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INTERPRETING THE RESULTS OF SHORT-DURATION INCOME-MAINTENANCE EXPERIMENTS: AN INVESTIGATION OF BIASES IN PREDICTING LONG-RUN BEHAVIOR

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Abstract

Among the many problems in interpreting results from the on-going income-maintenance experiments, perhaps the most challenging is that of inferring what the behavioral responses to a permanent negative income tax (NIT) will be on the basis of a short-duration experiment. This paper examines that problem with respect to the labor-supply response of the participants. A model of labor supply is presented which indicates that the response of a "rational" individual to a temporary NIT would differ from the response to a permanent NIT, and that a temporary experiment will consequently yield a biased prediction of "permanent" behavior.

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Among the many problems in interpreting results from the on-going income-maintenance experiments, perhaps the most challenging is that of inferring what the behavioral responses to a permanent negative income tax (NIT) will be on the basis of a short-duration experiment. This paper examines that problem with respect to the labor-supply response of the participants. The first section of the paper presents a model of labor supply which indicates that the response of a "rational" individual to a temporary NIT would differ from the response to a permanent NIT, and that a temporary experiment will consequently yield a biased prediction of "permanent" behavior. In the second section certain qualitative statements are made about the nature of the biases and several strategies for directly measuring them are explored. In the third and fourth sections, data generated from the Graduated Work Incentive Experiment (GWIE) are examined to check their consistency with the theory developed. The last section deals with several complicating factors which both modify the qualitative predictions of the theory and raise other problems which are inherent in dealing with experiments of short duration.

I. THE PREDICTED EFFECT OF A NEGATIVE INCOME TAX ON LABOR-FORCE BEHAVIOR

Income and Substitution Effects on Labor Supply

The labor-supply decision of an individual serves to allocate his consumption between goods and leisure (or nonmarket uses of time). If he is free to vary his hours of market work, he will consume leisure (withhold labor services) up to the point where the last hour of leisure is just worth its price in terms of purchased goods.

By taxing earned income at a positive rate, a NIT has the effect of lowering the net wage rate in addition to increasing the income of an individual. Thus, if leisure is a "normal" good, a NIT reduces the supply of labor, compared to a situation where no such program is in effect, for two reasons: (1) part of the increased income can be consumed in the form of additional leisure time; and (2) the reduction in the wage rate makes leisure less expensive relative to other goods.

Correspondingly, an individual placed under an income-maintenance experiment faces conditions differing from those created by a permanent NIT in two respects. First, he is faced with a temporary rather than a permanent increase in his income stream. The permanent income hypothesis (discussed in section III) suggests that his absolute response to the income change from a temporary plan will be smaller than his response to the income change from the same plan on a permanent basis. Clearly, the present value or wealth of a permanent plan is larger, and this leads to a larger income effect. Second, he is faced with a temporary rather than a permanent change in the "price" of leisure (or effective wage rate). The transitory nature of the price change will cause him to substitute current for future consumption. In a permanent plan, unlike the experiment, the price of leisure would be the same in future periods as in the present period; consequently there would be no cross-substitution effect amplifying the own-substitution effect of the current price-of-leisure reduction. Thus, the experimentally observed price effect provides an exaggerated measure of the price effect of a permanent NIT.

What is discussed, therefore, is a welfare program expected to have both "income" and "substitution" effects, and an experimental adoption of the program which yields a downward bias in the estimated "income" effect, and an upward bias in the estimated "substitution" effect. Detection and measurement of these biases is essential to a proper interpretation of the results of the experiment. To sort out the biases, the experiment must be capable of identifying separate estimates of the income and substitution effects. This requirement provides an additional ex post rationalization for varying income guaranteees and tax rates independently across sample points in an NIT experiment, rather than a simple two-way comparison between a single NIT scheme and a control group..

The Biases in the Income Effect

The consumer is assumed to maximize a discounted sum of utility over a lifetime (or time horizon) of N time periods, where the length of the experiment is chosen as the unit of time measurement. The effects of an NIT in force during the first period can be compared with the effects of an NIT in force through all N periods of his remaining lifetime. The size of the permanent income effect relative to the observed temporary income effect depends upon the present value of respective income increases resulting from permanent and experimental NIT plans. The size of the income-effect bias depends, therefore, upon the time horizon of the individual, N, and upon the interest rate, r, facing the individual.

Let L_1 be the quantity of leisure consumed during the experimental period, G_1 the size of the experimental income guarantee, and G the guarantee of equal magnitude for a corresponding permanent plan; then the relationship between the effects of the permanent and temporary income changes on the demand for leisure of a household under the "break-even point" can be expressed as follows:[1]

$$\frac{\delta L_1}{\delta G} = \frac{\delta L_1}{\delta G_1} \sum_{i=1}^{N} (1+r)^{1-i} = \frac{\delta L_1}{\delta G_1} (1+R), \quad \text{where} \tag{1}$$

$$R = \left[\frac{1 - (1+r)^{1-N}}{r}\right].$$

Thus, the value of R determines the magnitude of the income-effect bias. Table 1 indicates that for plausible interest rates and time horizons, the bias is quite large relative to the measured income coefficient. For instance, if a household on a three-year experiment faces a real annual interest rate of 10 percent and a time horizon of 30 years (N = 10), the bias in the income effect would be 2.8 times the measured current income effect. Even if the time horizon is as short as two times the length of the experiment, the bias would be 75 percent as large as the measured coefficient. It should be pointed out that while many have argued that low-income households have very short-time horizons, it is not clear that evidence used to support these arguments in fact supports the existence of short-time horizons as opposed to high discount rates.[2] In equilibrium the household is assumed to adjust its discount rate to the market rate of interest; thus, we deal with interest rates rather than subjective discount rates.

The Bias in the Price Effect

A negative-income-tax plan has the effect of lowering the "price" of leisure, that is, the wage rate. The effect of any price change can be decomposed into "income" and "substitution" effects. The former indicates the response

Table 1

Values of R for Selected Interest Rates and Time Horizon Lengths

Annual Interest Rate	r	Value of R					
		N=1	N=2	N=5	N=10	N=20	N=∞
.05	.1576	0	.864	2.812	4.645	5.952	6.435
.08	.2597	0	.794	2.321	3.369	3.803	3.851
.10	.3310	0	.751	2.059	2.791	3.008	3.021
.12	.3937	0	.718	1.867	2.412	2.535	2.540
.15	.5209	0	.658	1.561	1.876	1.913	1.920
.20	.7280	0	.579	1.220	1.364	1.374	1.374

Notes:

$$R = \frac{1 - (1+r)^{1-N}}{r}$$

r = real interest rate (3-year base period)

N = number of periods in lifetime (3-year periods) due to the effect of a price change on the household's real income. The latter indicates the remaining price effect after the household has been compensated for any change in real income.

The "gross" price effect of the experimental change in the wage rate, W_1, can be written as:

$$- \frac{\delta L_1}{\delta W_1} = - (\bar{L} - L_1) \cdot \frac{\delta L_1}{\delta G_1} - \left(\frac{\delta L_1}{\delta W_1}\right)^* , \tag{2}$$

where $-\left(\frac{\delta L_1}{\delta W_1}\right)^*$ is the "substitution" effect or compensated-price effect of the wage-rate change.[3] Both components of equation (2) provide biased representations of the effects of a permanent wage-rate change.

The relative bias in the income component is approximately equal to the income-effect bias described by equation (1).[4] Thus, if leisure is a "normal" good, the negative influence of the income component in equation (2) will be understated by the experiment. For reasons to be stated below, the positive influence of the substitution effect, $-\left(\frac{\delta L_1}{\delta W_1}\right)^*$, will be overstated by the experiment. Under these circumstances both components of the bias work in the direction of overstating the value of $-\frac{\delta L_1}{\delta W_1}$, that is, making the experimental price effect on leisure more positive than a permanent effect. The direction of the bias is unambiguous, even though the gross price effect itself may be positive or negative.

II. STRATEGIES FOR MEASURING THE BIASES

Income Effects

The bias in the income effect was shown in section I to depend on the value of R, which was defined as a function of the interest rate and the time horizon.

While R can be calculated if direct estimates of the interest rate and the time horizon are available, it is useful to know whether the experiment itself is capable of generating information about the size of the bias. Such knowledge is useful for two reasons. First, if direct evidence concerning r and N is not available, it provides a substitute source of information; second, if direct evidence concerning r and N are available, it provides an independent, indirect source of evidence which must be consistent with the direct evidence if our underlying theory is to be accepted as a mode for analyzing biases in the experimental results. Two methods of estimating R will be outlined here.

1. If sufficient assumptions are made to imply a uniform lifetime consumption stream for the individual, the marginal propensity to consume (MPC) out of "permanent" income will be equal to one. (The value of leisure time is included in both the consumption and income definitions.) In this case (as is shown in section III) $\frac{1}{(1+R)}$ equals the observed experimental marginal propensity to consume. Therefore, if the consumption and saving behavior of households can be measured, an estimate of R can be generated. Measurement problems become critical at this point, however. For instance, since durable goods are purchased for consumption over several time periods, household expenditures may provide an overestimate of actual consumption during the experiment. It is therefore essential that information be collected concerning stocks of durable goods held by experimental households. Similarly, it is possible that some uses of leisure time have an "investment" component which should not be included in a measure of current consumption.

If the lifetime consumption stream for the household is not uniform, the MPC out of permanent income need no longer be equal to one. In this more general case, $\frac{1}{(1+R)}$ equals the temporary MPC as a fraction of the permanent MPC.

Information is required about behavioral responses to income changes perceived to be permanent. Two possibilities come to mind in this instance. First, if the households under observation experience fluctuations both in nontransfer income and in transfer income, and if we have information about the perceived permanence of changes in nontransfer income, then the difference in consumption responses to changes in various sources of income can yield information about the value of R. (This procedure would be complicated by the fact that the experiment itself is presumably inducing changes in earned income as well as transfer income.) Second, independent time-profile data regarding lifetime-consumption data could conceivably be exploited to generate estimates of a permanent MPC by age.

2. If we accept the assertion that all households have a time horizon equal to their remaining life expectancy--that is, that the importance of the future in current planning can be described by the size of the discount or interest rate rather than by some notion of a planning horizon which varies in length across individuals of the same age--then the implicit interest rate faced by the individual, r, can be measured by varying the length of the experiment relative to the lifetime of the individual.

One obvious strategy for accomplishing this would be to allocate the NIT sample to plans of unequal duration. The difference in income responses of the groups on experiments of unequal duration can be derived by solving the expression

$$\frac{\partial L_1}{\partial G_T} = \frac{\partial L_1}{\partial G_1} \cdot \left[\frac{(1+r) - (1+r)^{1-T}}{r}\right] \tag{3}$$

for r, where G_T and G_1 denote the (same) income guarantee provided in experiments of T periods and one period respectively. It should be pointed out that this strategy would be successful only if households are informed of the duration of their payments at the beginning of the experiment; a partial extension of the experiment announced at the end of the initial period does not achieve the same effect.

An alternative, less robust, strategy would be to maintain a uniform experimental period but to vary the length of life of the participants implicitly by observing the relationship between the observed labor-force response and the age of the participant. The means of "control" in such a procedure is tenuous, however, since it is assumed that the only reason persons of different ages behave differently is the length of time remaining until death. To the extent that age represents seniority, physical vigor, and other factors which affect labor supply, this assumption would not be tenable. In addition, the use of age as a proxy for the time-horizon duration would be extremely sensitive to assumptions made about the degree of certainty with which households perceive their length of life. Nevertheless, information about the age of participants can be used to supplement whatever alternative procedures are used to measure r.

The Substitution Effect

Recall that the bias in the estimated substitution component of the price effect is related to the fact that the net wage rate is changed only in the experimental period rather than in all periods. In order to isolate this bias, we utilize a fundamental result of neoclassical consumption theory; the price-weighted sum of compensated effects of a single price change on the consumption of all goods is zero. In a multiperiod context, this principle applies to the effects across all time periods of a given price change.

Two applications of this result are relevant for our purposes. First, the weighted sum of compensated effects of a permanent change in the wage rate can be written as:

$$-\left[\left(\frac{\partial C_1}{\partial W}\right)^* + W\left(\frac{\partial L_1}{\partial W}\right)^*\right] - \frac{1}{(1+r)}\left[\left(\frac{\partial C_2}{\partial W}\right)^* + W\left(\frac{\partial L_2}{\partial W}\right)^*\right] = 0, \quad (4)$$

for a two-period illustrative example.[5] In this case a permanently imposed NIT would uniformly affect the price of leisure in every time period, and there would be no reason a priori to expect intertemporal substitution effects to occur. Thus, while it is not strictly necessary for the most general case, it would be reasonable to expect that each bracketed sum within equation (4) would equal approximately zero as well. For example, we would expect to observe:

$$-\left[\left(\frac{\partial C_1}{\partial W}\right)^* + W\left(\frac{\partial L_1}{\partial W}\right)^*\right] \sim 0. \quad (5)$$

Second, the expression corresponding to equation (4) for an experimental change in the wage rate is:

$$-\left[\left(\frac{\partial C_1}{\partial W_1}\right)^* + W_1\left(\frac{\partial L_1}{\partial W_1}\right)^*\right] - \left(\frac{1}{1+r}\right)\left[\left(\frac{\partial C_2}{\partial W_1}\right)^* + W_2\left(\frac{\partial L_2}{\partial W_1}\right)^*\right] = 0. \quad (6)$$

In this case, however, the net wage rate changes only in the experimental time period. Since the experiment therefore distorts relative prices across time periods, we would normally expect that intertemporal substitution would occur, such that:

$$-\left[\left(\frac{\partial C_1}{\partial W_1}\right)^* + W_1 \left(\frac{\partial L_1}{\partial W_1}\right)^*\right] > 0, \qquad (7)$$

where equation (7) specifies the value of increased consumption during the experimental period due to the intertemporal substitution. Thus, equation (7) corresponds roughly to a weighted sum of <u>all</u> biases in current price effects, not just the leisure bias. In order to identify the portion of the total bias that applies to the quantity of leisure consumed, we can exploit the assumption that an individual's total satisfaction can be expressed as a discounted sum of satisfaction attained in all time periods. If the individual's utility function is additive in this intertemporal sense, it can be shown that the allocation of substitution effects of a future price change on current levels of consumption will be proportional to the effects of an income change on current levels of consumption.[6] Since the source of our experimental substitution bias can be viewed as deriving from our <u>failure to change the future wage rate</u>, current substitution biases will also be proportional to the observed effects of an income change.

Given the above results, the bias in the observed weighted value $-W_1\left(\frac{\delta L_1}{\delta W_1}\right)^*$ will be approximately equal to the weighted sum of all biases multiplied by the fraction of an increase in income which is consumed in the form of leisure. Namely,

$$-W_1\left[\left(\frac{\partial L_1}{\partial W}\right)^* - \left(\frac{\partial L_1}{\partial W_1}\right)^*\right] \sim \left[\left(\frac{\partial C_1}{\partial W_1}\right)^* + W_1\left(\frac{\partial L_1}{\partial W_1}\right)^*\right] \cdot \left\{\frac{W_1 \frac{\partial L_1}{\partial G_1}}{\frac{\partial C_1}{\partial G_1} + W_1 \frac{\partial L_1}{\partial G_1}}\right\}; \qquad (8)$$

that is, we have

$$\left\{\text{Value of Bias in Leisure Substitution Effect}\right\} \cong \tag{8a}$$

$$\left\{\text{Total Value of Intertemporal Substitution}\right\} \cdot \left\{\text{Marginal Budget Share of Leisure}\right\}.$$

The expression in (8) can be measured directly, given the assumptions we have made about the additive nature of preference over time. It does not, however, provide us with an intuitive feel for the significance of the magnitude of the substitution bias. Given substantial additional assumptions about the nature of the individual preferences, it can be shown that the relative size of the bias can be approximated by the following relationship:

$$\frac{-\left[\left(\frac{\partial L_1}{\partial W}\right)^* - \left(\frac{\partial L_1}{\partial W_1}\right)^*\right]}{-\left(\frac{\partial L_1}{\partial W}\right)^*} \cong (\mathrm{MPS}) \cdot \left[\frac{W_1 \frac{\partial L_1}{\partial G_1}}{\frac{\partial C_1}{\partial G_1}}\right], \tag{9}$$

where MPS is the experimentally observed marginal propensity to save out of increments to income. Thus, the higher the MPS, and the higher the relative marginal share of leisure in the consumption bundle, the larger will be the substitution effect. It should be emphasized that expression (8) is of more general applicability than expression (9).[7]

The reader should again be reminded of the problems associated with a proper measurement of the marginal propensity to save, discussed earlier. In addition, it should be reiterated that the budget shares appearing in the above approximations are marginal, not average, concepts.

III. EVIDENCE ON THE TRANSITORY NATURE OF NIT PAYMENTS USING DATA ON HOUSEHOLD EXPENDITURES AND SAVING: THEORETICAL ISSUES

The basis for using data on expenditures for testing the degree to which experimental NIT payments are perceived to be transitory is the "permanent income hypothesis" (PIH) associated with Milton Friedman.[8] In its broad outlines this theory probably has the widest current acceptance among economists, although there is frequently considerable controversy over specific hypotheses (or predictions) that are made when the theory is applied in particular situations. Certainly the body of theoretical and empirical work on the topics of consumption and saving done in the framework of the PIH is larger than that associated with competing theories.

The basic idea of the PIH is that individuals or households adjust their consumption expenditures to their normal or permanent incomes, and that they will attempt to maintain that level of consumption despite short-run fluctuations in their actual current income. If we consider the identity that income, y, equals consumption, c, plus saving, s, then it is clear that the way in which consumption (or one's "standard of living") is maintained is by saving more (or repaying debts) when income is temporarily low. The theory implies that transitory income--a windfall gain or loss--will change the household's savings account, but it will not change (or change only slightly) the household's expenditures on consumer goods and services.

Underlying this intuitive and reasonable proposition is, of course, a model that is both more explicit and more general. Without restating the theory in its rigorous formulation, we can describe its main outline to facilitate our use of the theory in interpreting the data from the income-maintenance experiment.

A general formulation of the PIH is that consumption over a period of time (let this be the length of the experiment for the purpose of this discussion) depends on the value of one's wealth, V. Expressing this relation in its simplest form, we have:

$$c = \beta V. \tag{10}$$

Wealth depends upon the annual returns to one's human and nonhuman assets (which is one definition of annual income), the number of years for which returns are generated, and the interest rate at which the individual discounts all future returns.

Since wealth is not known with certainty, it is convenient to use expected wealth as the variable affecting consumption. Expected wealth can be converted, given an interest rate, into a periodic income flow that would be sustainable forever. The periodic flow would equal kV, where k is the conversion factor. Clearly, the conversion of that same amount of wealth into an annual income flow that would be sustainable just over one's lifetime would be larger the shorter is one's lifetime, conversely, the larger would be the conversion factor. This conversion factor is, in fact, $\frac{1}{(1+R)}$, as the term was defined in the first section. Wealth multiplied by $\frac{1}{(1+R)}$ may be defined as permanent income, y_p, namely:

$$y_p = \frac{V}{(1+R)} . \tag{11}$$

This is the amount of maximum sustainable consumption per period over one's lifetime. For various reasons we might expect some fraction slightly less than one, let us say α, to be actually consumed each period. We can write

$$c = \alpha y_p, \tag{12}$$

where

$$\beta(1+R) = \alpha \leqq 1.$$

The size of α would be affected by the uncertainty of one's life span, the stage in one's life cycle, a desire to leave an inheritance, and various other characteristics of the individual household.

Current income for a given time period, y_c, is defined to be a sum of a permanent component and a transitory component:

$$y_c = y_p + y_t. \tag{13}$$

The dependence of consumption on permanent income (equation 12), and the definition of the latter in terms of wealth (equation 11), implies that transitory income affects consumption only as it affects wealth. Assume, for example, that y_t is a windfall gain. The wealth of the individual is $V + y_t$; his previous permanent income, y_p^o, is increased by the amount $\frac{1}{(1+R)} \cdot y_t$. The relation between consumption and the new amount of income can be written:

$$c = \alpha \left[y_p^o + \left(\frac{1}{1+R}\right) y_t \right] . \tag{14}$$

If we estimate this relation on the basis of a simple, but somewhat more general, linear model with a stochastic component, u, we have:

$$c = \alpha_0 + \alpha_1 y_p^o + \alpha_2 y_t + u. \tag{15}$$

The theory implies that $\alpha_2 = \frac{\alpha_1}{(1+R)}$. Since $\frac{1}{(1+R)}$ is a number like .25 or .50, the coefficient of y_t should be smaller than the coefficient of y_p by a factor of from 2 to 4. Clearly, if the model is correctly specified it is possible to determine (1+R) as α_1/α_2, and then to use R as a direct measure of the bias associated with the short duration of the experiment, as discussed in the second section.

In the Graduated Work Incentive Experiment (GWIE) it is reasonable to view the NIT payments received by each family as transitory because each family has been told that the experiment will last only three years. The lower the interest rate and/or the longer is the time horizon which the individual applies to the NIT amount, the larger is the value of R. Higher R values widen the differences between the transfer and its face value of y_p, and widen the difference between α_2 and α_1. If, alternatively, the recipients of NIT believe or act as if the experiment were permanent, they would look upon the NIT payments as components of permanent income. We could then expect no difference in their consumption behavior from that of the central group with the same characteristics and the same amounts of total income--assuming that the main component of variation in reported total income is due to differences in permanent income. At the present stage of our empirical work, unfortunately, we have little confidence in our ability to identify permanent components of reported income.[9]

Ideally the income of households participating in the short-duration experiment would be divided into three components: the "permanent" component (such as pay from their regular job), y_p; the NIT experiment payments, y_t^*; and the rest of the transitory component (such as that due to overtime or, conversely, unemployment), y_t. The consumption equation would then appear as follows:

$$c = \alpha_0 + \alpha_1 y_p + \alpha_2 y_t + \alpha_2^* y_t^* + u. \qquad (16)$$

A persistent problem in using and testing the PIH is the unobservable nature of permanent income (or, equivalently, of wealth and the appropriate discount rate). The observations available are for current income, and in general these include an unknown mix of permanent and transitory components. This means that we cannot measure α_1, α_2, and α^* directly and cannot, therefore, measure the appropriate value of R that applies to each of these components of income.

One model available for estimation is a reformulation of equation (16) but with current income replacing the unobservable y_p and y_t. Thus,

$$c = \alpha_0 + \alpha_c y_c + \alpha^* y_t^* + u \tag{17}$$

where $y_c = y_p + y_t$ and α_c is a weighted average of α_1 and α_2. Using the two-fold assumption that (1) y_p is the source of a much larger component of variations in y_c than is y_t and (2) α^* is approximately equal to α_2, we can calculate α^*/α_c (expected to be less than one) as an upper bound on the discounting factor $\frac{1}{1+R}$ that applies to NIT payments.[10] This is obviously a less satisfactory piece of information than a direct estimate of $\frac{1}{1+R}$, or even a range of values that places a lower as well as an upper bound on the estimate. Indeed, equation (16) will not be estimated directly in this paper, since data for consumption have not yet been completely tabulated. Instead, we shall examine several models of selected aspects of consumption and saving behavior.

Another question arises as to whether replacing y_p by y_c in the model introduces bias in the estimated value of α^*, the coefficient on NIT experimental payments. The answer is unclear, as the following simplified situations illustrate. First, consider an example in which no bias results. Let the "true" model be

$$\begin{aligned} c &= a_0 + a_1 y_p + a_2 y_t + a_3 \text{ (NIT)} \qquad (18) \\ &= a_0 + a_1 y_p + a_2 y_t + a_3 [G - T(y_p + y_t)] \end{aligned}$$

If y_p and y_t are held constant the only sources of variation in NIT with which to measure a_3 are the guarantee, G, and the implicit tax rate, T, which were varied across households in the experimental design.

Replacing equation (18) with the estimated model

$$
\begin{aligned}
c &= b_0 + b_1 y_c + b_3 \text{ (NIT)} \\
&= b_0 + b_1 y_c + b_3 \text{ } (G - Ty_c)
\end{aligned}
\tag{19}
$$

is inconsequential in this case, since with y_c held constant the only variation in NIT again is that associated with G and T. Thus, the expected value of the estimated coefficient b_3 should equal the true coefficient a_3.

If, however, y_c is measured during the course of the experiment rather than as a preexperiment value, we would expect higher values of G and T to be associated with lower values of y_c, due to the work disincentive effect. Under these conditions, b_3 may be a biased estimate of a_3.[11]

Before turning to the empirical work with data from the GWIE we should reiterate that, in addition to the problem of measurement of income, another difficulty in applying the PIH (or any other theory of consumption behavior) is distinguishing consumption from investment (or saving). (This difficulty is present in our treatment of "leisure" (or nonmarket activity) as well as of cash consumption, since the distinction between investment-type activities and consumption activities in the nonmarket sphere is ignored). When investment expenditures take the form of accumulated deposits in a checking or savings account, no important ambiguities are present--this is "pure" saving, not consumption. Similarly, food purchases represent, for all practical purposes "pure" consumption and not saving. Consumer durables on the other hand, typically constitute part consumption and part investment. For example, the purchase of a house is primarily an investment, and only the annual use or rental value of the house should be considered as consumption. Automobile purchases have a relatively larger consumption component than houses but a smaller one than clothing. In the work reported below, we try to exploit these distinctions as an alternative way to approach the ideal measure of consumption which the PIH requires.

IV. EVIDENCE ON THE TRANSITORY NATURE OF NIT PAYMENTS USING DATA ON HOUSEHOLD EXPENDITURES AND SAVING: EMPIRICAL WORK

The empirical work reported in this section should be viewed as preliminary and suggestive only of the methods to be used when the experiment is completed. At this stage of our analysis consumption and income data from GWIE are available for only a maximum of six quarters, and the expenditure data is available only for certain items.

Measures of Aggregate Behavior

As an overall check on whether families receiving NIT payments (that is, the treatment families) are consuming less and saving more than control families, we can look at the ratio to average income of debt and of average expenditures on food, clothes, durable goods, and savings in financial assets. Depending on the sample of observations used, NIT payments make up between 11 and 15 percent of family income for the treatment families, so their average propensities to consume and to save should differ in a predictable way based on the weight of the transitory component (that is, the NIT payments).

As shown in Table 2, the ratios do not unambiguously indicate more savings and less consumption by treatment families, although there is some tendency in this direction. The treatment families spend a lower fraction of their income on food than control families, .36 compared to .39,[12] which agrees with the theoretical expectation. The ratio of expenditures on clothing is the

Table 2

Expenditures on Selected Items, Savings, and Debts as a Percent of Income, Treatment and Control Groups in the GWIE*

Item	Treatment Families (Ts) (percent)	Control Families (Cs) (percent)
Food[a]	36.0	39.0
Clothing[b]	7.0	7.0
Durable goods[c]		
First quarter to sixth quarter	3.3	2.5
Savings in financial assets[d]		
Preenrollment	1.6	1.6
First quarter	1.5	1.8
Sixth quarter	2.7	1.7
Debts[e]		
Preenrollment	15.8	18.0
First quarter	17.1	17.5
Sixth quarter	29.2	23.6

*Tabulations are based on families with reported annual incomes over $2,000 and with some positive amount of food and clothing expenditures. A slightly different sample was used for the savings and debt calculations, after excluding observations with missing data for these variables. These restrictions were made in an effort to increase the level of accuracy of the data used.

[a]The questions referred to expenditures last week for food prepared at home (mean=$45 for Ts and $43 for Cs). This amount times 52 was divided by annual income (mean=$6, 566 for Ts and $5,732 for Cs): n=461 Ts, 301 Cs.

[b]The question referred to clothing purchases during the last six months (mean=$225 for Ts and $196 for Cs). This amount times two was divided by annual income (see footnote a). For the mean incomes and sample sizes, for the T and C groups.

[c]Durable goods expenditures were derived by subtracting durable goods stocks; sixth quarter stock values minus first quarter stock values. Durable goods consist of automobiles, furniture, home production appliances (such as washing machines, sewing machines, dish washers, stoves, refrigerators, and vacuum cleaners), and "other" appliances (including television sets, radios, air conditioners, and motor cycles.) Note that homes are not included. The stock values for these durable goods were relatively low, amounting to around $1200 per family. Many families did not own a car. The changes in stock values, which correspond to an economic definition of net expenditures (that is, expenditures minus depreciation or sales) were also relatively low: $213 for treatment families and $147 for control families.

Table 2 (cont.)

[d]Savings in financial assets refers to the amount of money in checking accounts or in any kind of savings accounts. (Means for Ts are $107, $97, and $179 at the time of preenrollment, the first quarter after NIT payments were made, and the sixth quarter, respectively. Means for Cs are $97, $108, and $101 at the three points in time, respectively. These amounts were divided by the amount of income reported in 1969--roughly the period from the first quarter through the fourth quarter. The average income for Ts was $6,366 for a sample of 440. The average income for Cs was $5,989 for a sample of 351.)

[e]Debts are defined as total amounts of money owed, excluding mortgages, although, inadvertently, some mortgage debts were reported in the sixth quarterly. (Means for the Ts are $1,063, $1,101, and $1,709 at the time of preenrollment the first quarter after NIT payments were made, and the sixth quarter respectively. Means for Cs are $1,053, $1,142, and $1,946 at the three points in time, respectively. See footnote d for the incomes and sample size of the T and C groups.)

same, .07, for both groups.[13] Net expenditures on durable goods were slightly higher for treatment families, in accordance with the PIH, but the difference is small and statistically insignificant.[14] The absolute amount of expenditures [or change in stock values (see footnote 15)] was $213 for treatment families and $147 for control families, and the small difference was not accounted for by NIT payments (as will be shown below in the discussion of the regression analyses).

The ratio of savings in the form of liquid assets to income was the same before the experiment began for the two groups of families, and by the sixth quarter the treatment families had increased their mean savings by 67 percent (from $107 to $179) while the control families' savings rose by only 4 percent (from a mean of $97 to $101). The direction of the differences in savings is in agreement with the PIH and the view that NIT payments are regarded as transitory income. However, the absolute amount of the difference in savings increases appears small--only a $72 increase for the treatment families and a $4 increase for control families in comparison to the 1969 average NIT payment of $784.

Since much of the savings of poor people may be in the form of paying off debts, it is interesting to examine the changes in debt/income ratios for the two groups. Contrary to our expectations, the treatment families increased their debt by a larger amount than control families. Prior to the experiment both groups had debts outstanding (not including mortgages) of about $1000. The treatment families increased their debts by about $900, compared to a mean increase of about $400 on the part of the control families. The explanation that this debt increase for treatment families actually corresponds to positive savings in the form of durable goods purchases does not hold up at this point in the investigation, since durable purchases are only slightly higher for treatment families--not enough to explain their debt increase.

The evidence from the aggregative statistics is in general inconclusive as to whether NIT payments are viewed as transitory, and attempts to use these data to quantify the magnitude of the time horizon or discount rate that applies to NIT payments would be inappropriate. Unfortunately, tests using regression analysis of disaggregated data are also unsatisfactory and inconclusive at this early stage. They will be discussed briefly to illustrate the methodological issues involved.

Statistical Tests Using Disaggregated Data

There are several advantages in using individual family data from the GWIE. Over one thousand observations are potentially available to estimate experimental effects on consumption and saving in the context of models which include a number of control variables. The basic model estimated is similar to equation (17), except that more components of family income can be identified and their separate effects measured.

$$c = \alpha_0 + \alpha_1 y_e + \alpha_2 y_{nwc} + \alpha_3 y_{wc} + \alpha^* y_{nit} + \alpha_4 D + BX + u. \tag{20}$$

The symbol, c, stands for some measure of consumption per family, like food or clothing expenditures, D is a dummy variable taking the value 1 if the family is in the control group, and 0 if in the treatment group. X is a symbol representing a vector of other conditioning variables, such as ethnicity and location, and u is the stochastic term. Income is divided into four components: y_e is wage and salary earnings or labor earnings from self-employment; y_{nwc} is nonlabor income that is not "conditioned" (or responsive to) work behavior, such as property income, rent, dividends, or the like; y_{wc} is nonlabor income that is work conditioned, like welfare payments or unemployment compensation; y_{nit} is the negative-income-tax payment received by the treatment families. We are interested in the three income components other than y_{nwc}, because this latter type of nonlabor income is relatively unusual among the nonaged working poor, and in our sample has an average value of only $50 per family per year.

One hypothesis is that $\alpha_1 > \alpha^*$ on the grounds that earnings represent a more permanent form of income than NIT payments. The null hypothesis is, in this context, $\alpha_1 = \alpha^*$. A second hypothesis is that $\alpha^* < \alpha_3 < \alpha_1$, reasoning that NIT payments are viewed as more transitory than other work-conditioned payments, which are, in turn, viewed as more transitory than earnings. A third hypothesis concerns the dummy variable, D, that denotes being in the control group. Since the model explicitly includes the value of the NIT payments, there is no obvious reason why the consumption and saving behavior should differ for the two groups, although it could be argued that the income security offered the treatment families would cause them to spend more and save less, even when they are above the breakeven point and receive no NIT payments. Hawthorne effects of undeterminable sign could also be present--another reason to include the D variable.

Food Expenditure Regression Results

Table 3 shows the results of a regression model like equation (20) for expenditures on food prepared for home consumption. As in Table 2, the data used are confined to families with reported incomes in 1969 of $2000 or more and with some positive amount reported spent on food and clothing. Four specifications were used; two with the dependent variable measured as dollars spent per year on food (with and without the control group) and two with the dependent variable measured as the ratio of food expenditures to income (with and without the control group).

Table 3

Regression Results with Food Expenditures as Dependent Variable, Models I-IV

Independent Variables	All Families: I. Dollars of Annual Food Expenditures as Dependent Variable	All Families: II. Ratio: Food Expenditures/ Income as Dependent Variable	Treatment Families Only: III. Dollars of Annual Food Expenditures as Dependent Variable	Treatment Families Only: IV. Ratio: Food Expenditures/ Income as Dependent Variable
1. Earnings, y_e	.040 (3.49)*	-.0000559 (23.28)*	.042 (2.74)*	-.0000523 (18.02)*
Total income elasticity	.109		.118	
2. Nonwork-conditioned income, y_{nwc}	.089 (1.14)	-.0000289 (1.78)	.293 (2.66)*	.0000113 (.55)
Total income elasticity	.242		.824	
3. Work-conditioned income, y_{wc}	.088 (3.71)*	-.0000492 (9.99)*	.086 (2.80)*	-.0000459 (7.98)*
Total income elasticity	.240		.242	

Table 3 (cont.)

Independent Variables	All Families: I. Dollars of Annual Food Expenditures as Dependent Variable	All Families: II. Ratio: Food Expenditures/ Income as Dependent Variable	Treatment Families Only: III. Dollars of Annual Food Expenditures as Dependent Variable	Treatment Families Only: IV. Ratio: Food Expenditures/ Income as Dependent Variable
4. NIT payments, y_{nit}	.136 (3.45)*	-.0000488 (5.95)*	.140 (3.31)*	-.0000442 (5.57)*
Total income elasticity	.370		.390	
5. T = in control group	17.0 (.33)	.0067 (.63)	----	----
6. Family size	274. (6.52)*	.053 (6.14)	216. (3.94)*	.036 (3.51)
7. Family size squared	-7.98 (2.62)*	-.0018 (2.89)*	-3.65 (.93)	-.001 (.95)
8. Black	-573. (9.26)*	-.098 (8.17)*	-562. (7.55)*	-.095 (6.77)*
9. White	-77. (.95)	-.018 (1.09)	-108. (1.07)	-.019 (1.03)
10. Trenton	-90. (.88)	-.003 (.14)	-139. (1.13)	-.013 (.57)

Table 3 (cont.)

	All Families		Treatment Families Only	
Independent Variables	I. Dollars of Annual Food Expenditures as Dependent Variable	II. Ratio: Food Expenditures/ Income as Dependent Variable	III. Dollars of Annual Food Expenditures as Dependent Variable	IV. Ratio: Food Expenditures/ Income as Dependent Variable
11. Paterson-Passaic	154. (1.83)	.032 (1.87)	159. (1.52)	.035 (1.78)
12. Jersey City	137. (1.65)	.032 (1.88)	89. (.83)	.022 (1.08)
13. Constant	776. (4.66)*	.51 (14.78)*	965. (4.60)*	.54 (13.82)*
R^2	.39	.49	.42	.47
Standard deviation of residuals	564	.12	575	.11
n	762	762	461	461

Notes: Numbers in parentheses are regression coefficients and t-ratios. Asterisk (*) indicates that the coefficient is significant at the 5 percent level (t = 1.96).

The coefficients of the income components reveal some interesting differences, but they are counter to the theoretically expected result. The effect of a dollar of NIT payments is to increase food purchases by about 14 cents for both groups--all families and just the treatment families. This appears to be a small marginal propensity to spend on food, but it is larger than that for the earnings variable. The effect of a dollar increase in earnings is to increase food expenditures by only four cents. The elasticities of food purchases with respect to income, based on the coefficients of the income components, are also shown in Table 3, and range between .11 for earnings and .37 for NIT payments.[15]

When the dependent variable is the ratio of food expenditures to total income $\left(\frac{c_f}{y}\right)$, the coefficients for earnings and NIT payments are similar, but the earnings coefficient is more negative--implying a lower effect on food expenditures. The problem here may be that a spurious negative correlation exists between the ratio, $\frac{c_f}{y}$, and the independent variable, earnings, y_e, since y_e constitutes about 80 percent of y. Thus, the coefficient of y_e may be biased and more negative than its "true" coefficient simply because any random errors in the measurement of y_e will produce a negative correlation between $\frac{c_f}{y}$ and y_e.

An interesting comparison may be noted between the coefficients of y_{wc} and y_{nit}. Since the objective of research on the experiment is to obtain estimates of the effects of a permanent NIT plan, the availability of a comparison with income from on-going income-maintenance programs--like unemployment compensation and welfare, which presumably are considered relatively permanent, may indicate the extent to which the NIT payments are received with the same time horizon in mind. As shown in Table 3, the income from NIT has a larger positive effect on food expenditures than other work-conditioned income in every comparison. Recall that if the NIT payments were viewed by the recipients as a pure windfall gain, the PIH would predict that nearly all of it would be saved in one form or another and, that, therefore, the effect on a pure consumption item like food would be very small. The regression results indicate that the NIT payments are viewed as at least as normal or permanent a source of income as are the other work-conditioned payments.

Two reservations should be made about this interpretation, however. One is that y_{wc} may be measured with more error than y_{nit}, since the former is based on interview recall and the latter stems from official records. If y_{we} is measured with more error, its coefficients will be biased toward zero. The second is that participants in a permanent NIT plan may view NIT payments as more of a permanent source of income than they would view the other types of work-conditioned payments. If this were true, the equality of the effects of y_{wc} and y_{nit} would be evidence that the experimental effects of NIT are biased downward, as the first section of this paper suggests.

Although many comments could be made about the regression results reported in Table 3, a full analysis of expenditure behavior of the experimental families is not the objective of this paper. We conclude the discussion of Table 3 by briefly noting three additional findings. First, the small and insignificant effect of D, the dummy variable designating being in the control group, is evidence against any peculiar or special expenditure behavior on the part of the treatment families. Second, the general similarity between coefficients of the variables in regressions using all families and those using only treatment families is evidence against any strong interactions between the variables used and the treatment status. Finally, the values of the coefficients of y_{nit} are sufficiently similar to those of y_e and y_{we} to constitute evidence against the notion that NIT payments constitute "funny money" and that the experiment is an artificial experience.

Clothing Expenditure Regression Results

The regression results for clothing expenditures are both less interesting and less satisfactory than for food. First, they are of less interest because clothing expenditures represent a much smaller fraction of household income; thus sampling variability is probably relatively large. Second, the question about clothing expenditures refers to "the last six months," so recall error is a more serious defect. Finally, clothing represents a more durable good than food and is less likely to permit as sharp a test about consumption decisions versus saving decisions.

The regression results for clothing are less satisfactory because of the lower overall explanatory power of the fitted relation and the insignificant results of many of the income variables in several regressions. No income variable is significant in regression I for all families, with the dollar amount of expenditures as the dependent variable. In regression III, which is restricted to treatment families, income from earnings has a significant positive effect on the dollar amount of clothing expenditures, but the coefficients of both work-conditioned transfer payments and NIT payments are insignificant and small in magnitude.

To the extent that clothing represents a form of consumption expenditure, the larger income coefficient and elasticity of the earnings variable compared with the elasticities of NIT payments in equation III support the proposition that NIT payments are more a transitory component of income than earnings.

Table 4

Regression Results with Clothing Expenditures as Dependent Variable, Models I-IV

Independent Variables	All Families: I. Dollars of Annual Clothing Expenditures as Dependent Variable	All Families: II. Ratio: Clothing Expenditures/ Income as Dependent Variable	Treatment Families Only: III. Dollars of Annual Clothing Expenditures as Dependent Variable	Treatment Families Only: IV. Ratio: Clothing Expenditures/ Income as Dependent Variable
1. Earnings, y_e	.0083 (1.35)	-.0000099 (8.75)*	.017 (2.05)*	-.0000084 (5.83)*
Total income elasticity	.023		.048	
2. Nonwork-conditioned income, y_{nwc}	.0483	-.0000020	.135	.0000105
Total income elasticity	.132		.380	
3. Work-conditioned income, y_{wc}	.0113 (.90)	-.0000106 (4.54)*	.011 (.69)	-.0000107 (3.75)*
Total income elasticity	.031		.031	
4. NIT payments, y_{nit}	.0118 (.56)	-.0000093 (2.41)*	.014 (.64)	-.0000082 (2.08)*
Total income elasticity	.032		.039	

Table 4 (cont.)

Independent Variables	All Families: I. Dollars of Annual Clothing Expenditures as Dependent Variable	II. Ratio: Clothing Expenditures/ Income as Dependent Variable	Treatment Families Only: III. Dollars of Annual Clothing Expenditures as Dependent Variable	IV. Ratio: Clothing Expenditures/ Income as Dependent Variable
5. T = in control group	-18.7 (.69)	-.00064 (.13)	----	----
6. Family size	-.81 (.04)	.0047 (1.15)	-44.8 (1.53)	-.0049 (.96)
7. Family size squared	3.27 (2.02)*	.0001 (.49)	6.26 (3.00)*	.0008 (2.14)*
8. Black	133. (4.33)*	.021 (3.71)*	133. (3.00)*	.018 (2.14)*
9. White	25. (.58)	.006 (.78)	-15. (.28)	-.003 (.34)
10. Trenton	-157. (2.90)*	-.021 (2.13)*	-137. (2.08)*	-.018 (1.60)
11. Paterson-Passaic	169. (3.81)*	.032 (3.92)*	174. (3.11)*	.030 (3.05)*

Table 4 (cont.)

Independent Variables	All Families: I. Dollars of Annual Clothing Expenditures as Dependent Variable	All Families: II. Ratio: Clothing Expenditures/ Income as Dependent Variable	Treatment Families Only: III. Dollars of Annual Clothing Expenditures as Dependent Variable	Treatment Families Only: IV. Ratio: Clothing Expenditures/ Income as Dependent Variable
12. Jersey City	232. (5.28)*	.041 (5.10)*	225. (3.91)*	.035 (3.50)*
13. Constant	95.1 (1.08)	.072 (4.40)*	201. (1.79)	.10 (5.18)*
R^2	.27	.23	.30	.23
Standard deviation of residuals	299.	.056	307.	.05
n	762	762	461	461

Notes: Numbers in parentheses are regression coefficients and t-ratios. Asterisk (*) indicates that the coefficient is significant at the 5 percent level (t = 1.96).

However, a "no difference" verdict appears appropriate in comparing the y_e and y_{nit} effects in equations I, II, and IV. The results do not appear to be reliable and are not worth extensive discussion at this time. Let us simply note: the insignificant effect of the dummy variable for the control group in the all-families regression, and the similarity of income coefficients for work-conditioned transfer payments and NIT payments. As with the food regressions, an optimistic inference from these results would be that the experimental payments are not "distorting" consumer behavior, and that NIT payments have effects similar to those of other types of transfer payments which are "normally" (and often permanently) available to urban poor families.

Regression Results for Durable Goods and Debts

Durable goods and debts are items in household accounts which provide an important outlet for savings. Consumers, in general, and poor households, in particular, face a substantial differential between the interest rate they can earn on liquid assets and the rate they must pay on their debts, so debt repayment is an efficient method of saving. Also, the "use" value of durable goods, reinforced perhaps by the motive of emulative consumption (a striving for the standard of living expressed in the advertising media) makes durables an attractive form of saving, even though some debt acquisition is often entailed. Certainly, the high costs of information and the diseconomies of small scale transactions preclude stocks, bonds, real estate, and the like from being popular savings outlets for poor people.

As an empirical matter, the amounts spent on durable goods and on changing one's debt position are substantially larger and more variable than are savings in the form of liquid assets among the families in the experiment.

The analysis of the assets and debts of the households in GWIE has only recently begun.[16] The initial impression, however, is similar in one important respect to the previous analysis of disaggregated data: there is no evidence to indicate that NIT payments are used to a greater extent for "saving" than other forms of income. In this respect, the analysis provides no evidence that NIT payments are viewed as being more transitory than other sources of income.

Table 5 shows two regression equations which estimate relationships between durables and debts (as dependent variables) and NIT payments, other income sources, and a number of other independent variables. The dependent variable in regression A is the change in the value of durable stocks from the first to the sixth quarterly. The dependent variable in regression B is the change in the debts outstanding from the first to the sixth quarterly. Both regressions show a similar result for the NIT income variable--a positive and insignificant coefficient, which is slightly smaller than the coefficient of the earnings variable, and about the same size as the coefficient for the variable measuring work-conditioned income. The smaller coefficient in the durables equation is contrary to what is expected on the basis of our application of the PIH, but the fact that debts are increased more in response to earnings than in response to NIT payments is in accordance with the PIH. As noted before, however, there is no visible evidence that NIT payments are used to reduce debts, and the effect of such payments on durables is so weak that this form of savings does not explain the positive effect on debts.

There are no remarkable results among the remaining list of explanatory variables. In both regressions the effect of the amount of durable stocks or of debts at the initial period is negative, reflecting the tendency for any disequilibrium high (or low) level to be brought to its "normal" state (like

Table 5

Regression Results with Durable Goods Expenditures and Changes in Debts, First Quarter to Sixth Quarter, as Dependent Variable (All Families)

	Independent Variable (Mean values in parentheses)[c]		Regression A: Change in Durable Goods Stocks, Q1 to Q6 as Dependent Variable[a]		Regression B: Change in Debt Amounts, Q1 to Q6 as Dependent Variable[b]	
1A.	First quarter stock of durables	($1120)	-.28	(7.01)*	---	
1B.	First quarter amount of debt	($1163)	---		-.50	(6.72)*
2.	Earnings, y_e	($5164)	.067	(4.13)*	.144	(2.58)*
3.	Non-work-conditioned income, y_{nwc}	($48)	-.071	(.52)	1.51	(3.16)*
4.	Work-conditioned income, y_{wc}	($404)	.050	(1.29)	.169	(1.26)
5.	NIT payments	($468)	.046	(.89)	.122	(.68)
6.	Home owner	(.18)	21.9	(.22)	202.7	(.55)
7.	Family size	(6.02)	55.0	(.90)	-14.4	(.07)
8.	Family size squared	(...)	-3.04	(.72)	11.1	(.76)
9.	Black	(.33)	163.3	(1.64)	256.8	(.74)
10.	White	(.46)	226.1	(1.67)	602.2	(1.28)
11.	Age of head, less than 35	(.47)	241.0	(3.15)*	377.8	(1.43)
12.	Age of head, over 50	(.11)	-10.8	(.09)	-6.29	(.02)

Table 5 (cont.)

Independent Variable (Mean values in parentheses)			Regression A: Change in Durable Goods Stocks, Q1 to Q6 as Dependent Variable[a]		Regression B: Change in Debt Amounts, Q1 to Q6 as Dependent Variable[b]	
13.	Trenton	(.08)	-208.7	(1.22)	-363.9	(.61)
14.	Paterson	(.26)	-67.6	(.48)	185.4	(.38)
15.	Jersey City	(.29)	-441.4	(3.23)*	351.3	(.74)
16.	Constant		-203.1	(.76)	-846.5	(.92)
R^2			.102		.097	
n	671		---		---	

Notes: Numbers in parentheses (except 1st column) are regression coefficients and t-ratios.

[a]The change in value of the stock of durable goods from the first quarter (Q1) to the sixth quarter (Q6) is the measure of "net expenditures" on durables used on the dependent variable. The mean value of the dependent variable (for all families--treatment groups and control groups) is $189.

[b]The change in amounts of debt from the first quarter (Q1) to the sixth quarter (Q6) is the measure of debt acquisition (or debt repayments) used as the dependent variable. The variable was obtained as a sum of the reported debts on a list of items, including home mortgages. The mean value of the dependent variable is $612. It should be noted that this source of debts information differs from the source for debt figures in Table 2, which was the answer to the single question: "How much do you owe altogether? This question was the only source of debt information for the quarterly interview administered at the time of preenrollment.

[c]Mean values apply to the time of the sixth quarterly interview except for the values of the lagged dependent variable, which refer to the first quarter.

a regression toward the mean) by smaller (or larger) flow amounts in the intervening periods. Earnings has a significant positive coefficient in both regressions. For each $100 increase in earnings, durable stocks increase by $7 and debts increase by $14. The effect of being in the age group in which the head is under 35 years has a significant positive effect on the acquisition of durables, which is consistent with life cycle theories. The effect of the younger age on debt levels is positive but not statistically significant at conventional levels. Two other variables had significant effects. A large effect of nonwork-conditioned income on debts was measured in regression B, and the effect of being a Jersey City resident was negatively related to the change in durables in regression A. We have no explanation for these results.

One finding which emerged from the analysis of durable goods expenditures leads to a digression on alternative theories to the PIH of consumer behavior. It turns out that the treatment families spent less than control families on the "other appliances" category, which includes (primarily) television sets, radios, air conditioners, and motor cycles. We see no tendency, therefore, for the NIT recipients to "blow" their windfall gains on ostentatious purchases. The reader may recall the suggestion by Houthakker that the gainer of a windfall would splurge his gain in transitory income on an expensive restaurant meal, rather than save it all as Friedman hypothesized.[17] Of course, the more serious intent of Houthakker's example was to emphasize a consumption motive in the household's use of transitory income. Nonetheless, his example leaves ambiguous the question of whether normal or customary consumption expenditures as distinct from unusual consumption expenditures are increased by the receipt of windfall gains. If it is the latter, there is a question of just how such expenditures should be categorized. Perhaps they are more akin to investments--purchases serving to increase one's utility (in the form of happy memories) in the years to come.

Another competing hypothesis is the relative income hypothesis, which suggests that people base their consumption standards on those of some reference group. The reference group is not always well defined. If it is the household itself at some previous time period, then the hypothesis becomes hard to distinguish from the PIH, wherein the previous time period marks a point of "normal" or permanent income. In any case this hypothesis is not useful to us unless we can determine whether the recipients of NIT payments consider their improved economic status a short-run or a long-run relative improvement--which is the question we are trying to determine.

Concluding Remarks About the Empirical Work

The empirical work presented in the preceding sections is intended only as a trial run of methods to be used when the data on expenditures and saving become available for the second and third years of the experiment. The models and hypotheses developed in this and the previous sections unquestionably make stringent demands for extensive and accurately measured data. The data used above are not adequate in this regard, nor were they expected to be. It was worthwhile nevertheless, to undertake a preliminary analysis to discover the strengths and weaknesses in the data and techniques.

One lesson to be learned is that a great deal of reliance must be placed on obtaining detailed information regarding income, saving, and certain components of consumption. An exhaustive listing of expenditures is almost impossible to obtain, so total consumption is best measured as income minus saving--where the latter includes the saving (or investment) component of durable-goods purchases. Hopefully, information about major durables (like horses, automobiles, and large appliance and furniture items) can be measured reliably and will suffice. Measuring income accurately is, of course, fundamental to the objective of the experiment, and much effort is devoted to this task.

The technique of using food purchases to represent "pure consumption" remains attractive, but it will be necessary to measure food for a period when the families have had time to make some adjustment to the receipt of NIT payments. The quarterly interviews that take place in the second or third years should meet this requirement. We will also want to match the family composition for the period of food consumption with the composition pertaining to the period when income is measured.

A critical but troublesome explanatory variable in the model measuring expenditure functions is permanent income. It should be clear from the empirical work in this section that identifying sources of steady income is difficult for low income families. One strategy to be pursued will be the development of instrumental variables to represent permanent or normal income. For example, assume that education is correlated with permanent income and uncorrelated with transitory income. Assume further that the effect of education in food expenditures, holding permanent income constant, is zero. Then a stratification of the sample by the head's educational attainment permits an identification of the effect on consumption (or savings) of the permanent component of income. This would be compared with the effect on consumption of the NIT component of income. Another device for getting at permanent income would be to restrict the regression analysis to a subset of families which have a relatively stable pattern of employment, and earnings. This would also offer an opportunity to measure "normal" NIT payments among treatment families.

V. COMPLICATIONS IN THE ANALYSIS

The data problems discussed above are part of the standard range of problems associated with the specification and estimation of economic behavioral relationships. These econometric problems must be effectively handled before the theoretical analysis at the beginning of this paper can be applied. The theoretical analysis given in the first two sections of this paper deal with the possible biases in a short-duration experiment within the context of a nonstochastic model. This section marks a return to that context in order to examine the sensitivity of the analysis to several of the model's underlying assumptions.

The first two sections of this paper exploit a number of assumptions, which, although common to much of economic analysis, would, if violated, substantially complicate and modify the results. Since a thorough assessment of the impact of each of these complications is a major undertaking in itself, only an outline of the difficulties is provided here.

The Assumption of Rational Behavior

Economists use the term "rational" to mean, in general, adherence to a systematic decision rule in adapting to economic constraints. When discussing consumer choice, they assume that the decision rule is designed to bring an individual to the highest possible level of "utility" or satisfaction, given these constraints on his behavior. The particular form of the decision rule depends on the context in which it is used. When labor-supply responses to a NIT are analyzed, for example, the assumption of rationality implies that participants perceive a "substitution effect" which tends to cause a shift from (market) work to non-(market) work activities in response to the higher implicit tax rate on earnings.

In the analysis of biases in the experiment the assumption of rational behavior carries several implications. One is that the families recognize the three-year duration of the experiment and perceive the lower implicit price on leisure "now" as opposed to "later." Another is that they recognize that the income gains are temporary and, consequently, that the maximum potential increase in their standard of living will be temporary, other things equal. This assumption becomes relevant as a rationalization for the distinction made between behavioral responses to permanent and transitory income changes, as predicted by the permanent income hypothesis. In summary, "rationality" implies a qualitative set of reactions to the (assumed) known temporary duration of the income changes and price changes of the experiment; such qualitative reactions are basically those implied by conventional models used in economic research.

A rather different implication of the assumption of rationality is, to put it negatively, that the participants do not view the experiment as some sort of "game"--rather, that they perceive it as a normal governmental-sponsored income-maintenance program, except for its small scale and short duration. Another way of expressing this point is to say that the behavior of the individuals is not significantly or seriously distorted just because they are part of an experiment (as distinct from the experimental treatments per se). Such "Hawthorne effects" are assumed not to exist in this analysis. Indeed, if such effects were present, one would have to know their precise form to infer information about behavior in nonexperimental settings, and this information is not available, even in a qualitative sense.

The Assumption of "Perfect" Labor Markets

For our purposes the substantive content of the assumption of perfect labor markets is two-fold: (1) that in nonexperimental settings the labor markets facing low-income workers permit some substantial amount of choice in time spent at work over a period of one to three years; and (2) that this same degree of flexibility (assuming there is some) applies during the experiment.[18] The contrary view to the first proposition is that most workers are constrained to work either some fixed amount of time per week (and per year) or not to work at all--or, alternatively, that any changes which the worker may desire in the "standard work year" will be prohibitively costly.

An argument against the second proposition is that the experiment is too short to justify the costs of modifying work choices, even if workers were willing to make them in response to a permanent income-maintenance plan. We shall discuss these issues separately for primary workers and secondary workers--defining the former as male heads of the household in the prime working ages and the latter as all others (teenagers, older workers, wives, and other adult females in the household).

Primary workers. If primary workers were likely to respond to a permanent income-maintenance program by quitting work altogether, it would indeed be unlikely that a short-duration experiment would induce this same response. In an experiment, the costs of returning to full-time work after interrupting one's seniority and career committment would surely be too large relative to the gains in utility from the consumption of more leisure over the three years. However, one may also question the likelihood of a complete labor-force withdrawal by prime-age male heads in response to a permanent plan--particularly at the levels of generosity likely to be enacted in the near future. If so drastic a reduction in labor supply is not likely, then the question of an experimental bias from this source is irrelevant.

The argument that the experiment will fail to detect a more piecemeal reduction in work effort by primary workers is, perhaps, more important because of the likelihood that the response to a permanent plan would be characterized by a gradual reduction over time. The decline that has been taking place over the last 100 years in such measures of the labor supply as the work day, the numbers of days worked per week, the number of holidays, vacations, etc., all suggest a downward flexibility in work time by primary workers over the long run. There are two reasons for expecting less flexibility in the experimental situation: (1) the costs to the workers of adjusting temporarily, as has been mentioned, and (2) the costs to employers of adjusting their demand for a small number of workers as compared to a situation in which the whole market (or nation) is covered by an income-maintenance plan.

The amount and kind of flexibility in work choices that primary workers actually exercise during a short-duration experiment remains to be determined. It does not seem likely that deliberate selection among part-time jobs of varying work lengths is a realistic option for such groups. But one can postulate feasible mechanisms of achieving flexibility--changes in the amount of time spent on second jobs or working overtime, variations in days absent for miscellaneous reasons; and, what may be an important mechanism for low-income workers, differing amounts of time spent between jobs as a result of voluntary and involuntary job turnover.

In any case, it is clear that work schedules will be less flexible for a short-term experiment than a permanent program. And for this reason the expected upward bias in the "price effect" of the tax on earnings for the treatment workers will be less than that predicted by the analysis above. (The predicted bias toward zero in the income effect will be accentuated.)

Secondary Workers. Labor-force participation is more intermittent for secondary workers, and the types of jobs offered them provide a much more varied range of hours per week and/or weeks per year. These facts suggest considerable flexibility in labor-supply choices, and the economic model of labor supply adopted in this paper can be expected to apply without much strain. The extent to which the short duration of the experiment constrains these choices is, again, an empirical question, but there is a good deal of evidence pointing to the responsiveness of labor-supply decisions of secondary workers to short-run economic fluctuations. There is less reason to expect, therefore, that the upward experimental bias in work responses of secondary workers will be much reduced by institutional constraints on their work choices.

The experimental bias in the income effect unfortunately does not have as neat an interpretation as that of the substitution effect. Recall that the income effect (presumed to be negative) on labor supply is expected to be biased toward zero in a short-duration experiment, where the income transfer payments tend to be viewed as transitory income gains. However, previous research (discussed below) has indicated a strong negative relationship between transitory income and the labor supply of secondary workers. In the following paragraphs we suggest that this exaggerated relationship is due to implicit transitory price changes which accompany the income changes referred to in the analysis.

The hypothesis that secondary workers adjust their time spent at work to transitory income changes experienced by their family units has its origins in the "additional worker hypothesis" of the 1930s.[19] At that time attention was focused on the entry into the labor force of secondary workers in families experiencing income losses because of the widespread unemployment of primary workers. In the modern version of this hypothesis as formulated by Jacob

Mincer, the effect of transitory income changes (up or down) on the labor supply of secondary workers, particularly wives, is believed to be stronger than that of equal amounts of change in permanent income.[20] The rationale is that labor-supply changes are an alternative to increases or decreases in savings, especially among poor families who are unlikely to have either large asset holdings or easy access to capital markets.

Translated into terms consistent with the theoretical analysis of this paper, in which exaggerated labor-supply responses are produced by transitory income changes (relative to permanent changes, of course), the basis for the "additional worker hypothesis" is as follows:

(1) First, the price of borrowing is sufficiently high that when the family suffers a temporary decline in income the entry of secondary workers to the labor force is the more expedient way of maintaining the family's consumption standards. Conversely, the returns from saving are sufficiently low that when the family income is temporarily high the departure of the secondary worker is preferred.

(2) Second, any shortfall in income brought about by unemployment of the male head will produce a surplus of nonmarket work time for the household, driving down the marginal value of all other family members' nonmarket work activities relative to the value of their market work activities. Conversely, any windfall gains in income stemming from extra jobs or overtime of the male head will produce a shortfall of nonmarket work time for the household, and this will tend to increase the marginal value of all other family members' nonmarket work activities relative to the value of their market work activities.[21]

Transitory income changes that result from NIT payments will not necessarily carry the same price effects regarding capital markets and nonmarket work activities as do transitory income changes over the business cycle. Nevertheless, in many instances the NIT payments will be high (or low), depending on the amount of unemployment (or "overtime") or on other sources of income loss (or gain). As a consequence, the expected bias toward zero in the income effect of transitory NIT payments will be pulled in the opposite direction by the hidden (or implicit) price effects set in motion by the circumstances surrounding a transitory income change.

The Assumption of "Perfect" Financial Markets

The permanent income hypothesis is based on the assumption that households can stabilize their consumption in the face of fluctuations in income by borrowing and saving in "perfect" financial markets. (We now disregard the adjustments to income changes by means of secondary workers in the family.) Imperfections in capital markets, whether in the form of high discriminatory interest rates, ignorance by borrowers of institutions in the financial market, or whatever, would interfere with the ability of households to stabilize their consumption as predicted by the PIH.

While low-income households certainly do not have the same access to financial markets as do higher income households or business enterprises, the implications for the PIH of this differential access should not be overemphasized. In the experimental income-maintenance program the nature of the treatments is, let us recall, to provide only positive transitory income changes. All that is required for the qualitative predictions of the PIH to hold is that the experimental households save during the experiment and dissave subsequent to the experiment. Since the financial restrictions placed on such households are probably more stringent for borrowing than saving, they would be more able to follow the saving-dissaving pattern predicted for the experiment than an alternative pattern of borrowing-repayment.

Furthermore, we have found that many experimental households have been increasing their financial debts, presumably with accumulated durable assets as collateral--indicating that low-income households do have substantial access to financial markets, although at high interest rates.

The Assumption of Equilibrium Behavior

The above analysis is based on the assumption that households are in "equilibrium" at the time the experiment began and that they adjust quickly enough to permit a measure of response in a post-treatment equilibrium status. Violations of both parts of this assumption create their own difficulties.

If not all households are in equilibrium, and if there is a tendency to move toward equilibrium, certain behavioral responses may be inappropriately attributed to the experiment. The incorrect attribution will occur if deviations from equilibrium are systematically related to variations in experimental treatments. Consider the case where the expected deviation of a household's income from its "normal" income is positively correlated with the deviation of observed income from the sample mean. In this case there would be a tendency for the household's income to "regress toward the mean" independently of experimental treatments.

But, since the size of transfer payments is related to the income level there will be a spurious correlation between the degree of expected adjustment to initial disequilibrium and the size of the transfer payment.[22] To the extent that the degree of disequilibrium can be successfully approximated by a deviation-from-the-mean measure, econometric methods exist for correcting for such "regression toward the mean." For equations predicting the current level of earnings or hours worked of experimental households, the normal correction procedure calls for inclusion of the lagged value of the dependent variable on the righthand side of the equation.[23]

Even if all households are in equilibrium at the beginning of the experiment, they may adjust to a new situation with a time lag--it may take time to perceive their "optimal" behavioral strategy, to make labor-market decisions, and to make the necessary alterations in their behavior to act upon the strategy. The implications of such lags are similar to the effects of costs associated with the institutional constraints in the labor market which interfere with costless entries to and exits from the labor force, as discussed above. Again, the rationale underlying these arguments is that whenever costs are associated with adjustment, households will be less likely to adapt to a temporary change in conditions than to a permanent change. If the experiment is sufficiently short compared to the length of the lag, the size of the observed response will be biased toward zero compared to the effect of a permanent plan, after abstracting from the biases already discussed.

The Assumption of Certainty or Perfect Knowledge

The analysis in sections I and II of this paper makes a formal comparison between two situations: (1) an experimental situation where the individual knows with certainty the duration of the NIT, and (2) an alternative situation where the individual knows with certainty that the NIT is permanent. Similarly, it is assumed that any uncertainty the household has about all other information relevant to the future does not affect its behavior--such relevant information includes income streams, wage rates, and other price variables.

Uncertainty could be introduced into the analysis in a number of ways. Households might, for instance, be uncertain about the duration of the experiment. Similarly, households might infer from the existence of experimentation that there was some likelihood of the program being permanently adopted by the end of the three years.

The effects of such uncertainty can be decomposed into two parts--effects due to an increase in the expected length of experiment, and effects due to the fact that there is a distribution of possible outcomes around the expected outcome. The former make the experimental responses more like the permanent responses which we would like to measure, but the latter can have a confounding impact on a proper interpretation of the experiment. To the extent that households have a precautionary demand for saving, the introduction of uncertainty about the level of future income (given an expected level of future income) will reduce the level of current consumption. Thus, the presence of such uncertainty may lower the consumption of leisure time during the experiment, compared to a NIT for which the provisions are known with certainty. The effects of such uncertainty working through expectations about the wage rate are rather complicated, and will not be investigated here.

Uncertainty about future nontransfer income streams, as well as variability of income without regard to uncertainty, can also affect the analysis. The presence of a positive tax rate on earned income lowers the variability of after-tax income. Even households above the breakeven point will be affected by a NIT so long as there is some positive probability of falling below the breakeven point at some future date. To the extent that households tend to be risk averters, the NIT mitigates this tendency and permits more risky behavior to be attempted. For instance, we would expect households to reduce their level of saving for precautionary purposes. They might also be more willing to change jobs in search of occupational advancement, to undergo training, to migrate, and so on.

It should also be pointed out that the above analysis is based on the assumption that incomes remain below the breakeven point. When incomes fluctuate above and below the breakeven point, the analysis becomes much more complicated. In specifying empirical relationships, these complications must be recognized and accounted for.

VI. EPILOGUE

In this paper we have made a preliminary attempt at breaking new ground in identifying sources of bias in using short-duration experiments to predict long-run behavior. Future research efforts must be concentrated in two major areas. First, the importance of the complicating factors identified in section V must be assessed; where appropriate, they should be integrated formally into the analysis. Second, as indicated in the text of the paper, empirical implementation of the analysis poses major difficulties. Work in both areas in continuing.

Footnotes

[1]The derivation of this expression and equations (2) through (9) of this paper are provided in Charles E. Metcalf, "Making Inferences from Controlled Income-Maintenance Experiments" (Madison, Wis.: Institute for Research on Poverty, 1971). Discussion Paper No. 103-71.

[2]Milton Friedman appears to use high discount rates and short time horizons interchangeably. In "Windfalls, the 'Horizon,' and Related Concepts in the Permanent-Income Hypothesis" in Christ et al., Measurement in Economics, Studies in Mathematical Economics & Econometrics in Memory of Yehuda Grunfeld (Stanford University Press, 1963), Friedman defines a discount rate, r, which corresponds to the proportion of wealth consumed in any one year, and a time horizon, N, which is the inverse of the discount rate. Thus he used r=10 percent and N=10 interchangeably. One interpretation of this correspondence is that the behavioral effects of a 10 percent discount rate with an infinite time horizon are the same as the effects of a ten-year horizon with a zero discount rate. In this paper we distinguish between these two notions, and the time (1/1+R) corresponds to the proportion of wealth consumed in any one year. For any finite time horizon and positive interest rate, (1/1+R) will exceed both the discount rate and the inverse of the time horizon. A consumption rate in excess of the discount rate is due to the fact that we allow the household to exhaust its wealth over its lifetime. Similarly, a household with a ten-year lifetime can consume more than 10 percent of its present value in a given year due to the presence of the positive discount rate.

[3]The minus signs associated with equation (2) and subsequent expressions for price effects reflect the fact that, ceteris paribus, the NIT lowers the wage rate facing households.

[4]This statement is true only if individuals expect their real wage rate and the generosity of the NIT plan (after correcting for inflation) to be constant over time. An anticipated growth of real wage rates complicates the analysis; this complication will be treated in a later paper.

[5]Two period illustrations are used for equations (4) and (5); the appropriate sum includes terms for every time period in the general multiperiod case. Again, the asterisk (*) attached to each price effect denotes a substitution effect after compensation for changes in real income due to the price change.

[6]For example, if an individual consumes 25 percent of an increase in income in the form of additional leisure time, 25 percent of an increase in current consumption due to intertemporal substitution effects will also take the form of additional leisure time.

[7]The following set of conditions is sufficient for the approximation to hold exactly: additive preferences within, as well as across, time periods; identical preferences across time periods; and a subjective rate of discount such that the household opts for an intertemporally uniform consumption stream at the given market rate of interest. For a formal exposition of these conditions, see Metcalf, "Making Inferences from Controlled Income-Maintenance Experiments."

[8]Milton Friedman, A Theory of the Consumption Function (Princeton, N.J.: Princeton University Press, 1957).

[9]The assumption that NIT payments would be viewed as a form of permanent income under a permanent NIT plan is itself debatable, and will be relaxed below. It is a reasonable assumption if, under a permanent plan, the families receiving payments believe that their incomes will remain about the same for a future period that matches their time horizon, or if they "count" upon such permanent NIT payments as "always" available to shore up their income when they fall under the breakeven level, even though their actual income sometimes or often exceeds the breakeven level.

[10]A hypothetical example may clarify the relationship among numerical values of the different α-coefficients. Assume that the permanent component of income accounts for 3/4 of the sample variance in current income and that the underlying values for α_1, α_2, and α^* are .9, .1, and .3 respectively. Then the value of $\left(\frac{1}{(1+R)}\right)$ associated with experimental income is 1/3 $(=\frac{\alpha^*}{\alpha_1})$ and our measured value for $\frac{\alpha^*}{\alpha_c} = \frac{.3}{(.75)(.9) + (.25)(.1)} = \frac{.3}{.7} \sim .43 > \left(\frac{1}{1+R}\right)$. Given the dependence of R upon r and N, we see that: for a given interest rate, the time horizon associated with GWIE payments would be underestimated; and for a given time horizon, the implied interest rate would be overestimated.

[11]An illustration of this possibility can be derived from the specification

$$c = a_0 + a_1 y_p + a_2 y_t + a_3[\overline{G} - T(y_p + y_t)]$$

in which $\overline{G}$ is held constant so that all variation in NIT is caused by $T(y_p + y_t)$. Again defining $y_c = y_p + y_t$, we can transform the specification into the following share expression:

$$\left(c/y_c\right) = \frac{a_0 + a_3\overline{G}}{y_c} + a_1\left(\frac{y_p}{y_c}\right) + a_2\left(\frac{y_t}{y_c}\right) - a_3 T$$

If, as assumed in the "extreme form" of Friedman's PIH, we assume $a_1 = 1$ and $a_2 = 0$, we have:

$$(c/y_c) = \frac{a_0+a_3\overline{G}}{y_c} + \frac{y_p}{y_c} - a_3T \qquad \text{(a)}$$

The model to be estimated in share form, corresponding to (19) in the text, is

$$(c/y_c) = \frac{b_0+b_3\overline{G}}{y_c} + b_1 - b_3T \qquad \text{(b)}$$

Except for a constant, b_1, equation (b) differs from equation (a) in terms of an omitted variable, y_p/y_c. The relationship between b_3 and a_3 can then be determined by the formula:

$$b_3 = a_3 + da_1 = a_3 + d,$$

where d is found in the auxiliary regression:

$$y_p/y_c = \alpha_0 + dT + \varepsilon$$

Unless the disincentive effects of T on income influence y_p and y_c proportionally, $d \neq 0$ and hence $b_3 \neq a_3$.

[12]The ratio of food expenditures to income is defined as follows: The denominator is annual income reported in 1969; the numerator is 52 times the amount of money spent on food prepared at home "last week" (asked during a quarterly interview administered in 1969). The ratios, .39 and .36 appear abnormally high. The national average percent of income spent on food for urban families with similar incomes was only around 21 percent based on the 1961 Survey of Consumer Expenditures. In this survey the ratio of food expenditures to income, using definitions similar to those in the GWIE survey, was about .26 for households with incomes less than $3000 per year; .21 for the $3000 to $4999 income bracket; and .17 for the $5000 to $7499 income group. The weighted average of these expenditure ratios is .21. (See Expenditure Patterns of American Families, National Industrial Conference Board, New York, N.Y., 1965). One reason for the larger percent spent on food by experimental families is that their average family size is about 6, larger than for the national sample of urban low-income families which is just under 4. However, even comparing the national sample of low-income families with 4-person families

in the experiment, the discrepancy is still large. Why the percent is so much higher in the GWIE survey is a puzzle. Perhaps the reference period actually used by the experimental families was longer than one week. In any case, for purposes of illustrating some methodological points in this paper, we will assume that the differences in percentages as between treatment and control groups are accurate, even though the levels of these percentages appear to be inaccurate.

[13] Ibid. The average percent of income spent on clothing for all urban families earning less than $5000 in the nationwide Survey of Consumer Expenditures in 1961 was a little over 8 percent.

[14] Durable expenditures are measured by subtracting the sixth quarterly stock value from the first quarter stock value, and therefore span five quarters. (No data on durables was collected in the preenrollment interview.) These values had to be estimated by the analyst on the basis of the original cost, age, make, etc. of the item, and undoubtedly are poorly measured at the initial point in time, although the change in values should be measured with acceptable accuracy. This source of "expenditures" is generally preferred to just asking about purchases over the previous period, because the latter is believed to be inaccurate because of memory loss and vagueness regarding the date of purchase. Questions about stocks can be answered by a simple reference to ownership. In addition, the stock information can be adjusted for depreciation or sales to enable one to calculate net expenditures. (See footnote c in Table 2 for further information about the measure of durable goods in this analysis.)

[15] The income elasticity of food expenditures is defined as the percent change in food expenditures with respect to (that is, divided by) the percent change in income. A percentage change in any income component represents of course, a smaller fraction of the percentage change in total income. To obtain the elasticity of total income, the reciprocal of the component as a fraction of the total is multiplied by the elasticity of the component. The component elasticity is simply the regression coefficient times the ratio of the mean of the income-component to mean expenditures.

[16] We are indebted to Walter Nicholson for supplying us with some of his preliminary analysis of these topics.

[17] H. S. Houthakker, "The Permanent Income Hypothesis: A Review Article," American Economic Review (June 1958):396-404.

[18] We will assume that all measures of labor-supply responsiveness to the income-maintenance plans are quantitative units of time spent at work, such as hours per year. Actually, another and highly relevant form of supply could be measured in terms of work effort; one such measure is, of course, earnings, and this would offer added flexibility in work choices.

For a discussion of this hypothesis in the context of the depression and a review of the literature up to the mid-1950s, see Clarence Long, The Labor Force Under Changing Income and Employment, National Bureau of Economic Research (Princeton, N.J.: Princeton University Press, 1958), pp. 181-201.

[20]See Jacob Mincer, "The Labor Force Participation of Married Women," Aspects of Labor Economics, National Bureau of Economic Research (Princeton, N.J.: Princeton University Press, 1962), pp. 63-67; and "Labor Force Participation and Unemployment: A Review of Recent Evidence," in Prosperity and Umemployment, ed. R. A. Gordon (Berkeley and Los Angeles: University of California Press, 1966), pp. 73-112.

[21]No distinction is drawn between leisure or home work types of nonmarket activities, but the strength of the cross-effects of unemployment of the male head on the labor-force activity of other family members would depend on the complementarity and substitutability among family members regarding leisure and home work activities.

[22]An example may help make the point clear. Assume household A has a transitorily high income in the base period and household B has a transitorily low income in the base period. Then A will be expected to experience a smaller positive (or larger negative) change in income than B during the next period--since both are expected to move toward their "normal" income levels. However, at the time that the base period income is reported, A will receive a smaller NIT payment than B. Thus, the "perverse" correlation between NIT payments and income change will be observed: A has a low NIT payment and a low (or negative) change in income, and B has just the opposite.

[23]If some of the right-hand variables (e.g., when current income is used as an explanatory variable) are also subject to "regression toward the mean," the correction procedure is more complicated but still manageable. The method of correction is closely parallel to procedures for handling errors in variables.