each individual above four in New York. The amount in other states is assumed to be the fraction of $40 equal to the ratio of their guarantee to the New York guarantee for a family of four. We then correct this figure in other years based on the growth in the guarantee level in each state.

In view of the complications in calculating actual benefits and the arbitrariness of our procuedures, it is not surprising that occasionally actual welfare payments to a family exceeded our estimate of the guarantee. In such cases we used the actual payment as a substitute measure of the guarantee.

be negative and between zero and minus one, while the coefficient of the wage rate should be positive. Examination of Table VI-7 for the Wisconsin data and Table VI-8 for the Michigan data reveal that all male equations satisfy these conditions. All coefficients are significant except that of unearned income for the Spanish-surnamed in the Wisconsin sample. The ordinary least squares (OLS) and generalized least squares (GLS) coefficients are similar, with the GLS wage coefficient somewhat higher and the unearned income coefficient somewhat lower than the corresponding OLS coefficient. The constant term is affected by the GLS technique. Compared to the OLS constant term, the GLS constant is very close to zero, conforming to our theoretical requirement that the constant term be zero. The biggest difference seems to be in the higher R^2 of the GLS equations, indicating that the equations for the variability in male earnings contribute substantially to the explanation of average earnings. In principle, the GLS estimates are preferred. We did indicate some possibility of bias in the earnings variability equations which could reduce the reliability of the GLS estimates. Nevertheless, we shall rely on them in our remaining discussions.

There are possibilities of bias in these results. Unemployment insurance is included in the measure of unearned income. As with the earnings variability equations, causality runs not only from UI to earnings but also the other way, creating bias in the coefficient estimates in two ways. First, UI is received because of unemployment when earnings are zero. The more a person is unemployed, the lower his mean earnings will be, but the higher his UI benefits. This produces a negative bias in the coefficient of unearned income. Second, UI benefits are higher, the higher a person's earnings before he lost his job. This means that higher mean earnings will be associated with higher UI, for a given length of time unemployed. But this creates a positive bias in the coefficient of unearned income. It is not possible to know which bias is stronger or whether they just balance each other.

To investigate our estimates further, it is useful to unscramble the coefficients. In Chapter V, we demonstrated that the coefficient of unearned income is - α_m, where α_m is the coefficient of leisure of the male in the Cobb-Douglas utility function. It measures the fraction of "full income" that the household chooses to devote to leisure. In other words, there is some maximum amount that the person could conceivably work, $\overline{H}_M$. The "full income" of the household is its income on the assumption that the male (and other household members too) work the maximum amount. In fact, the husband will ordinarily work less than $\overline{H}_M$ taking some of the family's full income in the form of leisure. The amount of income foregone for the sake of the leisure of the male is the fraction α_M of "full income." The coefficient of the wage rate was shown to equal $(1-\alpha_M)\ \overline{H}_M$. We argued that $\overline{H}_M$ should not be considered a technical constant, but rather is a behavioral parameter. It might measure the maximum amount an individual could be induced to work under the most favorable circumstances. Alternatively, $\overline{H}_M$ might reflect a limitation on the amount the individual is able to work due to external market forces. Individuals often cannot work the amount they want at the prevailing wage. In particular, we know that there was a substantial amount of unemployment in the cities of the NIT experiement, and this unemployment increased during the experiment. Blacks and Spanish-surnamed were particularly affected. To an extent, external market forces have already been introduced in the equations for earnings variability, but we then investigated only fluctuations in earnings about the mean. But the mean itself was reduced for the entire duration of the experiment because of external market forces. The depressed state does not show up in wage rates, which are inflexible downward, but in unemployment. A convenient way to introduce external market factors into the mean earnings equation is to assume that they play a role in determining $\overline{H}_M$. Thus, $\overline{H}_M$ should be interpreted as the maximum amount that a person <u>either</u> would be willing to work <u>or</u> would be able to work given current market conditions. From the actual coefficient estimates, we can deduce the values of α_M and $\overline{H}_M$ which are presented in Table VI-9.

For the Wisconsin sample, there are noticeable differences in α_M and $\overline{H}_M$ by race. α_M is highest for whites, lowest for those with Spanish surnames, and intermediate for Blacks. Whites would devote about 10% of "full income" to leisure while those with Spanish-surnames would give up only about 2%. Thus, the actual work effort of those with Spanish-surnames will be much closer to their maximum $\overline{H}_M$ than for whites, with Blacks in between. But now consider the estimates of $\overline{H}_M$. There are big differences here, with the largest value for whites and the smallest for Spanish-surname. Assuming a 40-hour week, a person would work 520 hours per quarter. But we estimate a maximum work effort for whites of 551 hours, 400 hours for Blacks, and 374 hours for those with Spanish-surnames. We cannot distinguish an $\overline{H}_M$ voluntarily low from one low because of market pressures. Nevertheless, our results are at least consistent with the idea that Blacks and those with Spanish-surnames had considerably more difficulty in obtaining employment during the NIT experiment.

For the Michigan sample, Blacks and other minorities had a lower value of α_M than whites, but a higher value of $\overline{H}_M$. A 40-hour week would lead to an annual work effort of 2,000 hours. The estimated values of $\overline{H}_M$ are considerably higher than this for both whites and non-whites. Apparently, unemployment was a less serious problem for those in the Michigan sample than for those in the Wisconsin sample. On the other hand, the estimates of α_M are higher from the Michigan sample. indicating that these people were perhaps somewhat more willing to devote their resources to leisure than those Wisconsin sample.

TABLE VI-9

Unscrambled Coefficients, Male Equations

	Wisconsin Sample[1]		Michigan Sample[2]	
	α_M	$\bar{H}_M$ (quarterly)	α_M	$\bar{H}_M$ (annual)
White	.1040	551	.1576	2,354
Spanish Surname	.0232	374	.0970	2,542
Black	.0430	400		

[1]Based on GLS coefficients, Table VI-7.

[2]Based on GLS coefficients, Table VI-8.

TABLE VI-10

Mean Earnings Equations for Females: Regression Results, Wisconsin Data

Variable	OLS
Female Wage	120.7832 (5.86)***
Unearned Income	-.0467 (-2.28)*
Constant	29.483
R^2	.17
F ratio	20.11
Number of Observations	200

NOTES

Only women with positive mean earnings included in regressions.

Mean Earnings: mean over time of female earnings (including periods of zero earnings), in 1967 prices.

Mean calculated only over quarters in which female present.

Female Wage: same as wages in Table VI-4

Unearned Income: same as in Table VI-7.

b. Females

The results for females are much less satisfactory than those for males. With the Wisconsin sample, the GLS estimates for females were completely unsatisfactory. In addition the ordinary least squares results for Blacks and those with Spanish-surnames were unsatisfactory. We need OLS coefficients in order to construct our measure of earnings variability, $\hat{\sigma}$. In order to estimate $\hat{\sigma}$, we treated the Wisconsin females as a whole in deriving OLS estimates, which are presented in Table VI-10. The coefficients have the right signs, and the coefficient of unearned income lies between zero and minus one. The explanatory power of the equation is poor.

For the Michigan sample, we did obtain OLS estimates for both white and Black and other minority female spouses (Table VI-11). The coefficients all satisfy the expected conditions, although the unearned income coefficient for Blacks and other minorities is not significant. The equation for non-whites is still poor, although we did use it in calculating $\hat{\sigma}$. The GLS estimates for both racial categories of female spouses were again unsatisfactory. The GLS estimates are based on predictions of $\hat{\sigma}$ calculated from the variability equations. Whereas for males in both the Wisconsin and Michigan samples GLS made a strong improvement, the GLS estimates for Michigan female spouses are decidedly worse than the OLS estimates. Thus, for female spouses in both the Michigan and Wisconsin samples, we must rely on the OLS estimates.

Unscrambled coefficients for female spouses are presented in Table VI-12. The reliability of these coefficients is questionable. However, the estimates of $\overline{H}_F$ are consistent with what might be expected. For the whole Wisconsin sample, $\overline{H}_F$ is 127 hours, compared to full-time quarterly work effort of 520. For the Michigan sample, $\overline{H}_F$ is 878 hours for whites and 899 for non-whites, compared to full-time annual work effort of 2,000. The maximum amount female spouses would be willing to work appears to be substantially less than the normal full-time effort. Female

TABLE VI-11

Mean Earnings Equations for Female Spouses:
Regression Results, Michigan Data

Variable	Whites OLS	Blacks and Other Minorities OLS
Female Wage	7.8346	4.9970
	(10.97)**	(7.54)***
Unearned Income	-.1053	-.0279
	(-2.93)**	(-1.03)
Constant	1.092	3.922
R^2	.30	.13
F ratio	64.92	28.92
Number of Observations	301	404

TABLE VI-12

Unscrambled Coefficients, Female Equations

	Wisconsin Sample[1]		Michigan Sample[2]			
	Female Spouse		Female Spouse		Female Head	
	α_F	$\overline{H}_F$(quarterly)	α_F	$\overline{H}_F$(annual)	α_F	$\overline{H}_F$(annual
White	--	--	.1431	878	.1053	876
Blacks and Other Minorities	--	--	.1976	899	.0279	514
Whole Female Sample	.047	127.				

[1]Based on OLS coefficients, Table VI-10.

[2]Female Spouse, Based on OLS coefficients, Table VI-11;
Female Head, based on GLS coefficients, Table VI-13.

NOTES TO TABLE VI-11

Only women with positive mean earnings included in regression.

Mean Earnings: mean over time of earnings (including periods of zero earnings), in 1967 prices. Mean calculated only over years in which female present.

Female Wage: mean over time of average hourly earnings of females, in 1967 prices. Mean calculated only over years in which average hourly earnings positive.

Unearned Income: same as in Table VI-8.

spouses may reach their limited target by part-time work. The major explanation of this result, however, is probably that the female spouses included in our regressions work for a period, but, then withdraw from the labor force altogether for the remainder of the time. Indeed, as discussed in Chapter V, the earnings of a person not in the labor force are not explained by the same linear equation that explains the earnings of workers. Our treatment of this difficulty was to assume that the earnings of anyone who never worked during the Michigan or Wisconsin surveys could not be explained by our linear model, so that such individuals were excluded from our regressions. For those who ever worked, any periods of zero earnings were considered temporary deviations away from a non-zero mean. That assumption seemed suitable enough when examining male earnings. Many of our females, however, are probably out of the labor market much of the time even though they do work at some time, so our treatment of them is probably inappropriate. While this reduces the reliability of the estimates the estimates do nevertheless provide a picture consistent with this view.

We have similar problems with the female heads of the Michigan sample. In Table VI-13, we do obtain satisfactory estimates from both OLS and GLS. All coefficients are significant and satisfy the appropriate conditions. However, the GLS estimates are slightly worse than the OLS estimates, indicating that the predicted values of $\hat{\sigma}$ from the variability equations do not contribute to the explanatory power of the mean earnings equations. Unscrambled coefficients for female heads appear in Table VI-12. As for female spouses, the estimates of $\overline{H}_F$ are far below full-time work effort.

2. The Impact of a Transfer Payment System on Mean Earnings

We can use our estimates of the mean earnings equations to predict the effects on mean earnings of changes in the guarantee level and tax rate of the transfer payment system, assuming no change in either the wage rate or unearned, non-welfare income, Q. Both the guarantee level, G, and the tax rates, t_E on earned income and t_Q

TABLE VI-13

Mean Earnings Equations for Female Heads:
Regression Results, Michigan Data

Variable	Whites OLS	Whites GLS	Blacks and Other Minorities OLS	Blacks and Other Minorities GLS
Female Wage	8.5004 (9.04)***	7.5229 (9.11)***	9.4317 (13.09)***	7.2120 (10.32)***
Unearned Income	-.1740 (-5.43)***	-.1431 (-5.37)***	-.2050 (-10.53)***	-.1796 (-11.30)***
Constant	6.572	1.464	8.192	2.193
R^2	.40	.36	.37	.32
F ratio	58.78	51.27	129.78	101.23
Number of Observations	182		442	

Variables same as Table VI-11. See Notes to that table.

on unearned income, appear in the unearned income term for a person receiving transfer payments. From equations (7) of Chapter IV, we know that the appropriate measure of unearned income, N, for a transfer recipient is

$$N = \frac{(1-t_Q)\ Q + G}{1-t_E}$$

where Q is the non-transfer, unearned income of the family. To determine the effect of a change in either G or one of the tax rates on earnings, we first calculate its effect on N. Then, the estimated regression coefficient of N gives the final effect on earnings. Notice that the effect of a change in G depends on the prevailing level of t_E. Similarly, the effect of a change in t_E depends on the existing level of the guarantee.

The New Jersey NIT experiment was designed to isolate the pure effect of various NIT plans. The intention was to determine whether an NIT in comparison to no transfer plan at all would affect earnings or work effort. As we discussed in Chapter II, this question could not be answered on the basis of the experiment because it was contaminated by the introduction of AFDC-UF. The effects of the various treatments can only be deduced from a non-experimental process of analysis. Our regression results provide a basis for such an analysis. Table VI-14 provides estimates of the difference in real earnings between a person receiving each of the eight experimental treatments and a person receiving no transfer payments at all. The initial 100% guarantee for a family of four in the experiment was \$3300, or \$825 on a quarterly basis. We will take this as our estimate of the real guarantee. The various guarantee levels for different experimental groups are then given as varying percentages -- 50%, 75%, 100%, 125% -- of this basic amount. Under the NIT experiment, the tax on

TABLE VI-14

Predicted Reductions in Quarterly Real Earnings Resulting from NIT Experimental Treatments in Comparison to No Transfer Payment, Wisconsin Sample

	Experimental Treatment		Male			Female
	Guarantee	Tax Rate	White	Spanish Surname	Black	
1.	50	30	$61	$14	$25	$28
2.	50	50	86	19	35	39
3.	75	30	92	21	38	42
4.	75	50	129	29	53	58
5.	75	70	215	48	89	97
6.	100	50	172	38	71	78
7.	100	70	286	64	118	129
8.	125	50	215	48	89	97

Predictions assume a family of 4. Guarantees are percents of $825.
No unearned income is assumed. Family income is low enough so that family is always eligible for benefits. GLS estimates of α_M used for males, OLS estimate of α_F is used for females.
Applying the t test for the significance of predictions, we found that all figures for white males differ significantly from zero at the .001 level, for females and Black males at the .05 level, and the results for those with Spanish surnames do not differ significantly from zero.

earnings t_E, was equal to the tax on unearned income, t_Q. However, it simplifies calculations to consider an individual with no other unearned income, Q. (The predicted reduction in earnings would be smaller, the more Q the family had.) The numbers in Table VI-14, then, give the difference by race in male earnings and in female earnings, not by race, for members of a family of four. Earnings are first calculated given a particular NIT treatment, and then in the absence of any transfer payment. The difference between these -- the reduction in earnings due to the NIT treatment -- is recorded in Table VI-14. (Note that the dollar amount of the reduction is the same no matter what the initial earnings of the individual.)

There are substantial differences by treatment group and by race. The earnings of white males show the largest reduction; Spanish-surnamed males the smallest. All the results for white males differ significantly from zero at the .001 level, for females and Black males at the 5% level; for Spanish males the results do not differ significantly from zero. From our formula, both the guarantee level and tax rate must affect earnings if there is any effect of unearned income. The table gives an idea of the quantitative effects. The tax rate, for example, is the same on lines 2, 4, 6, and 8, but the guarantees differ. Comparing these lines for every group, we see a larger reduction in work effort the larger the guarantee (although the magnitude of the difference by guarantee varies by race). Similarly, the guarantee is the same on lines 3, 4, and 5 and also lines 6 and 7 (at a different level), but the tax rate differs. There is a noticeable impact from changing the tax rate.

Although the NIT experiment was originally designed to compare the effects of an NIT with no transfer program, we have seen that the actual comparisons in most studies of the NIT were between an NIT and AFDC-UF. Indeed, as the Wisconsin staff has noted, the interesting comparison is between an existing program and some revision of it.

The welfare contaimination of each of the NIT experimental groups made it impossible to observe even this effect experimentally. However, we can attempt to deduce the effects of a change in the existing system using our estimated equations for mean earnings. The Michigan sample consists of families faced with the existing welfare system. The chief difficulty is that there is not simply one existing program, but many different ones in different states. We will present, therefore, a few possibilities to illustrate how a change in an existing program can affect earnings. Since our results are best for males, we will consider adjustments by male heads using the Michigan data.

In the fifty states, the average guarantee available to a male-headed family of four, including AFDC-UF, General Assistance, and Food Stamps, is $2,431. The effective tax rate for a male head, beginning to work up to 20 hours at a wage of $1.60, averages 41%; while if he were to work up to 40 hours at a wage of $2.00, the tax rate would be 88%.[7] To approximate the actual system, we will assume a guarantee of $2,400 and a tax rate of 40%. We will then consider an increase in the guarantee of $1,000, and, alternatively, an increase in the tax rate by 10 percentage points. For the sake of comparison, we repeat some of these exercises for a guarantee of $3,600 and a tax rate of 40% and 70%. We assume throughout that the family has no other unearned income. Again, the dollar change in earnings does not depend on the initial level of earnings. Results are presented in Table VI-15.

The predicted reductions for whites all differ significantly from zero at the 5% level and for Blacks and other minorities at the 1% level. The interesting thing to observe is how the effect of a given change in a parameter of the transfer system depends on the existing system. Consider first the increase in guarantee. A higher guarantee would have the same effect on earnings no matter what the initial guarantee, so we do not illustrate this effect. The effect of the guarantee can vary substantilly, however, depending on the prevailing tax rate. At a tax rate of 25%,

TABLE VI-15

Predictions of Reductions in Male Annual Earnings Resulting from Changes in Existing Programs, Michigan Sample

Proposed Program Change	Existing Program Characteristics Guarantee	Tax Rate	White	Blacks And Other Minorities
1. Increase guarantee $1,000	$2,400	40%	$263	$162
2. same	2,400	70	525	323
3. same	2,400	25	210	129
4. Increase Tax Rate by .10	2,400	40	126	78
5. same	2,400	70	630	388
6. same	3,600	40	189	116
7. same	3,600	70	946	582

Predictions based on GLS estimates in Table VI-8.

Applying the t test for predictions, all results for whites differ signficantly from zero at the 5% level and for Blacks and other minorities at the 1% level.

a $1000 increase in the guarantee reduces earnings of whites by $210, or about 70 hours during the year if the hourly wage is $3. If the tax rate is 40%, the reduction is slightly larger, $263. But when the tax rate is 70%, the effect is more than doubled, $525. In a similar way, the effect of raising the tax rate depends on the initial tax rate and also on the initial guarantee. For example, raising the tax rate by 10 percentage points from 40% to 50% has a modest effect on earnings, an effect slightly larger when the guarantee is $3600 (line 6) rather than $2400 (line 4). However, in both cases the effect is about five times greater when the tax rate is raised from 70% to 80%. It thus appears that, at least in our Cobb-Douglas model, a given increase in the tax rate has a somewhat larger discouragement effect on work effort the higher the prevailing guarantee. In addition, the discouragement effect of both a higher guarantee and a higher tax rate depend on the prevailing level of the tax rate. For low and moderate tax rates, the extra discouragement associated with higher tax rates of guarantees is small. However, as the tax rate becomes large, an increase in the guarantee and especially a further increase in the tax rate begin to have a very substantial effect in lowering work effort. Although we are not prepared to generalize these results, it is likely that the relationship between earnings, the guarantee, and the tax rate will be non-linear; thus, differential responses depending on existing parameter levels should be carefully investigated.

C. Conclusion

For our statistical investigation of the pattern of earnings, we chose to concentrate on two measures of pattern: the mean and the standard deviation. We attempted to identify factors explaining mean earnings and those accounting for the variability in earnings, to measure these explanatory factors, and to test their significance statistically. In chapter V we developed a procedure to estimate jointly the mean and variability equations, thereby improving both. In explaining mean earnings, we relied primarily on the standard economic model of work effect, in particular on the

Cobb-Douglas variant of that model.

In explaining the variability in earnings, we had no similar guidance from standard economic theory. We speculated that variability depends on market forces beyond the control of the individual, on some of this own limitations, skills, and abilities, also largely beyond his control, but also possibly on his own preferences. We tried to identify variables relating to these factors, and then by an essentially experimental process, chose those variables that seemed to contribute most to the explorations of the variability in earnings. We found first that a number of industry and occupational dummy variables were significant, as well as some measures of individual characteristics. It is hard to generalize about these, since what mattered varied by group and locality. However, to the extent that these variables measured external market forces, we could conclude that much of the variability in earnings is explained by factors beyond the control of the individual. But then we found variables which suggested a possible voluntary basis to some of the variability. The effect of the unearned income term for Michigan males seemed to reflect a desire for breaks in employment, made possible by an income cushion in the family on which to rely. Although the desire for leisure seems to rise with income, there generally appears to be a continuing long-run commitment to the labor force. In contrast, the unearned income term for females, or, alternatively, the presence of the husband variable, produced effects suggesting that an income cushion would lead to a complete withdrawal from the labor ~~force~~. Since transfer payments may be expected to have an effect similar to that of unearned income, it seems likely that they also could contribute to variability in earnings. Nevertheless, our work in this area must be considered exploratory rather than definitive. In Chapter VIII, these results will be considered once more to investiage their policy implications.

We were able to investigate more explicitly the role of the transfer payment system in affecting mean earnings. We found for almost all groups a small but significant reduction in work effort resulting from any increase in the benefits offered by the transfer system. Perhaps the most interesting conclusion was that the effect of any change in the transfer system depends on the program already in place. For example, when the tax rate is at low or moderate levels, a given increase in it will cause only a modest reduction in work effort. But if the tax is high already, further increases begin to have much more marked effects on earnings. The effect of changing the guarantee depends in a similar way on the prevailing tax rate. We are not sure how far we can generalize this conclusion of the Cobb-Douglas model, but it seems safe to conclude that previous studies have not adequately investigated the interaction between the guarantee and tax rate.

CHAPTER V

Footnotes

1. Robert E. Hall, "Turnover in the Labor Force," Brookings Papers on Economic Activity, 3:1972

2. Peter F. Doeringer and Michael J. Piore, Internal Labor Markets and Manpower Analysis (Lexington, Mass., D.C. Heath, 1971).

3. Martin S. Feldstein, "Lowering the Permanent Rate of Unemployment," Harvard Institute of Economic Research, Discussion Paper Number 259, 1972, pp. 77-106.

4. We calculated a guarantee level, including an adjustment for family size, for each family each period. Members of the control groups were faced with AFDC-UF guarantees, as were members of the experimental groups, only in the periods when they received it. Otherwise, members of the experimental groups were faced with their experimental guarantees.

5. Another interpretation is that program participants first reported net earnings, but interviewers sought information on gross earnings. With the careful reporting required NIT recipients, they learned to give their gross earnings, wheras control group participants did not. The switch from net to gross among NIT participants could show up as higher variance for them. Again, it is not clear why only the Spanish should be so affected. For discussion of this problem, see Harold W. Watts, et al, "Concepts Used in the Central Analysis and Their Measurement" in Watts and Rees, Chapter B I.

6. Rueben Gronau, The Intrafamily Allocation of Time: The Value of the Housewives' Times," American Economic Review, September, 1973.

7. Storey, pp. 5, 53 and 55.

CHAPTER VII

An Analysis of Transfer Payments*

A. Methods of Study

Any transfer payment system is governed by a complicated package of regulations. Different families are likely to receive varying treatments under it depending on their circumstances. First, there are rules governing eligibility -- whether benefits to a family are to be positive or zero. Then if the family is eligible, its benefits are calculated. Under both the NIT and AFDC, benefits are calculated according to a formula of the form.

$$(1) \quad W = G - tY$$

where W is the amount of the transfer payment, G the guarantee level, t the tax or the benefit reduction rate, and Y the income of the family. However, there are complications in applying the formula, since there may be differences in the guarantee or the effective tax rate between families. For example, the guarantee typically varies by family size. The tax rate may differ with the type of income (earned or unearned). In addition, the effective tax rate under AFDC is affected by program features such as the set-aside and the work expense allowance. It is not possible to produce a single formula that will explicitly incorporate all the rules of a transfer system. Nevertheless, it is reasonable to relate transfer payments to a few key variables like income and family size, where the rules of the system, though not entering the formulation directly, do determine the observed relationship between the payments and the explanatory variables. This procedure is particularly useful in comparing

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*In this chapter we rely heavily on the work of Mr. Barry Sun, a research assistant in the Heller Graduate School, associated with this research grant.

transfer systems having different rules, for one can then examine whether the observed relationships differ between systems. In the remainder of Part A of this chapter, we will try to identify measurable variables that can contribute to the explanation of transfer payments. In Part B we will apply our model to the Wisconsin study sample where we can compare several NIT plans and AFDC-UF. In Part C we will investigate welfare experience in the Michigan sample where families are covered by AFDC, AFDC-UF, and General Assistance programs which differ by state.

A principal concern in studying transfer programs is to learn about the "welfare dependency" of recipients. One measure of welfare dependency for a family would be the proportion of time it received transfer payments over a prolonged period. Chapter III provides some information on this measure in Tables III-6, 7, and 8. Yet there is more to the story of welfare dependency. In an income conditioned program, a family may receive benefits even though it has income, provided its income is low. The amount of its payment ranges from its guarantee level when it has no income, down to zero as its income rises to the breakeven level of the program. Thus, the welfare dependency of a family depends not only on whether or not it receives benefits, but also on the magnitude of the benefits received. A measure reflecting both elements of welfare dependency is the mean over time of transfer payments received by the family. The mean will be zero only if the transfer payment in each period is zero, in which case the family has never received transfer payments over the period studied. At the other extreme, the highest degree of welfare dependency occurs when the mean equals the guarantee level of the family, for then it must have equaled the guarantee level all the time. Intermediate values of the mean benefit arise either because the family is ineligible some of the time (receiving a zero benefit) or because it has income even while receiving benefits (so that these are below the guarantee level) or both. It should be noted that the mean benefit depends not only on the income of the family, but also on the structure of the transfer program. It thus measures welfare dependency in the context of a given program and cannot be interpreted as a measure of the relationship between family income and "family needs." Notice finally that the mean benefit measures the average cost to the government of supporting the family under the existing welfare system over a prolonged period of time. We will use the mean benefit level of a family as our dependent variable.

In explaining mean benefits, we must deal with the question of eligibility. The rules differ substantially from one program to another and can get complicated. For our purposes, it is useful to distinguish three types of families: 1) those who are never eligible and are thus never recipients, 2) those who are eligible in every period of the study so that they always receive benefits, 3) those whose eligibility varies over time so that they receive benefits, but only some of the time. For types 2 and 3, mean benefits will vary with income. Although type 1 families experience different levels of income, their mean benefits are always the same — zero. Thus type 1 families should not be included in the same equation that explains the mean benefits of families of types 2 and 3. (Differences between type 1 families and the others are discussed in Chapter III.) We return to a discussion of what determines eligibility after considering what determines benefits for type 2 and 3 families.

In any period in which a family is eligible, its transfer payment is related to its income by equation (1). For a family of type 2 (which is always eligible), the mean benefit, $\overline{W}$, will be related to mean income, $\overline{Y}$, by the same equation, provided G and t remain constant over time. If we could statistically estimate equation (1) just for type 2 families, the estimated coefficient of $\overline{Y}$ should be the average, effective tax rate. However, our samples of type 2 families alone are too small, since type 3 families are far more common. For a type 3 family, $\overline{W}$ will also be related to $\overline{Y}$, but not by equation (1). In periods when the family receives no benefits and when its income is high, equation (1) would predict a negative W while the actual benefit would be zero. The value of $\overline{W}$ predicted by equation (1) would thus be smaller than the true value. If we statistically estimate the relationship between actual $\overline{W}$ and $\overline{Y}$, the estimated coefficient of $\overline{Y}$ should be negative, but smaller (in absolute value) than the tax rate. The coefficient of the mean income term will thus measure the combined effect of the average, effective tax rate and the proportion of time families in our sample are ineligible.

Observe that equation (1) is a linear expression involving income in one term and the guarantee in the other. As we observed earlier, the main reason for differences in the guarantee between families is differences in family size. For simplicity we assume that the guarantee is a linear function of family size, F.

$$(2) \quad G = A + BF$$

The basic guarantee for a family of four is thus A + 4B, and B measures the increase in guarantee resulting from each additional person in the household. For a family of type 2, mean benefits would be

$$(3) \quad \overline{W} = A + B\overline{F} - t\overline{Y}$$

where $\overline{F}$ is the mean family size. However, if we estimate an equation in the form

$$(4) \quad \overline{W} = a_o + a_1\overline{F} - a_2\overline{Y}$$

for both family types 2 and 3, we would expect each of the estimated coefficients, a_o, a_1, and a_2, to be smaller than the corresponding program parameters A, B, and t. The reason again is that type 3 families are ineligible for benefits part of the time. During this ineligible time, equation (3) would predict a negative W when actual benefits are zero. The estimating procedure will thus tend to scale down the program parameters in arriving at an equation that will predict actual $\overline{W}$.

Indeed there is a serious difficulty in estimating equation (4) for family types 2 and 3 combined. Consider two families, one of type 2 and the other of type 3, such that $\overline{F}$ and $\overline{Y}$ are the same for both. Since the type 3 family was ineligible some of the time, it may have had a high income in periods of eligibility counterbalanced by a low income in the other periods to make its mean income equal to that of the less volatile income stream of the type 2 family. But then $\overline{W}$ will probably differ for the two families even though equation (4) would predict an identical value for both. The problem is that we must introduce variables to control for the changes in eligiblity among type 3 families. Changes in eligibility result primarily from changes in income or in family structure.

As the illustration in the last paragraph showed, there may be an independent effect of the variability in income on the mean benefit to the extent that the variability is associated with changes in eligibility. A convenient measure of variability is the standard deviation of income. However, the relationship between $\overline{W}$ and income variability is likely not to be a simple one. Consider a family with low mean income. If it is a type 2 family (always receiving benefits), then variability in its income has no independent effect on $\overline{W}$. The variability begins to matter only when income in some periods becomes large enough to make the family ineligible. To the extent that the family is ineligible part of the time, its $\overline{W}$ will be smaller than that of a type 2 family with the same mean income. Thus, for a family with a given low $\overline{Y}$, the larger the variability in income, the greater the likelihood that the family will be ineligible some of the time. The greater the period of its ineligibility, the smaller will be its $\overline{W}$. We may thus expect a negative relationship between income variability and $\overline{W}$ for a family with low mean income. Consider, however, a family with mean income above its breakeven level. On the basis of the mean alone this family would receive no benefits. It becomes eligible to the extent that its income becomes low enough in some periods. The higher the variability of its income, the greater the likelihood that it will be eligible part of the time, so the greater its $\overline{W}$. To summarize, we expect no relation between $\overline{W}$ and income variability for families of type 2. For families of type 3 we expect a negative relationship when mean income is low and a positive association when it is high.

To accommodate all these cases in one model, we first define a variable S_y, equal to the standard deviation of income for families of type 3 and equal to zero for families of type 2. We then add this variable to equation (4), but instead of assuming that its coefficient is constant, we make it an increasing function of mean income of the form $a_3 \sqrt{\overline{Y}} - a_4$. (Any increasing function of $\overline{Y}$ could be tried, but we obtained the best results using the square root.) This coefficient is negative when $\overline{Y}$ is low enough, but becomes positive as $\overline{Y}$ rises. Our model thus becomes

$$(5) \quad \overline{W} = a_o + a_1\overline{F} - a_2\overline{Y} + (a_3\sqrt{\overline{Y}} - a_4)S_y =$$

$$a_o + a_1\overline{F} - a_2\overline{Y} + a_3S_y\sqrt{\overline{Y}} - a_4S_y$$

We thus add not only the variable S_y, but also the product term $S_y\sqrt{\overline{Y}}$ as a separate variable.

Consider now changes in eligibility resulting from changes in family structure. Suppose a female-headed family received AFDC. If the woman marries, the family is no longer eligible for AFDC. It may qualify for AFDC-UF if such a program is available in its state, but eligibility standards are more stringent than for AFDC. As another example, if a husband leaves his family, the family may become eligible for AFDC. Thus, although a male-headed family may receive welfare, it is usually easier for a female-headed family to qualify. The number of periods the male head is present is a variable that might measure this effect. The fewer periods the male head is present, the more periods the family is likely to qualify for welfare and so the higher its mean benefits may be.

As one final consideration, note the likelihood of bias in estimating equation (5), due to simultaneity. For not only does income determine transfer payments, but transfer payments may influence income. We could have replaced actual income by an instrumental variable constructed to be uncorrelated with the error in equation (5).

This construction could have been obtained by using the predicted values from our male and female earnings equations, then adding these to other non-welfare income (assumed exogenous) for each family. In view of the poor quality of our female earnings equations, we decided not to attempt such a procedure. Note in making comparisons that while all magnitudes in our earnings equations are expressed in real terms, the variables in equation (5) are all in nominal terms. The theory of labor supply underlying the earnings equations relates only real variables. In contrast, transfer payments are calculated in terms of nominal income.

B. NIT and AFDC-UF in the Wisconsin Study Sample

1. The Choice Between NIT and AFDC

The introduction of an AFDC-UF program in New Jersey shortly after the beginning of the NIT experiment gave NIT recipients the option of switching to an alternative program. Thus, average NIT benefits over time for a family could be low either because the family received small transfer payments altogether or because the family switched to AFDC-UF. Two families could have the same values for all of the independent variables in equation (5) but different values of mean NIT benefits if one of the families switched while the other did not. In estimating equation (5) for the various experimental and control treatments, it is necessary to add a variable to control for program switching. The only switching possible was from a family's assigned NIT treatment group to AFDC-UF (or back again). What matters is the amount of time spent under the alternative program. Let P be the ratio of the number of quarters in which NIT was received to the number of quarters in which any transfer payment -- NIT or AFDC-UF -- was received. We will use P as our variable to control for switching. It measures the percent of total welfare time spent on NIT.

As was shown earlier in Table 8 of Chapter III, P differs substantially from one experimental treatment group to another. Apparently, the inducement to switch was stronger in some of the groups than in others. Before proceeding with the estimation of equation (5), it is thus interesting to investigate what determines P. Our sample for this investigation is limited to members of the eight experimental NIT treatment groups who had the option of receiving NIT benefits. To insure a finite P, we limit the sample further to those families who receive some transfer payment, whether AFDC-UF, NIT, or both, during the experiment. We then try a number of explanatory variables and estimate the relationship by least squares. The result is reported in Table VII-1.

The first three variables in the table are variables that we expect to explain mean payments under any given program as in equation (5). From line 1 we observe that the higher mean income, the larger the proportion of "welfare" time spent on NIT rather than on AFDC-UF. This is consistent with the features of AFDC-UF which require first that the male head not be working in order to qualify for benefits (although he must have worked recently), and second that family income be less than the guarantee level. In addition, male earnings are taxed at a high rate if the man begins to work a substantial number of hours. As a result of these features, income must be low in any period in which AFDC-UF benefits are received. In contrast eligibility in the NIT plans depends only on income, not on male hours worked. Moreover, the income eligibility level under NIT is at the breakeven level, not the lower guarantee level. Since income of NIT families can be higher than that of AFDC-UF families while receiving benefits, it is not surprising that families with higher mean income tend to spend relatively more of their welfare time on NIT.

The measure of income variability, S_y, has a negative effect on P, so that families with greater variability spend more welfare time on AFDC-UF. As we have just observed, income must be very low for a family to qualify for AFDC-UF. But the male head must have been working before his earnings fell to zero. Thus, AFDC-UF recipients must have fluctuating incomes to qualify. In contrast, earnings of NIT family heads need not fall to zero for the family to qualify. A family can stay on NIT with little

TABLE VII-1

Regression Estimates for P, Proportion of Welfare Time Spent on NIT

1.	Mean Income	.0002 (12.58)***
2.	S_y	-.0003 (-7.51)***
3.	Family Size	-.0443 (-6.25)***
4.	NIT Guarantee Minus AFDC-UF Guarantee	.1671 (4.98)***
5.	NIT Tax Rate Minus AFDC-UF Tax Rate	-.2420 (-2.56)*
6.	Trenton	.1509 (3.93)***
7.	Constant	.830
	R^2.	.32
	Number of Observations	549
	F ratio	41.99

TABLE VII-1

Notes

Sample includes only members of experimental treatment groups who received some form of transfer payment during experiment.

P: ratio of number of quarters on NIT to number of quarters in which any transfer payment (NIT or AFDC-UF) was received.

Mean Income: mean over time of non-transfer income in current prices.

S_y: same as in equation (5). Equals zero for a family receiving NIT all twelve periods. Otherwise equals standard deviation of income.

Family Size: mean family size over duration of experiment.

NIT Guarantee Minus AFDC-UF Guarantee: NIT guarantee, as fraction of poverty line for the family, determined by experimental treatment group of family; AFDC-UF guarantee assumed to be 1.26 poverty line, since quarterly guarantee in New Jersey for a family of four was $1041 beginning in 1969 while the initial poverty line for a family of four used in the experiment was $825 per quarter.

NIT Tax Rate Minus AFDC-UF Tax Rate: NIT tax rate determined by experimental group of family. AFDC-UF tax rate assumed to be .67.

Trenton: Dummy variable equals one if family lived in Trenton, zero otherwise.

Variables included in step-wise regression, but omitted from table due to insignificance: Spanish-Surname, Black, Jersey City, Scranton, meam hours worked by male and by female.

fluctuation in income. Indeed, this result is consistent with our later findings that AFDC-UF payments are larger the larger the variability in income, while NIT payments decline with variability, at least up to a fairly high level of mean income.

The negative coefficient of the family size term may reflect the fact that even as a family size becomes very large, benefits continue to rise under AFDC-UF. In contrast, NIT benefits increase only up to a family size of eight.

The differential guarantee and tax rate have significant effects on the choice of program in the expected directions. The higher the NIT guarantee relative to that of AFDC-UF, the more attractive the NIT. On the other hand, a relatively higher NIT tax rate induces families to switch to AFDC-UF.

The positive coefficient for Trenton probably results from the fact that the experiment began there earlier than in the other cities. Since AFDC-UF was not introduced until after the start of the experiment, families in Trenton had less time in which they could receive AFDC-UF. If they were to receive anything in the early quarters of the experiment, it had to be NIT.

2. Empirical Results on NIT and AFDC-UF Payments

In order to detect differences in payment systems, it is necessary to estimate equation (5) separately for each different welfare treatment. We must first separate mean NIT payments from mean AFDC-UF payments (remembering that some individuals receive both, serially). There are eight different NIT treatments, so a separate regression is run for mean NIT payments in each of the NIT treatment groups. Results appear in Table VII-2. Members of the control group were eligible only for AFDC-UF. In addition, any eligible family in the experimental group could switch from NIT to AFDC-UF once the program became available. Since members of the control group were faced by only one program while members of the experimental group had a choice, we estimated separately mean AFDC-UF payments for the experimental group as a whole and for the control group. Results appear in Table VII-4.

TABLE VII-2

Regression Results on Mean NIT Payments; By NIT Experimental Group

		(1) 50-30	(2) 50-50	(3) 75-30	(4) 75-50	(5) 75-70	(6) 100-50	(7) 100-70	(8) 125-50
1.	$\overline{Y}$	-.1082 (-5.00)***	-.0251 (-3.18)**	-.1618 (-11.29)***	-.3178 (-11.86)***	-.0860 (3.96)***	-.3262 (-9.83)***	-.2964 (-5.52)***	-.4092 (-19.28)***
2.	$S_y\sqrt{\overline{Y}}$				.0224 (7.42)***		.0142 (2.80)**	.0187 (2.96)**	.0032 (.83)
3.	S_y	-.0255 (-.84)		-.0751 (-3.05)**	-.9138 (-7.12)***	.0698 (1.22)	-.6347 (-3.08)**	-.8097 (-3.34)**	-.1548 (-.89)
4.	F	23.5708 (3.50)**	3.3339 (1.13)	59.0629 (12.18)***	33.9749 (5.34)***	5.8050 (.74)	50.6452 (5.38)***	36.6342 (3.92)***	99.6072 (15.12)***
5.	P	231.8211 (5.46)***	52.6988 (2.83)**	524.6372 (13.75)***	153.8533 (3.42)***	160.2210 (3.34)**	619.6882 (7.23)***	296.0828 (3.51)***	1101.7542 (16.91)***
6.	Constant	-79.268	.061	-270.083	243.36	2.366	-61.557	133.127	-462.28
	R^2	.54	.27	.81	.70	.38	.80	.60	.86
	Number of Observations	40	34	84	84	37	59	61	122
	F Ratio	10.33	3.68	83.19	35.47	4.99	43.23	16.28	146.86

TABLE VII-2

Notes

Regression samples include only families with positive mean NIT payments.

$\bar{Y}$: mean over time of non-transfer family income, in nominal prices.

S_y: equals zero for families receiving NIT payments all 12 periods; equals the standard deviation of non-transfer family income for others.

$S_y \sqrt{\bar{Y}}$: product of Sy and $\sqrt{\bar{Y}}$.

$\bar{F}$: mean over time of family size up to 8; a larger family is counted as having 8 members. (NIT payments adjusted on basis of family size only up to a family of 8.)

P: ratio of the number of periods in which NIT payments received to the number of periods in which any transfer payment - NIT or AFDC-UF - received.

In examining the NIT payments equations, notice first that P is strongly significant with positive coefficient in every equation. As is reasonable, the larger the fraction of welfare time spent on NIT, the larger is the mean of NIT payments over the twelve quarters of the experiment. The variable P is included to control for the fact that families in every experimental group chose to spend some of their welfare time on AFDC-UF rather than on NIT. As the significance of P shows, this opportunity to choose between programs had an important effect on the NIT payments actually received. Indeed, it is possible that P does not fully control for the phenomenon so that other coefficients are also influenced by the opportunity to switch to AFDC-UF.

Mean income (line 1) is strongly significant with a negative coefficient in all eight NIT groups, but the magnitude of the coefficient varies from group to group. Recall that the coefficient depends first on the tax rate and second on the amount of time over which positive NIT payments were received. To see the effects of these two factors on the coefficient, we can compare groups that differ only in the tax rate, e.g., 1 and 2; 3, 4, and 5; 6 and 7; and also groups that have the same tax rate, differing only in guarantee, e.g., 1 and 3; 2, 4, 6, and 8; 5 and 7. Although the tax rate is 50% in group 2 as opposed to 30% in group 1, the mean income coefficient for group 2 is much smaller. The reason is obvious from Table VII-3 where we see in line 1 that members of group 2 receive NIT payments for an average of only 2.88 quarters as opposed to the 7.25 for group 1. Although the guarantee is the same in both groups, the higher tax rate greatly reduces the breakeven level (line 2 of Table VII-3, for a family of four) making families ineligible at a lower level of income. The only difference between groups 3, 4, and 5 is again the tax rate. The mean number of periods for which NIT was received again declines dramatically as the tax rate

TABLE VII-3

Miscellaneous Characteristics of NIT Experimental Groups

	(1) 50-30	(2) 50-50	(3) 75-30	(4) 75-50	(5) 75-70	(6) 100-50	(7) 100-70	(8) 125-50
1. Mean Number of Periods of NIT Receipt	7.25	2.88	9.48	6.20	3.97	9.44	6.82	10.67
2. Quarterly Breakeven Level, Family of Four	$1375	825	2063	1238	884	1650	1179	2063
3. Dividing Level of $\overline{Y}$				$1665		1998	1875	2333
4. Predicted Mean NIT Payments	$96	33	250	78	102	288	136	490

Notes

line 1: means calculated only over families who at some time received NIT benefits, i.e., for whom mean NIT payments is positive.

line 2: calculated as of 1968. The guarantee was adjusted upward during the experiment as the price level rose. The breakeven level thus increased, too, in the same proportion as the guarantee.

line 3: the level of $\overline{Y}$ such that S_y has a negative effect on mean payments below it and a positive effect above it. Calculated by formula $\left(\frac{a_4}{a_3}\right)^2$ where a_4 is the coefficient of S_y and a_3 is the coefficient of $S_y\sqrt{\overline{Y}}$ in equation (5).

line 4: Predicted mean NIT payments for family of four with $\overline{Y}$ = 1300, S_y = 400, P = 1; based on coefficients in Table VII-2.

increases. Nevertheless, the mean income coefficient is actually higher in group 4 relative to 3, so that the direct effect of the tax rate on the coefficient in this case apparently predominates over its indirect effect on participation in NIT. The coefficient for group 5 sharply falls again, showing the strong effect of families not participating in NIT, either because of ineligibility or switching to AFDC-UF. Similarly, the coefficient is lower for group 7 than for group 6. In considering changes in the tax rate, then, with one exception, the indirect effect of the tax rate on program participation predominates over its direct effect on the mean income coefficient. When comparing groups with the same tax rate, it is apparent that the higher the guarantee level, the greater the magnitude of the mean income coefficient. This also reflects the importance of program participation since the higher the guarantee (with given tax rate), the higher the breakeven level and also, the more attractive the NIT treatment relative to AFDC-UF. It appears then that the coefficient of mean income reflects not only the direct effect of the tax rate on payments received, but also the significant, but indirect effect of both the tax rate and guarantee level on program participation.

From equation (5) we know that the effect of the variability in income is reflected by the coefficients of both S_y and $S_y\sqrt{\bar{Y}}$. The product term is included to detect the possibility that the effect of variability on mean earnings may switch from being negative to positive at some mean income level. The S_y term is significant with the expected negative sign for groups 3, 4, 6, and 7, while the product term is significantly positive for groups 4, 6, and 7. Thus for groups 4, 6, and 7 (and also for 8, even though the coefficients are insignificant), we can caluculate the levels of mean income which divide between negative and positive effects of S_y (line 3, Table VII-3). In all cases, these levels are somewhat above the breakeven level for a family of four. Since larger families have larger breakeven levels, the dividing income levels could be close to or even below breakeven levels for them. These numbers are not unreasonable in view of our argument that high variability should reduce mean payments for families heavily dependent on welfare while it should increase mean payments for families with higher income. Both S_y and the product term have the correct sign for group 8, but the coefficients are insignificant. That is not surprising, since the S_y term measures the effect of variability on mean payments to the extent that the variability results in a change in eligibility for benefits. In group 8 -- the most favorable treatment -- there are few shifts in eligibility: families who receive benefits tend to receive them most of the time. In groups 2 and 5, families receive NIT for such limited periods that the equations are of questionable validity altogether. Overall then, to the extent that an effect of the S_y term emerges, it seems to be negative, at least for low mean incomes. For groups 4, 6, and 7 we can detect a shift to a positive effect in the neighborhood of the breakeven level.

The family size term is positive for all groups and significant for all, except groups 2 and 5. This seems to confirm the unreliability of the equations for these two groups. For the other groups, variations in the coefficient from group to group accord with expectations. First, in the formula for calculating benefits, the family size correction is larger the more generous the guarantee of the treatment group. Due to this direct effect on payments, we should expect a larger coefficient the higher the basic guarantee. Second, the coefficient is increased by greater program participation, which is encouraged by higher guarantees and discouraged by higher tax rates. By comparing coefficients on line 4 of Table VII-2, we observe that coefficients are larger when the guarantee is larger, given a tax rate. They are smaller the higher the tax rate, given a guarantee level.

We performed tests to determine whether each equation taken as a whole differs significantly from others. We tested all eight equations simultaneously as well as the equations for all groups with either the same tax rate or the same guarantee. In all cases, the appropriate F ratio shows differences significant at less than the .001 level. In view of the unreliability of groups 2 and 5, the tests were repeated omitting these two groups. Again, the differences are strongly significant.

To see the implications of the differences, we calculated the predicted value of mean NIT payments based on each of the eight equations for a family of four having a mean income of $1300 per quarter, a value of S_y of $400 a quarter, and a value of P equal to one so that its entire welfare time is spent on NIT. The results are presented in line 4 of Table VII-3. The $1300 mean income is above the quarterly breakeven levels for a family of four for groups 2, 4, 5, and 7. Payments arise in these groups only because the variability in income occasionally makes the family eligible. Payments are much higher when the mean income is well below the breakeven level, as in groups 3, 6, and 8. Groups 3 and 8 have the same breakeven level but very different predicted payments. Group 3 has a high breakeven level because of its low tax rate, while group 8 has a high guarantee. Although eligibility standards are the same for the two groups, the higher guarantee gives higher payments in group 8.

Examining now the results for mean payments under AFDC-UF presented in Table VII-4, the mean income term is again negative and significant while the family size term is positive and significant in both equations. In contrast to the NIT results, the variability term, S_y, is positive in both equations, although significant only among the experimental group. In the NIT program, a change in income across the breakeven level changes eligibility (except for complications introduced by the income accounting system). In contrast, income must fall below the breakeven level to the guarantee level and male earnings must fall to zero for a family to qualify for AFDC-UF. It is conceivable that those with greater variability in income are likely to qualify more often and thus have a higher mean AFDC-UF payment. In addition we already observed in our discussion of the choice between NIT and AFDC-UF that those with higher variability in earnings tend to spend more time on AFDC-UF. The sample in the experimental group equation may thus include a large number of families with both high variability and high mean AFDC-UF payments. The significance of S_y only for the experimental group may thus reflect the peculiarities of a self-selected sample which chooses the program presumably because of its relative attractiveness

TABLE VII-4

Regression Results on Mean AFDC-UF Payments, Wisconsin Sample

	Experimental Group	Control Group
1. $\overline{Y}$	-.3088 (-7.59)***	-.2892 (-5.99)***
2. S_y	.2267 (3.22)**	.0725 (.87)
3. $\overline{F}$	83.5259 (8.59)***	73.0752 (6.74)***
4. Trenton	-161.9201 (-2.69)**	-124.7691 (-1.85)
5. Jersey City	107.1278 (1.89)	
6. Constant	421.133	[illegible]3.626
R^2	.39	.39
Number of Observations	177	112
F ratio	21.72	17.09

Notes

Regression samples include only families with positive mean AFDC-UF payments.

$\overline{Y}$: mean over time of non-transfer family income, in nominal prices.

S_y: equals zero for any family receiving AFDC-UF for 12 periods; equals the standard deviation of non-transfer family income for others.

$\overline{F}$: mean over time of actual family size.

Trenton, Jersey City: dummy variables.

to them.

In order to control for the program switching in the experimental group, we tried the same variable, P (the ratio of periods of NIT receipt to periods on either form of welfare), used for the same purpose in the NIT payment equations. It was insignificant. In view of the possible peculiarities in the experimental group sample, the equation for the control group probably gives a more reliable picture of the determinants of payments under AFDC-UF. However, with only one equation, we cannot observe how particular elements of program structure affect the form of the equation as we could with the NIT groups.

We include city dummy variables in the AFDC-UF equations to detect two possible effects. First, AFDC-UF could be administered differently in different cities. Such differences are less likely under NIT. Second, since the experiment ran over different periods of calendar time in each city, and since AFDC-UF was introduced after the start of the experiment, families in different cities had the AFDC-UF option available for different lengths of time. We observe a negative coefficient for Trenton, significant for the experimental group.

C. Welfare Payments in the Michigan Sample

A convenient feature of the Wisconsin sample is that we can distinguish several subgroups within which all families are subject to the same transfer program. The Michigan sample, however, is a national sample. There are numerous differences in guarantee levels and tax rates between states and variations in administration not only between but within states. In order to get samples large enough to study, we will group states into the same four categories used in Chapter III. It should be remembered that there *may* be significant differences within these categories. Moreover, the distinguishing characteristics of each category are not known with the same precision as is the case with the Wisconsin subgroups.

Whereas the Wisconsin sample consists essentially of male-headed families, the Michigan sample contains a variety of family types. Male-headed families may qualify for General Assistance or AFDC-UF in those states offering it, while families with female heads may be eligible for AFDC. In view of the more stringent eligibility requirements for AFDC-UF and its more limited availability, we will study mean payments for families where the male head is always present separately from other families. The other families then include those which always have a female head as well as those having both male and female heads at different times. The families with changing family heads may qualify for AFDC-UF when the male is present. However, the likelier source of benefits for such families is AFDC. We assume then that our category combining families with female heads and changing heads will receive primarily AFDC and General Assistance with just a small amount of AFDC-UF, while the AFDC-UF payments will be concentrated in the category of male-headed families. Results from estimating equation (5) are presented in Table VII-5.

Since welfare status could change with a change in family head, the number of periods the male head was present was included as a variable, but it was insignificant. Casual observation of Table III-7 had suggested the influence of disability on mean payments, but it too was insignificant as a variable. Apparently, with the close association between income and disability, the income term picks up most of the effect of disabilities.

As with the mean payments equations for the Wisconsin sample, the family size term is always positive and usually significant while the mean income term is always negative and significant. In addition the S_y term and the product term appear with correct signs for the low, low and high, high groups.

In examining the mean income coefficient within each group of states, it is clear that the coefficient for always male-headed families is lower than that for the other families in the first three categories. This is a reflection primarily of the lower participation rate of male-headed families in welfare programs. The exception is the high, high group. We know that participation of male-headed families is higher in this group. In addition many of the states with AFDC-UF fall in this category.

TABLE VII-5

Regression Results on Mean Transfer Payments, Michigan Sample

Type of State (by levels of guarantee and tax rate)	LOW, LOW		LOW, HIGH		HIGH, LOW		HIGH, HIGH	
Type of Family Head	Always Male	Other	Always Male	Other	Always Male	Other	Always Male	Other
1. $\overline{Y}$	-.1437 (-2.52)*	-.2010 (-4.96)***	-.1212 (-3.59)***	-.2923 (-8.84)***	-.3007 (-4.99)***	-.3233 (-9.28)***	-.6105 (-7.58)***	-.3270 (-7.68)***
2. $S_y\sqrt{\overline{Y}}$	.0101 (2.32)*						.0189 (3.62)***	.0056 (3.27)**
3. S_y	.7780 (-2.70)**	.0794 (-1.22)					-1.2297 (-2.64)*	-.5845 (-3.96)***
4. $\overline{F}$	61.1674 (2.86)**	125.5702 (6.28)***	83.8197 (2.68)*	253.6944 (8.59)***	95.4283 (1.58)	386.7529 (12.29)***	393.0293 (7.98)***	386.5400 (11.49)***
5. Constant	772.67	784.35	668.11	1003.60	1883.43	978.25	1763.45	1456.38
R^2	.28	.41	.25	.45	.51	.72	.77	.69
Number of Observations	52	112	47	149	27	83	40	119
F ratio	4.60	25.35	7.31	58.83	12.71	104.51	28.48	64.44

Variable definitions same as in Table VII-4 except that transfer payments here include AFDC-UF, General Assistance, and AFDC.

Moreover, the effective tax rate in AFDC-UF is higher than that in AFDC, ranging up to 65 percent.

D. Conclusion

In this chapter we have used the mean transfer payment over a period of a few years as our measure of welfare dependency. It is more informative than a measure of simply presence on welfare since it presents the average cost over a prolonged period of supporting a family with a specific set of characteristics under a given program. Our principal conclusion is perhaps best stated in a negative way. Essentially, knowing the degree of welfare dependency implies little else about the characteristics of the family (other than that it is poor over at least part of the period under consideration). In particular, one cannot deduce matters such as whether or not family members are lazy or whether or not they are unstable workers. There are two chief reasons for the uninformative nature of welfare dependency. First, our results show significant differences in what determines welfare dependency resulting from differences in program structure. Second, even under a given program, welfare dependency is likely to depend both on mean income and the variability of income so that many combinations of these two variables can produce the same level of welfare dependency.

The effect of program structure on welfare dependency can be seen most explicitly from the mean NIT equations for the eight experimental groups. We have there a set of eight transfer programs identical except for guarantee and tax rate. We explain welfare dependency in each program on the basis of the same independent variables. Table VII-2 shows that the coefficients of the variables vary in a systematic way as the tax rate and guarantee level change. Our discussion showed how the differences in coefficients result first from differences in payments, and second, from differences in rates of participation in the program. In other words, a major part of the effect on welfare dependency of changes in the guarantee level and the tax rate comes from changes in eligibility. Our tests showed that the coefficients differ significantly from one equation to another. This means that families in different treatment groups with the same values of the independent variables will exhibit different levels of welfare dependency.

Under a given transfer program, the appearance of both mean income and the variability of income, S_y, in the equation creates the difficulty in interpreting a particular level of welfare dependency. A given level of welfare dependency can occur with various combinations of mean income and variability. However, the trade-offs between mean income and variability are complicated. The variability term was introduced to detect the effect of changes in eligibility on mean payments resulting from changes in income. A given degree of welfare dependency can result either from steady low icome, or from income sometimes higher, but sometimes negligible. In other words, both the steady, low wage worker and the higher wage worker who experiences periodic breaks in employment may have the same degree of welfare dependency. Work patterns must be investigated directly. In general, they cannot be deduced from a measure of welfare dependency.

CHAPTER VIII

Implications of the Study for the WIN Program

A major objective of welfare programs is to move people from "welfare to work." Overwhelmingly, males in the low-income and near low-income populations typically move from welfare to work on their own. Over a longer stretch of time, most female heads of families in these income groups appear to do likewise. Therefore, care is needed in judging the success of the WIN Program. A welfare recipient returning to work is only a partial measure of program success. The critical element is how rapidly the change is made. Our results concerning work patterns and the determinants of work effort provide a framework for judging the extent to which work effort can be influenced by deliberate policy. In other words, our findings give an indication of the limits of success for a program like WIN.

Actually the study of program success is more complicated because it is necessary to distinguish a short-term success -- getting a welfare recipient to work -- from a long-term success -- getting a recipient to work in a situation where the probability is very low that he will leave work and return to the welfare rolls. Our results show that while there is much movement between work and welfare, there is little and slow movement out of the low income ranges for most families finding themselves there. A program therefore, which seeks to move people from welfare to work may be successful on a short-term basis but unsuccessful on a long-term basis. Unfortunately, our results provide little additional insight into the evaluation of long-term success, being useful primarily for the appraisal of short-term success.

In section A of this chapter we review our findings concerning patterns of work and the determinants of work effort in order to evaluate the opportunities for success of the WIN Program. Section B considers possible effects on work effort and welfare dependency of changes in welfare programs. Section C considers limitations of the study, indicating problems in the investigation of particular design elements of income maintenance programs like categorical programs or work tests.

A. Work Effort and the WIN Program

1. Work Patterns

The work patterns of males and females were investigated separately in both the Wisconsin and Michigan data sets.

Among male heads of families in the low income population, there is a variety of work patterns and substantial evidence of fluctuations in employment status and earnings over time. At any point in time during the NIT experiment, the Wisconsin data indicate that roughly 86 percent of the male heads were employed. During the three year experiment, however, roughly 96 percent of the males who basically remained with their families worked at one time or another. Similarly in the Michigan data, we found that over time almost all male heads worked at one point or another. Over a five year period, 96 percent of the male heads worked at some time. Thus, there is not a fixed group of employed working poor. Rather there is a flow of males through employment, with the group as a whole evidencing a high degree of labor force attachment.

Closer examination reveals that there are identifiable groups with significantly different work patterns. One interesting group in the Wisconsin sample, roughly one-fifth of the total, averages more than 41 hours of work per week during the entire experimental period. A majority of these men has substantial fluctuations in earnings, but the fluctuations do not result from unemployment. They result mainly from fluctuations in overtime hours or from moving in and out of moonlighting jobs. These very hard workers tend to be young, healthy, more educated, but nevertheless poor or near poor. In addition to these workers who work regularly more than full-time, there is another group, over 30 percent of the total, consisting of men who work steadily at just the full-time level. Workers in these two groups would not normally be covered by the WIN Program. Although any of them could lose their jobs, given a choice, these people voluntarily choose work. If they did lose their jobs and fell under the coverage of AFDC-UF and thus the WIN Program, they might need help only in finding new jobs. Other service treatments might be unnecessary. These two groups constitute nearly half of the Wisconsin sample of poor and near-poor male family heads. In the Michigan data, we also find a sizable group of stable workers: nearly two-thirds of the male heads averaged 1800 hours or more per year over the five years.

The remaining half of the Wisconsin sample consists of males who at some point during the study were out of work. In this half, there is a small proportion who never work and typically suffer from some disabling condition. Another group, constituting over 30 percent of the total has both unstable employment and earnings. When working, these people work full-time and earn wages similar on the average to those of other groups. However, they often are unemployed and change jobs frequently. The remainder of this half of the sample consists of those who work most of the time with one or two brief spells of unemployment during the period of study. Almost always those who lose these jobs do return to work again. This result holds for those covered by the WIN Program as well as for those who are not. (Since many of these were covered by the NIT experiment rather than by AFDC-UF, they did not in fact participate in WIN.) It is this half of the poor and non-poor who are the potential clients of the WIN Program. The very unstable workers might have special problems requiring special treatments. For most, however, the critical question is whether the observed unemployment is voluntary.

In considering a spell of unemployment voluntariness can appear at the beginning, the end, or both. For example, a persom may lose his job involuntarily, but then delay his return to work voluntarily. The WIN Program has little control over the loss of jobs. Its main concern is the return to work. Therefore, only voluntary influences on the speed of return to work need be considered. If there is no intentional delay, the WIN Program is useful primarily to the extent that it provides job placement. If, however, the worker seeks to prolong unemployment, then there is an opportunity for the WIN Program to succeed in inducing a more speedy return to work with treatments other than placement services. In order to judge the voluntariness of unemployment, it is necessary to examine the determinants of work effort and, in particular, how welfare programs affect work effort.

Although the study concentrated on males, substantial attention was devoted to the work effort of females. In the Wisconsin data, no more than 15 percent or so of the female spouses were employed at any point in time. Interestingly, in the Michigan data 77 percent of the female heads of families worked at some time during the five years. And over one-third of the latter group averaged 1800 hours or more of work per year over the five years.

2. Determinants of Work Effort and Earnings

The statistical analysis of mean earnings and of the variability in earnings from Chapters V and VI provide our evidence on the determinants of work effort. It is, of course, possible that a low wage rate will discourage a worker from exerting himself. However, we will concentrate on the determinants of variability in earnings and on the effects of unearned income (including transfer payments) on mean earnings. Although the principal interest of the WIN Program is in the work effort of those experiencing unemployment, it is necessary for statistical purposes to use a sample consisting of all workers whether or not they experienced some unemployment.

a. Unearned Income Effect

Our equations for mean earnings showed a significant negative coefficient for unearned income for most groups tested. That means that an increase in unearned income will tend to lead to a reduction in earnings and presumably also in work effort. Among males in both the Michigan and Wisconsin samples, the discouragement effect is largest for whites, while it is insignificant for those with Spanish surnames in the Wisconsin sample.

Our measure of unearned income depends on both the guarantee and tax (or benefit-loss) rate of the welfare program. To see the implications of our estimates consider some illustrations. Suppose that the welfare guarantee is increased by $1,000 from an initial level of $2,400, which is close to the current national average guarantee available to a male-headed family of four, including AFDC-UF or General Assistance and Food Stamps. Using the Michigan data, for white males, we predict a reduction in annual earnings that ranges from $210 per year at a benefit-loss rate of 25 percent to $525 at a benefit-loss rate of 70 percent. At a wage rate of $3.80 an hour, the corresponding reductions in hours of work per year are 55 and 138. For Blacks and persons of other races, the similar reductions are $129 and $323, with annual hours of work going down by 34 and 85.

Now suppose that the welfare program benefit-loss rate is increased by 10 percentage points from an initial level of 40 percent. At a guarantee of $2400 for white males, the predicted decline in earnings is $126, or at a wage of $3.80 an hour, 33 hours per year. Again, the induced decline in earnings is lower for males who are Black or of other races, amounting to $78, or 21 hours annually.

We found relatively little incidence of part-time work. Therefore, these reductions in work effort will mean for the most part increased unemployment (although for some it could mean a reduction in overtime or moonlighting). Moreover, this is extra unemployment that is voluntary. We may conclude then that existing welfare programs induce on the average a small but significant reduction in work effort among most groups in the poor and near poor population.

b. Variability of Earnings

Additional information on the voluntariness of unemployment is provided by our analysis of the variability of earnings. Not surprisingly, unearned income is a significant variable in the earnings variability equations for some groups, reinforcing the conclusion that increased unearned income discourages work effort. If higher unearned income reduces work effort, the reduction can take the form of either more or longer breaks in employment, producing greater variability in earnings.

In addition to unearned income, other signficant factors explaining the variability in earnings include various occupations, industries, locations, and numbers of job changes, as well as characteristics of the worker like health, disabilities, age, and education. These variables reflect labor market conditions beyond the control of the worker as well as some of his own qualities which he also cannot control, but which affect his labor market opportunities. Although these variables are related primarily to involuntary factors, the discussion in Chapter VI indicates that there may also be a voluntary component in some of them. It is thus not possible to say precisely what proportions of the variability in earnings are voluntary or involuntary. It is probably safe to conclude, however, that although on the average there is a voluntary component in the earnings fluctuations of an individual, much of the variability is due to factors beyond his control.

B. Welfare Liberalization and Welfare Dependency

In view of the frequent proposals for welfare reform, it is desirable to anticipate what difficulties would arise if a reform plan were adopted. Our results can give some insights into possible effects on work effort and welfare dependency. The discussion in the previous section suggests that moderate liberalization of welfare programs does not run the risk of eliminating work effort among the poor in general, even if coverage is extended to the working poor. Work and welfare will

will continue to go together, both serially and simultaneously. But liberalization may induce more cutbacks in work among some workers, as returns to work are delayed, overtime and moonlighting reduced, and voluntary job separations increased.

However, the extent of welfare dependency results not only from the labor market experience of individuals, but also depends greatly on the characteristics of the welfare program they face. Dependency, measured by time spent on welfare or amount of payments received over time, can be influenced markedly by simple changes in program characteristics, even if work behavior is completely uninfluenced by the program changes.

Relying on the Wisconsin data, we found not surprisingly that males who averaged high earnings during the experimental period received lower welfare payments than did those with low earnings. But whereas the differences between the two groups were substantial when considering regular welfare, they were relatively minor when looking at NIT payments. Unlike the regular AFDC-UF program, the NIT plans allowed families with working heads to receive payments and earnings simultaneously. Thus, men with "unstable-low" earnings who faced one of the NIT treatments received NIT payments averaging $225 per quarter compared to $231 for those with "stable high" earnings, a difference of only $24 per quarter. In contrast, the difference is much greater for recipients of AFDC-UF, where men with "unstable-low" earnings received an average of $172 in AFDC-UF payments per quarter compared to $53 for those with "stable-high earnings. Similar results are confirmed by the statistical tests of Chapter VII.

Liberalization of welfare programs will extend welfare dependency — simply as a matter of arithmetic. Raising benefit levels, for example, extends coverage and makes it more difficult for people to become totally ineligible. If work effort is affected negatively by liberalization, then dependency will increase for a second reason. Since work effort would undoubtedly decline somewhat, but also since so many more people will be covered, welfare liberalization would greatly expand the tasks of the WIN Program. The main concern with welfare liberalization, however, is likely to be not the reduction in work effort, but the increase in cost.

C. Limitations of the Study

The chief limitation in the study is that it was not able to investigate detailed design elements of the WIN Program. We relied on existing sets of data which are rich in information on work and welfare experience and which provide a picture of the context in which the WIN Program operates. However, an investigation of any specific design element would require a series of detailed questions in a specially designed survey. Since the project did not include its own survey, it could not deal with such questions, but only with the broader issues of the relationships between work and welfare. It should be noted that our continuing research on the work test is designed to evaluate detailed aspects of program operation based on a survey constructed specially for the project. This section concludes with a consideration of two topics not dealt with in this study, the work test and the problem of categorization.

1. Work Tests

A work test, strictly defined, would overcome the work discouragement effects of a transfer payment (or anything else reducing work effort) simply by making the payment conditional on some minimum level of work effort being maintained. However, as a practical matter, work tests are unlikely to operate so easily, since much unemployment is involuntary, resulting from labor market conditions beyond the control of the worker. In addition, some workers have characteristics that employers do not want, making it especially difficult for them to get jobs. Thus, work tests cannot require actual work effort of all; they can only demand some sort of evidence that the unemployed worker is seeking a job actively. The actual work requirement can be applied only once a job is available. Prior to that point, work tests usually are work registration requirements requiring only work search on the part of the registrant. However, a test of job search rather than a straightforward requirement of work opens opportunities for evasion. It is not necessarily a success for a work test to return a person to work; that would probably happen anyway. The critical test is whether the work test induces a person into a job more quickly than he would go on his own. Since voluntary unemployment is largely a matter of timing, it is not obvious that a work test will succeed. This research grant continues in its next phase with an empirical investigation of the effects of actual work tests based on surveys conducted in five cities. Thus, although a work test seems like an obvious device to increase work effort, careful investigation is required to determine whether this actually is the case.

2. The Categorization Problem

Although the desire is often expressed that the poor be encouraged to work, the concern does not apply equally to all poor. For example, it is often felt that women with young children or the disabled should not be required to work. In other words, an attempt is made to distinguish those to be encouraged to work from others. Such a process of categorization is needed in most cases where work encouragement devices are included in the income maintenance program. For example, the WIN Program excludes young persons enrolled in school and the disabled. As another example, if one believed that low tax rates had very little effect on work effort, one might want a categorical negative income tax with a low tax rate and a low guarantee for potential workers, but a high guarantee together with a high tax rate for others. Application of a work test, of course, requires a categorized population. Categorizing schemes can be even more complicated. Suppose, for example, that an income maintenance program involves several treatments to encourage work effort, like job training, counseling, and a work test. Then individuals must be categorized on the basis of their suitability for each of the treatments. Categorization thus is required to implement specific program features designed to encourage work effort.

Individuals can be categorized most readily on the basis of easily observed characteristics, for example, women with children under six. However, we have observed much unexplained variance in earnings in cross-sections of individuals even after controlling for demographic factors. It is likely that any grouping based solely on measurable demographic characteristics will include considerable variety in employment patterns. In our original proposal we suggested a refined approach to categorization. We argued there that anyone can work at some cost. A mother of young children can work if the chidren receive day-care treatment. Most individuals with disabilities can be placed in jobs that they can learn to perform. Individuals lacking skills may be trainable, while those with psychological problems interfering with work effort can recieve counseling. In other words, nearly everyone can be made to work if society is willing to absorb the cost. Now suppose the income maintenance program includes a sum of money for getting participants to work. If all potential recipients are arrayed according to the cost of putting them to work, the available sum should be applied beginning from those with lowest cost up to the point where the given sum is exhausted. That point marks the dividing line between the category of those who are to be required to work and the others. The approach can be modified if one wants to measure the benefits of putting a person to work. The calculation of individual costs may depend on the range of treatments to be offered. Allowing for various complications, the principle involves categorizing people on the basis of their labor market potential rather than some demographic characteristic, only loosely related to suitability for work.

If this principle were to be developed, several kinds of information would be needed. First, the effectiveness of a particular treatment in getting various sorts of individuals to work must be known. As we have discovered, learning the effects of a work test alone requires a major research effort. Second, the cost of each treatment must be established. Third, the various work related problems of individuals must be identified and people classified on the basis of such problems. We had intended to concentrate on the third area.

Serious difficulties beset attempts to classify people on the basis of their work-related problems. The program could establish general guidelines to be applied to each individual by a caseworker. The classification of individuals then depends largely on the judgments of the caseworkers, among whom there are significant differences in judgment. An approach such as we suggested could be valuable mainly if it reduced the discretion of caseworkers in assigning individuals to categories by developing a more specific set of guidelines. Guidelines should satisfy two criteria; 1) if the individual has some characteristics of interest, the guidelines should be such that there is a high probability of detecting that characterisitc; 2) if the individual does not possess some quality, the probability must be high that the guidelines not attribute it to him mistakenly. If follows that a reasonable set of guidelines must first distinguish those worker characteristics of concern to the program. But then there must be a highly reliable formula for predicting whether a worker possesses each of the characteristics.

The variability in the earnings of an individual is only one element contributing to an understanding of his work-related problems. Nevertheless, one can see some of the difficulties in constructing an adequate set of guidelines by examining our equations for the variability in earnings (Chapter VI, Part A). Regression equations like these allow prediction of the dependent variable given any set of values of the independent variables. Thus, as long as we knew the values of the independent variables for an individual, we could predict the value of his dependent variable without needing to observe it directly. The difficulty with prediction is that there is always some error. Now looking at our equations for the variability of earnings, their explanatory power is not too high and it can be shown tha the confidence limits for prediction are wide. That is, the probability is high that for any individual the true variability in his earnings differs substantially from its predicted value. As a consequence, a person could be easily placed in the wrong category of earnings variability on the basis of our equation. More realistically, categories would depend on several characteristics which, like the variability in earnings, are each likely to be predicted imperfectly. Satisfactory guidelines could be established only if the probablity is high that the error in the joint prediction is small.

The difficulty with an equation like ours for earnings variability is not hard to find. The independent variables include a number of easily measurable general characteristics of workers. The equation is useful in identifying which characteristics are significantly related to the variability in earnings and thus helps provide a picture of common elements contributing to the earnings variability of many individuals. Although there are similarities in earnings patterns, however, there are likely also to be unique elements affecting the earnings of many individuals for which the equation cannot control. The reason the predictive power of our equations is low (besides errors in measuring the variables) is thus the omission of many of the actual explanatory variables. It is conceivable that a survey designed to examine the details of work problems could yield more explanatory variables which could improve the predictive power of our equations. Yet there is no possibility of isolating all individual peculiarities and thus eliminating all errors in prediction. The practical question is how much of the variability in earnings (or any other variables of interest) is explained by unique factors peculiar to just single individuals and how much depends on features common to many people. The greater the uniqueness in individual behavior, the less readily research can be used to establish detailed guidelines for a procedure like categorization. But if the guidelines cannot be set reasonably, the process of categorization itself becomes questionable.

It may be that further research will provide the kind of detailed information that is necessary to construct guidelines for categorization. Indeed, programs to encourage work effort face the best chance of success if treatments are applied to those individuals most suited for them -- that is, if the recipient population can be adequately categorized. However, our results cannot be stated with the definiteness required to establish an adequate system of categorization.