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FINAL REPORT OF THE GARY INCOME MAINTENANCE EXPERIMENT: LABOR SUPPLY

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This paper reports on research conducted by Mathematica Policy Research under contract HEW 100-78-59 from the U.S. Department of Health, Education, and Welfare (HEW). This study is the culmination of several years of research design, data collection, model building, and analysis tasks under three different contracts. Kenneth Kehrer was project director for all of this work.

The work began as part of the conduct of the experiment at Indiana University Northwest, under contract SRS-70-63 between HEW, the Indiana State Department of Public Welfare, and Indiana University. In addition to the authors of this report, Richard Kaluzny, Lois Shaw, and Stanley Stephenson, Jr. were part of a research team that refined the data collection design, grappled with the difficult model specification issues, created analysis files and conducted the initial analysis. The results of this work were presented in the Initial Findings Report on the Gary experiment (Kehrer and Moffitt, 1976).

Further model development, file construction, and analysis took place at Mathematica Policy Research under contract HEW 100-76-73. Preliminary results of this work were presented in a number of papers at the ASSA Meetings in Atlanta City in September, 1976 (Kehrer et al., 1976; Shaw et al., 1976). This paper reports on further research on the labor supply response conducted under HEW contract 100-78-59, and constitutes the final report on the labor supply analysis of the Gary experiment. A condensed version is scheduled to appear in the Journal of Human Resources (Moffitt, 1979).

Several individuals made important contributions to the development of the data. In the early stages of the work, Lois Shaw (now at the Center for Human Resource Research) had major responsibility for the development of the analysis files. The final longitudinal data files were created under the direction of Professor John McDonald at the University of Illinois at Chicago Circle. This work required the care of several other people, including Donna Vandenbrink, LaJean Waller, David Gassman, Belva Wood, Denis Ables, Adolph Hendrickson, Pal Khera, John Friedmann and Carolyn Roth.

The main conceptual development of the labor-supply model was carried out by Professor Robert Moffitt at Rutgers University, who is responsible for most of the estimates presented in this report. In addition, several other individuals made important contributions to the development of the analysis over the years. These include Gary Burtless, David Greenberg, Robinson Hollister, David Horner, Stuart Kerachsky, Charles Mallar, Larry Orr, and Douglas Wolf.

Finally, we would like to thank all those who provided computational assistance for the analysis. First and foremost this includes John Friedmann, who estimated the adjusted-means regressions and developed many of the analysis files. In addition, significant contributions were made by Margo Hoft, Sandra Jamieson, Erno Krakkei, Gayla Olsen, and Debra Tessier.

INTRODUCTION

The Gary Income Maintenance Experiment was one of a coordinated series of experiments supported by the Department of Health, Education, and Welfare and the Office of Economic Opportunity to test the work-incentive effects and other consequences of alternative negative income tax (NIT) plans. The experiments were conducted with different population groups in different parts of the country. The income-support plans tested were similar in structure to those of existing welfare and transfer programs, except that the benefit formulas were simplified and eligibility was more universal, depending only on family income, family size, and the presence of a dependent child.

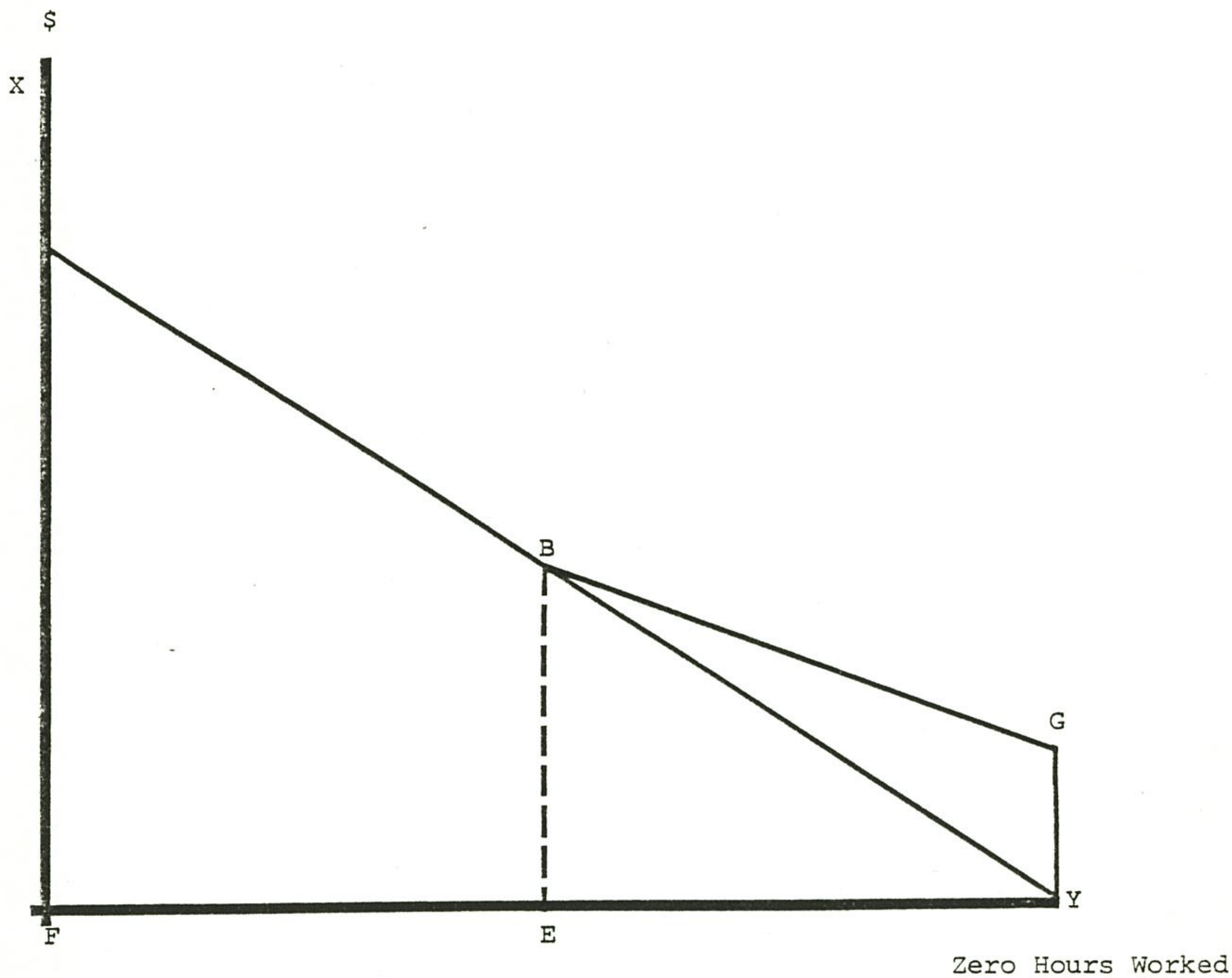
Under an NIT, benefits are determined by a guarantee level (that is, the basic benefit provided to a family with no other source of income) and a tax rate (that is, the rate at which the benefit is reduced as other sources of income increase). Some benefits are paid to all families with incomes below a breakeven level, with the largest benefits going to those families with the lowest incomes. Thus, under such a plan, the size of the benefit decreases as family income rises, but total family income always increases as earnings from work increase.

While an NIT is target effective (the highest benefits go to the poorest families) and has a reasonable incentive structure (total income always rises with increments in hours worked), it nonetheless creates disincentives to work. The potential effect of an NIT on labor supply is shown in Figure 1. Assume an individual facing an effective wage rate FX/FY. An NIT with a guarantee level of YG and a tax rate of t percent

FIGURE 1.

EFFECTS OF A NEGATIVE INCOME TAX

ON LABOR SUPPLY



would shift the individual's budget time to YGBX, where BE is the break-even level and the slope of the budget line below B is the wage rate times one minus the tax rate. The effect of an NIT would thus be to increase the income and reduce the net wage of many individuals. Both of these effects would be expected to reduce labor supply.

Available estimates of the potential labor supply reductions that would result from alternative NIT plans varied so widely in the 1960's that they were of little use in policy planning. One of the problems underlying those estimates was that existing data did not encompass the variation in tax rates and guarantee levels that were being considered in policy planning. Perhaps more important, those estimates were based on nonexperimental data, in which observed labor supply decisions and welfare participation decisions were potentially endogenous. For example, any nonexperimental study of the labor supply of AFDC families possesses a potential selectitivity bias arising from the fact that families go on AFDC in part through a labor supply decision. A study of the labor supply behavior of both AFDC and non-AFDC families cannot easily account for the potential differences in unobserved characteristics that may have led the families to sort themselves out into the two groups. These problems have been discussed more extensively elsewhere (Moffitt and Kehrer, forthcoming).

In order to obtain better estimates of the likely effects on labor supply of alternative NIT plans, the Office of Economic Opportunity and HEW launched a coordinated series of income maintenance experiments that were conducted over the past decade. The income maintenance experiments generally followed a classical experimental design, wherein a "treatment" is given to a randomly selected group of individuals, and their behavior is compared to that of a comparable group of individuals who do not receive the treatment. This design represents an attempt to eliminate the bias caused by unobserved variables, for the randomization that selects individuals for the experimental and control groups presumably results in identical distributions of unobserved variables within each group. Consequently, experimental data give the analyst an independent variable--treatment assignment--whose source of variation is truly exogenous.

In this paper we report on a major study of the labor supply response to the Gary experiment. Section I describes the model specifications we use in the research. After a discussion of the important characteristics of the Gary experiment (Part A), we discuss the nature of the cumulative tax rate problem and derive an average-tax-rate model (Part B). In Part C we examine several other issues of empirical specification of labor supply models, and describe our approach to each of them. We restate our basic model in Part D, incorporating the specifications discussed in Part C.

Our findings are presented in Section II. The results of the basic model are presented in Part A and tests of the sensitivity of the results to alternative specifications decisions are presented in Part B. We summarize our findings and our interpretation of them in Section III.

I. MODEL SPECIFICATION AND METHODOLOGICAL ISSUES

A. THE GARY EXPERIMENT

The Gary income maintenance experiment was conducted between 1971 and 1974 by Indiana University under contracts with the U.S. Department of Health, Education, and Welfare (HEW) and the Indiana State Department of Public Welfare. Here we describe the design and implementation of the experiment, the characteristics of the sample and of the Gary labor market, and the interpretation and generalizability of the labor supply findings from the experiment.

Four different negative income tax plans, combining two tax rates and two guarantee levels, were tested in Gary. The tax rates were either 40 or 60 percent, and the guarantee levels were either equal to the poverty level or about three-fourths of the poverty-level annual income for each family size. In 1972, for example, when the official poverty threshold for a four-person, nonfarm family was $4,275 the two Gary guarantee levels were $4,300 and $3,300 for that family size. Benefit schedules were adjusted every six months to compensate for increases in the cost of living.

The experiment enrolled both "intact" families (i.e., those with a husband present) and female-headed families (i.e., those without a male "head" present). Female heads have an especially important focus on the experiment because neither the New Jersey nor the Rural experiments were designed to study such families. One objective of the Gary experiment was to examine the effects of switching some female-headed families from

AFDC to more generous income-support plans. The lower Gary guarantee level (three-fourths of the poverty level) was about $1,000 a year more than the support level of the Indiana AFDC program. Another objective of the Gary experiment was to investigate the consequences of extending eligibility for income-support payments to intact families. These families were not generally eligible for AFDC in Indiana (Indiana did not participate in the AFDC-UF program).

The Gary experiment enrolled only black families, and eligibility was limited to families with at least one child under age eighteen. Of the 1,799 families who were voluntarily enrolled, 57 percent were randomly assigned eligibility for experimental income-support payments, while the remainder were control subjects. Almost 60 percent of the participating families were female-headed families.[1/]

The experimental group families were eligible for the income-support payments for three years. All participating families, both experimental and control group, filed monthly reports of income and family composition changes. All were interviewed before the experiment, about three times a year during the experiment, and after the experiment.

The families with a male head of household present (almost all of which were intact husband-wife families) usually had low incomes but generally were not extremely poor. The husbands were typically full-time unionized workers with a history of continuous employment, who were able to earn enough to keep their families out of poverty (only 10 percent of these families had incomes below the poverty line.) The wives, on the other hand, typically did not work outside the home (only 13

[1/] For a more detailed discussion of the design of the experiment, see Kehrer et al. (1975).

percent were employed at the start of the experiment). In the relatively few families where both the husband and wife were employed, the wife's earnings usually raised family income enough that the family no longer qualified for the receipt of NIT payments. Thus the husband-wife families studied did not include many families where the wife was working temporarily to help "make ends meet."

The husband-wife families studied in Gary would not be considered typical welfare families because of their attachment to the labor force and their income levels, and because public assistance payments were not generally available to husband-wife families in Indiana. But under the income-support plans tested in Gary, many of these families were eligible to receive modest income supplements. Therefore, the analysis of the Gary experiment can provide insight into the consequences of extending an income-supplement program to working, but low-income, families. On the other hand, neither the husbands (working in unionized jobs and possessing seniority) nor the wives (either not working or working in a job that put the family income above the breakeven levels of the NIT plans) would be expected to reduce their work effort significantly in response to the experiment.

The families with female heads were generally much poorer than the husband-wife families studied. Over 80 percent were receiving welfare benefits from the Aid to Families with Dependent Children (AFDC) program immediately prior to the experiment. About three-fourths of the experimental families that switched from AFDC to the experiment had incomes below the poverty line. The female heads on AFDC at enrollment were very dependent on welfare: 86 percent of their monthly income came public transfers, with AFDC grants alone accounting for slightly more than half of their incomes. As with the wives who were studied, only 13 percent of the AFDC female heads were employed.

The female-headed families not on AFDC prior to the experiment were somewhat better off--only 38 percent had incomes below the poverty level. Approximately 60 percent of the income of the non-AFDC female-headed families came from earnings (40 percent of the female heads in these families were employed), while most of the remainder of their income came from Food Stamps, Social Security, and other transfer programs.[1/]

Nevertheless, the female headed families studied were, as a whole, generally quite dependent on public transfers. The few who worked tended to work in low-wage jobs. Thus this group would be expected to be more likely than the husband-wife families to respond to the experiment by reducing their labor supply.

The characteristics of the Gary labor market also have important implications for the interpretations and generalizability of the labor supply findings reported here. The Gary labor market is dominated by a few large steel mills plus a number of manufacturing and fabrication plants. This has two important implications. First, in such a highly institutionalized labor market, few opportunities exist for marginal reductions in labor supply, either through reducing hours worked per week or hours worked during the year. Labor supply reductions generally have

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[1/] A more detailed description of the Gary sample can be found in Kehrer (1977).

to take the form of withdrawing from employment entirely. The results presented below tend to bear this out. Second, such a labor market contains few jobs traditionally held by women.[1/] Consequently, few females in the experiment worked and those who did often worked full-time and were more career-oriented (e.g., as teachers) and thus were not the part-time, casual labor which might be expected to respond to the NIT.

Clearly, then, the characteristics of the population groups and the local labor market are not identical to those in the U.S. as a whole. Consequently, caution should be exercised before extrapolating the results to a nationwide NIT program. The results we have obtained in analyzing the data suggest, in fact, that these particular population and labor market characteristics did affect the response, as will become clear below.

[1/] For example, the Gary SMSA ranked last in an index of labor markets favorable to women devised by Bowen and Finegan (1969, p.774).

B. AN AVERAGE-TAX-RATE LABOR-SUPPLY MODEL

A logical first step in estimating the effect of an NIT experiment is to estimate the mean experimental-control group difference:

$$L = \alpha T + X\beta + \varepsilon, \tag{1}$$

where L is some measure of labor supply, T is an experimental dummy variable equal to one if in the experimental group and zero if in the control group, X is a vector of exogenous socioeconomic characteristics, ε is a randomly distributed error term, and α and β are parameters. In such a model the coefficient α measures the "experimental effect", for it represents the mean experimental-control labor-suppy differential adjusted for differences in X. Estimates of α are presented below.

However, estimates of α are only a first step, for the mean experimental-control difference in one particular experiment cannot be generalized to a different population, such as one with a different income distribution and a different percent of families below breakeven. Also, it cannot be generalized to NIT programs with different tax rates and guarantees. For these reasons a more structural model is needed which specifies the particular budget constraint faced by the Gary participants.

The standard theory of labor supply suggests that hours of work under an NIT should be a function of the relevant net wage rate and the relevant value of net nonwage income. The NIT benefit is calculated to be equal to $[G-t(WH+N)]$, where G is the guarantee level, t is the tax rate (or "benefit reduction rate"), W is the hourly wage rate, H is hours of work, and N is nonwage income. Since earnings are taxed at the rate t, the net hourly wage rate is $W(1-t)$, and since the NIT benefit at zero hours is (G-tN), total net nonwage income is [G+N(1=t)]. Therefore the labor supply function can be written in implicit form as the following:

$$H = f[W(1-t), G+N(1-t)]$$

Estimation of this function on experimental data can be implemented by setting t and G equal to 0 for controls and by setting t and G equal to the correct values for experiments.

Unfortunately, the nonlinearity of the NIT budget constraint complicates this estimation. This hours function applies only to individuals with positive benefits and therefore with income less than the breakeven level of income, G/t. For individuals with income greater than G/t, benefits are zero and the marginal NIT tax rate is zero. In a sense, the problem that arises here is "which" tax rate the individual faces. But a more accurate way of describing it is to say that an individual simply faces <u>multiple</u> tax rates--that is, he or she faces a piecewise-linear budget constraint. The problem is compounded for both experimental and control families because they face other tax and transfer programs as well, either during the experiment or before it: the income tax, the payroll tax, a state income tax, Aid to Families with Dependent Children (AFDC), food stamps, and others. Since each of these programs alone contains multiple tax rates, their cumulative effect is a highly nonlinear budget constraint.

The most common approach to the problem in the literature has been to estimate some variant of the hours equations $H = H[W(1-t'), N']$, where $W(1-t')$ is the "local" net wage and N' is the "local" income intercept--that is, the net wage and income intercept of the segment upon which the individual is observed (Hall, 1973; Hausman and Wise, 1976; Keeley et al., 1978). Unfortunately, there are two serious problems with this technique. First, the tax rate is clearly endogenous since it is related to hours worked through the tax and benefit formulas. For example, in the case of an NIT, one could obtain a spurious negative tax effect simply because those who work more prior to the experiment (e.g., because of greater tastes for work) are more likely to have a zero local tax rate during the experiment. As a result of this problem, most studies (including those mentioned above) have used some type of instrumental-variables approach.[1/] The second problem with the technique is that the function is an incomplete representation of the total labor-supply function, for it only captures the marginal labor-supply choices made within a segment. Implicity it is assumed that an individual first chooses a segment of the piecewise-linear budget constraint, and then chooses a point on the segment. The "choice of segment" and the way the choice is affected by the NIT parameters is not specified.[2/]

To avoid those problems, Burtless and Hausman (1978) have developed an alternative technique which avoids the endogeneity of the tax rate and also estimates the choice-of-segment function. The technique

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[1/] For example, Hausman and Wise (1976) followed Rosen (1976) in evaluating all individuals' tax rates at the same, fixed hours point. Keeley et al. (1978) evaluate the tax rate at the hours worked in a previous period (preenrollment).

[2/] It is also clear that the "choice of segment" and the "choice of hours along a segment" are not separable choices, as the instrumental-variables technique implies. The two should, ideally, be estimated jointly, just as a person jointly chooses them when picking a particular point along the entire budget constraint.

is based firmly on the correct notion that individuals make utilizy comparisons between segments, but it requires the use of a fairly complex maximum-likelihood procedure. For the purposes of this project, where a very large number of equations of different types must be estimated, a simpler alternative is needed.

The method used for our analysis involves approximating the budget constraint rather than representing it in all its detail. Specifically, we smooth the budget constraint by averaging all the marginal tax rates along it. The cumulative tax rate in each segment of the individual's constraint is weighted by its fraction of hours worked covered, and then summed to obtain a weighted-average tax rate. This approach has the advantage of modeling the response to the entire budget constraint, not just to a local segment of it (although it only approximates the entire constraint) and of using a tax rate that is not endogenous. However, since it only approximates the (entire) budget constraint, it must *a fortiori* be considered only an approximation to the true labor-supply function.[1/]

The implications of the method can be seen by specifying the labor supply function linearly:

$$H = \gamma + \delta W(1 - r - t) + \eta(N+B_0), \quad (2)$$

Where "r" is the average tax rate on non-NIT income and "t" is the average tax rate on NIT income. NIT benefits at zero hours are B_0, equal to (G-tN). If an individual has a sufficiently high wage rate that he or

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[1/] See Moffitt and Kehrer (forthcoming) for a full discussion of all the models used in the experiments to address the problem.

she is above breakeven over some portion of the contraint, the averaging procedure assigns to such an individual a value of "t" which is a weighted average of the positive, below-breakeven NIT tax rate and the zero, above-breakeven tax rate. For experimentals who are below breakeven over the entire hours range, the average NIT tax rate "t" is the same as the below-breakeven tax rate. Thus, unlike some other experimental models, an individual who may "above breakeven" at preenrollment (i.e., located on the upper portion of the constraint) is allowed to have a response, although it is assumed that the stimulus (i.e., the average tax rate) is lower than for someone who is below breakeven at all hours points.

The same averaging procedure is followed for non-NIT tax rates. Federal income taxes, for example, impose increasing marginal taxes over the budget line in a series of segments corresponding to tax brackets. AFDC benefits, like those for an NIT, fall to zero at some point and create a nonlinearity in the constraint. The average tax rate "r" is calculated for each individual by constructing the individual's entire set of marginal tax rates at all hours points and by then calculating their average.

The extent to which this approximation of the contraint yields biased coefficients is an empirical question. Averaging the tax rates over the contraint throws away much information on marginal tax rates along the constraint, but the importance of these tax rates is partially affected by the opportunity to adjust hours marginally. On a priori grounds, one would expect that "intramarginal" tax rates would be less important for individuals who are working in highly structured labor markets which make marginal adjustments in hours of work difficult. As has been mentioned before, the Gary labor market is indeed highly structured and offers few part-time jobs to its population; thus it is not implausible that many individuals respond only to the average tax rate on earnings. This institutional feature of the local labor market provides, in fact, the major motivation for our development of an average-tax-rate model.

Equation (2) is modified slightly, but an important way, to allow NIT and non-NIT variables to have different coefficients:

$$H = \gamma + \delta' W(1-r) + \delta''(-Wt) + \eta' N + \eta'' B_0. \qquad (3)$$

This separation is important because it avoids "contaminating" the effect of the experimental stimulus with the effect of nonexperimental stimuli. If equation (2) were estimated, one could easily obtain estimates of δ and η which are, in large part, a result of behavioral responses to nonexperimental variables. This would defeat one of the main purposes of running an experiment, which is to obtain estimates <u>different</u> than those in nonexperimental data. Of course the significance of the coefficient differences is an empirical question, which we will examine below. However, despite the potential importance of separating the coefficients, this is the first experimental study to do so in a formal way.

Given our particular resolution of the kinked-budget-line problem, there nevertheless remain a number of important econometric and specification issues that must be addressed. The actual empirical implementation of equation (3) on the particular data set we have available is discussed in the next section.

C. EMPIRICAL-SPECIFICATION ISSUES

The data base available for the estimation of equation (3) is drawn from the interviews that were conducted periodically throughout the experiment. One preenrollment interview and seven during-experiment interviews provide the basic panel of information on income, labor supply, and demographic characteristics. Each variable is measured in monthly terms as of the month of interview. The selection of an analysis sample from the full set of enrolled individuals is discussed further below.

To estimate equation (3), the specification and econometric issues that must be resolved include the definition of the dependent variable, the choice of sample, the choice of estimating technique, and other issues. As several previous studies have shown, coefficient estimates may be quite sensitive to some of these decisions (e.g., Cain and Watts, 1973; DeVanzo et al., 1976). Unfortunately, while there are well-defined advantages and disadvantages to many alternative specifications and procedures, their net quantitative impacts are usually unknown and consequently there is no single "correct" way to proceed. Therefore, in this study we have chosen to use a variety of specifications and to conduct a large number of sensitivity tests, the nature of which will become clearer below. We will discuss each specification issue in turn and will (1) choose a preferred specification and (2) define a set of reasonable alternatives that we also estimate. The equation that is specified in the preferred way in all respects is called the "basic" model, and will be the benchmark equation around which all the sensitivity tests will revolve.

1. Dependent Variable

The dependent variable used in the basic model is hours of work per month, zeros included. As a sensitivity test an equation is estimated with an employment-status dependent variable--equal to one if employed and zero if not--as an indirect test of whether hours of work are flexible enough to allow individuals to respond marginally to the experiment. If hours are completely inflexible and individuals can respond only by lowering their probability of being employed, the coefficients in the hours equation should be equal to $\overline{H}$ times the coefficients in the employment-status equation, where $\overline{H}$ is the mean hours worked of workers. As has been mentioned before, the nature of the Gary labor market may generate such a result.

As a second sensitivity test, an equation is estimated with a "validated" employment-status dependent variable to test whether any underreporting of employment in the interviews affects the estimated experimental response. Presumably any differential underreporting by experimentals and controls would cause an overly-large labor-supply effect, since experimentals have an incentive to underreport hours in order to increase NIT payments. The validated employment-status variable is created by using data on individual earnings collected from the Indiana Department of Employment Security, to whom employers send earnings reports for certification of eligibility for unemployment insurance. These data may be more accurate than the self-reported data collected in the experimental interviews because it is reported by the employer rather than the individual. Note, however, that this gives us an outside measure only of employment status; hours of work are not reported by employers. In addition, since many low-income workers are not covered by unemployment insurance, their earnings are not reported to the Department. A separate paper by Greenberg et al. (1979) addresses this problem of coverage in more detail; it is ignored here.

2. Budget Line

The construction of the budget line and the average tax rate uses an individual's gross hourly wage rate and amount of non-work-conditioned nonwage income together with the formulas of the tax and transfer programs for which he or she is eligible. The formulas for two tax programs, the federal income tax and the social security payroll tax, and for two transfer programs, AFDC and the NIT, are involved.[1/] Both the NIT and AFDC reimburse positive taxes, and this must be taken into account in the formulas. In addition, the NIT taxes some forms of income at a 100 percent rate instead of at a rate of t. The details of the computations are shown in Appendix B.

The computation of the average tax rates is very simple once the end points of the budget constraint are chosen, for the average net wage rate is simply the slope of the line drawn between the endpoints. For the Gary sample, the endpoints chosen are zero hours of work and 173 hours of work per month, the latter being approximately equal to full-time work. This choice is based upon the tabulated characteristics of the

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[1/] The Indiana income tax is ignored because it is very small in magnitude. Food stamps are also ignored in the basic model because they are in-kind income and thus do not play the same role as cash income in the theory. However, as discussed below, food stamps are included as a sensitivity test.

sample, which show that few individuals worked over 173 hours, either by over-time or by moonlighting. The detailed average-tax-rate formula is also presented in Appendix B.

3. Estimating Technique

Since hours of work are clustered at zero for many of the subsamples, Tobit is used to estimate the basic equation. Whenever employment status is the dependent variable, probit is used to estimate the equation instead. As a sensitivity test, ordinary least squares (OLS) is also used to estimate equations of both types. In addition, since the data consist of a time series of cross-section observations, the equation is also estimated with a generalized least squares technique adapted for intermittent panel data (Avery and Watts, 1977). Unfortunately, it is very difficult to combine limited-dependent-variable techniques with appropriate panel-data techniques--hence the necessity to use one technique or the other, but not both. To control for the pooled data problem, an equation is also estimated which includes a dummy variable for each time period.

4. Sample Selection

Three major decisions are made regarding the selection of the analysis sample. First, several subgroups that are expected to have structurally a-typical labor-supply functions are deleted. These are the aged, the young, the self-employed, and the disabled. Second, families who changed marital status during the experiment (i.e., went from husband-wife to single-headed or vice-versa) are included in the sample and classified according to their contemporaneous marital status.

This decision is based upon previous research by Wolf (1977) which showed that the Gary experiment had no effect on marital dissolution, implying that marital status can be treated as exogenous to the experiment. Third, families who left the experiment ("attriters") are included in the sample for periods before their departure. This decision is based upon research by Hausman and Wise (1979) which indicates that there is little attrition bias in the Gary sample in a structural supply equation (i.e., one which includes the usual right-hand-side taste variables--these control for attrition effects). Nevertheless, despite our a priori notions, all three of these sample selection decisions are subjected to sensitivity testing by reestimating the equation by different criteria (see below).

5. Time Periods Examined

For the basic equation all seven during-experiment interviews are included in the data set rather than selecting only those in the middle year or middle two years of the experiment. In many analyses of other experiments' data, only the middle periods of the data have been used because it is suspected that this most nearly captures the long-run response to an NIT. Early periods may be affected by adjustment and start-up factors, while late periods may be affected by readjustments in anticipation of the end of the experiment. In the Gary data, however, there appears to be no such pattern in the mean experimental-control differences. The pattern of differences, reported in Appendix E, shows very little variation over time in the response and sometimes even shows a greater response in the earlier or later periods of the experiment than in the middle. Nevertheless, since these mean experimental-control-differences may not hold up in a structural model, we have also estimated our equation on the middle period of the experiments as a sensitivity test.

Another sensitivity test to our use of seven monthly data points is afforded by the use of quarterly data, which were made available late in the analysis. These data, formed by averaging a continuous monthly data file into quarters, represent more observations and a more continuous profile. The efficiency of the estimates, if not their consistency, should be improved by reestimation on this data set.

6. Sample Stratification and Preenrollment Differences

One of the problems in using the data from the Gary experiment is that the sample was stratified by income level and that the experimental randomization took place only within these income strata (Conlisk and Watts, 1969). Between strata, different experimental-control allocations were chosen according to a criterion function developed by Conlisk and Watts. The result of this stratification, which was followed in the other experiments as well, is that there are occasionally experimental-control differences in labor supply prior to the experiment.

The work by Hausman and Wise (1977) in this area provides evidence that the stratification had little impact in the Gary experiment. Hausman and Wise showed that the different sampling ratios by income level can be modeled as a straightforward type of selection bias and that Tobit-like maximum-likelihood procedures can correct it. In their empirical work on the Gary data, however, the actual empirical magnitude of the difference between correctly-estimated coefficients and biased, OLS coefficients is quite small. As it turns out, this may be because (ironically) the stratification was implemented very poorly in Gary, for very inaccurate measures of income were used in the computations.

Nevertheless, experimental-control differences did occur in some of our preenrollment-estimated equations. This may be a result of some residual stratification effect or it may be a result of other factors--either from the partial failure of the randomization or from sample deletions that cause nonrandom experimental-control differences. In any case, we therefore control for possible preenrollment differences in all our equations. There are basically two different ways of doing so. The first is to estimate both preenrollment and during-experiment experimental-control labor-supply differences, and to measure the effect of the experiment as the change in this difference from the earlier to the later period. This procedure has a familial resemblance to the first-difference method. The variation of it which we use is to pool both preenrollment and during-experiment observations into a single equation and to "net out" preenrollment differences. For example, in the context of the treatment-difference model considered in equation (1) some while back, this "netting out" procedure involves estimating the equation:

$$L = \alpha T + \beta TD + \delta D + \varepsilon,$$

where T is the previously-defined treatment dummy and D is a dummy equal to 1 if the observation on L is during the experiment and 0 if it is prior to the experiment. The coefficient β measures the experimental effect (i.e., the experimental-control difference net of preenrollment differences, as measured by α).

The second method is to estimate a during-experiment equation including the preenrollment value of the dependent variable as an independent variable. For convenience of exposition, the first method is labeled the "Net" model and the latter is labeled the "Lagged" model. As a rough rule of thumb, the Net model is relatively more desirable when the preenrollment selection criterion is related to unobserved variables, and the Lagged model is preferred when the criterion is related to observed variables (see Appendix C). In the basic model of this paper, the Net model is used because (1) the observed variable usually presumed to cause preenrollment differences in the NIT experiments is the income variable used in the sample income stratification but, as mentioned before, the stratification had little effect in Gary; and (2) the lagged dependent variable may bias the coefficient on the wage rate, which happens to be interacted with a treatment variable (the tax rate). Nevertheless, as a sensitivity test, the Lagged model is also estimated.

7. Wage Rates

Missing data for wage rates of nonworkers is a standard problem in labor-supply studies, and can be partly solved by using an instrument for the wage rate or by adopting more complex maximum-likelihood procedures (Heckman, 1974). In an experiment there is the additional problem that the NIT may affect individual wage rates; therefore, the experimental response may be missed if the during-experiment wage rate is used as an independent variable. In the basic model used below, actual preenrollment wage rates are used for workers and a predicted preenrollment wage rate is used for nonworkers. This asymmetry is allowed in order to increase the variance in the wage-rate variable, for predicted wage rates generally have relatively little variance. On the other hand, some bias may result from this procedure, for it introduces an error term into the wage-rate variable which is related to the dependent variable (i.e., whether working or not). Therefore, as a sensitivity test, the equation is also estimated with the predicted preenrollment wage used for all workers. In addition, a sensitivity test to the use of preenrollment values in general is conducted by estimating the equation using contemporaneous wage rates. If the experiment has an effect on wages, the coefficient estimates may differ. See Appendix D for the detailed wage-predicting equations.

8. Interdependence of Family Labor-Supply Decisions

The model developed in the previous sections is a model of individual labor supply and has not included the possibility that the individual may be a member of a household which contains more than one potential worker. However, the labor-supply decisions of household members may be interdependent. Family labor supply decisions have been analyzed in the context of an NIT by Killingsworth (1976). In the most general non-tax case, the wage rates of all employable household members should appear as exogenous variables in the labor supply equation of each employable person. If contemporaneous earnings of other household members are entered instead, two types of bias may arise. First, the presence of nonzero cross-substitution effects will bias the coefficient. However, as Killingsworth notes, the available empirical studies do not agree on the sign and magnitude of the cross-substitution effect. Second, even if cross-substitution effects are zero, a second type of bias may arise from the simultaneity that works through the income effect alone.

The problem is greatly complicated when tax and transfer programs such as the NIT are introduced. In the general tax case, the n tax rates in an individual's budget constraint (t_1, t_2, ..., t_n) are a function of other family members' labor supplies; thus the net wages are no longer exogenous. Even the drastically simplified, linearized budget constraint we use here is subject to the same problem, for the tax rates over the zero-to-full-time range which are averaged for an individual are different according to the value of nonwage income and the income of the spouse.

Our approach is to estimate a structural rather than reduced-form equation and to take into account only the interdependence of labor supply of the spouse. We use an instrumental variable for the earnings of the spouse,equal to the spouse's preenrollment wage rate times a fixed number of hours worked for the entire sample, equal to the mean. Thus in the husbands' equations, we add to his nonwage income the value of his wife's wage rate times 95 hours per month, the mean in the wives' sample. In the wives' equations, we add to her nonwage income a comparable instrumental variable for the husbands' earnings. In all equations, including those for female heads, the actual, contemporaneous earnings of "tertiary" family members is added to nonwage income.

For wives we also test a model with the "male chauvinist" assumption (Killingsworth, 1976) that wives take their husbands' earnings as exogenous, although not vice-versa. Husbands' actual, contemporaneous earnings are added to the wife's nonwage income. The results from this model and the one above may bracket the true effects for wives.

9. Other Independent Variables

Beside the four budget-constraint variables of equation (3), independent variables are included for the number of adults in the family, the total number of children, and the presence of children in various age ranges as dummy variables, which all together proxy several different effects (the need for home time, the availability of other people to supervise children, the number of people needing financial support, etc.). The local SMSA unemployment rate and the season (one if summer, zero if not) at the time of the interview are included to control for cyclical and seasonal variations in employment in the Gary SMSA. A variable for the presence of a multiple-family household--i.e., the presence of a separate family in the household which may share income, rent, expenses, etc.--is included because such families may be receiving some financial support not captured by the income variables in the equation. A number of these households were enrolled in the experiment.

10. Other Issues

Limited Duration of the Experiment. As many observers have noted, the limited-duration problem may be the greatest disadvantage of the income maintenance experiments. Some commentators have automatically assumed, however, that the response in the experiment will be therefore smaller than that in a permanent national program, but there are actually effects that work the other way (Metcalf, 1973; Ashenfelter, 1978). If income effects are negative (i.e., leisure is a normal good), it is true that a permanent program would provide many more years of benefits and thus would generate larger labor-supply reductions. But if intertemporal substitution effects are nonzero, individuals will also tend to take advantage of the short duration of the reduced price of leisure (relative to a permanent program) and will tend to overrespond. The net effects of these two opposing forces is ambiguous.

Also, as Rees and Watts (1975) point out, the net effect of a permanent NIT may be understated by a short duration experiment if it is costly to find a job after a worker quits or if it is difficult to change hours marginally. For example, adult males have a strong attachment to the labor force and may not wish to undertake the risk involved in quitting a job during a short-duration NIT.

However, we believe that the biases introduced by limited duration are probably relatively minor. For example, Metcalf (1974) developed a method of using consumption data to estimate the pure life-cycle bias under costless adjustment, and found that the biases were small--2 to 6 percent in the substitution (tax) effect and 8 to 27 percent in the income (guarantee) effect. Perhaps more accurate estimates are those from the Seattle-Denver experiment, where both three-year and five-year families were enrolled. Some results (Burtless and Greenberg, 1978; Keeley et al., 1978; Robins and West, 1978) indicate a substantial but insignificant difference in response between the groups. The most formal model (Moffitt, 1979) indicates that there is a slight underresponse in the 3-year sample. As for costs of adjustment, it appears that they are significant (Robins and West, 1978). However, a three-year experiment does not appear to seriously understate the response, for most of the labor-supply adjustment takes place within a two-to-four-year period.

Hawthorne Effect. A second problem is the possibility of a Hawthorne effect which occurs if the experimental subjects react to the act of being studied itself rather than to the experimental treatment, perhaps because of repeated interviewing or from publicity in the local media. In the usual case of such effects, the subjects learn the objective of the study and possibly the biases of the investigators from the interviewing and/or the publicity, and react to this knowledge itself, perhaps by changing their behavior so as to fulfill the prior expectations of the investigators.

Hawthorne effects are not likely to be a serious problem in the income maintenance experiments for two reasons. First, to the extent that such effects are additive and affect both experimentals and controls (as repeated interviewing and publicity do), the experimental-control difference may be unaffected. Second, Hawthorne effects are usually only found in small-scale social experiments where there is extensive direct contact between the investigators and the participants. In a large urban experiment with over a thousand subjects, in which there is little contact between subjects and investigators, and where the benefit-determination process is relatively impersonal (families mail in an income report form and receive a check), it is implausible that the subjects would internalize the perceived norms of the investigators and adjust their labor supply accordingly.

Income Truncation. Finally, a potential problem in all the experiments was the income truncation of the sample. In the New Jersey experiment, for example, no families with income greater than 1.5 times the poverty line were enrolled, causing substantial biases in the response (Hausman and Wise, 1976). When this issue was examined in the Gary experiment, however, the bias was found to be small and empirically unimportant (Hausman and Wise, 1977). The reason for this appears to be that a relatively large number of high income families were enrolled. Although smaller in number than low-income families, they were apparently great enough to fill out the income distribution and eliminate any truncation bias.

D. BASIC MODEL RESTATED

Having discussed the empirical specification at some length it will be useful to restate what we have called the "basic" model. The equation to be estimated is the following:

$$H = a_0 + a_1(-Wt)(D) + a_2(B_0)\ (D) + a_3W(1-r) \tag{4}$$

$$+ a_4N + a_5D + a_6(-Wt) + a_7B_0 + a_0X,$$

where

H = hours of work per month

W = hourly wage rate

t = average NIT tax rate

D = during-experiment dummy variable, equal to 1 if a during-experiment observation and 0 otherwise

B_0 = NIT benefit at zero hours

r = average non-NIT tax rate

N = non-NIT income at zero hours

X = vector of other variables.

As explained above, preenrollment differences in this "Net" model are controlled for by including preenrollment observations in the equation and by estimating coefficients on preenrollment-calculated net-wage and nonwage-income variables. In the equation, coefficients a_6 and a_7 measure preenrollment NIT differences (if any), and coefficients a_1 and a_2 measure the effect of the experiment. That is, the during-experiment dummy, D, is used to "net out" any preenrollment differences.

Equation (4) is estimated with Tobit on a pooled sample of all seven during-experiment interviews and one preenrollment interview, including both attriters and those with changed marital status for the periods for which they have data available. Preenrollment wage rates are used (predicted for nonworkers). All these procedures we subjected to sensitivity tests, to be discussed in Section II.

II. FINDINGS

Estimates of α in equation (1) are shown in Table 1. A range of estimates are provided by the use of two different statistical techniques to control for possible preenrollment experimental-control differences (see Appendix E). The table shows experimental effects on employment status and unconditional hours worked. The results show significant labor supply reductions for husbands and female heads, but not for wives. For husbands, employment-status reductions of 2.7 to 4.9 percent and unconditional hours reductions of 2.9 to 6.5 percent occurred. For female heads, employment-status and unconditional-hours reductions of 26 to 30 percent are found. The female-heads effects, it should be realized, are relative to a control group in which 80 percent of the female heads are on AFDC.

The results for husbands are very similar to those in other experiments, which ranged from 1 percent to 8 percent (Moffitt and Kehrer, forthcoming). Consequently, this should increase our confidence in the general order of magnitude of the male NIT response. On the other hand, the results for wives are very different from those in the other experiments, which ranged from 15 percent to 55 percent (Moffitt and Kehrer, forthcoming). One possible explanation is the very low employment rate of wives (15 percent): less than 90 of the 545 wives in the sample were working at preenrollment, experimental and control combined. This could have made differential experimental-control behavior difficult to detect statistically. This was probably a result of the Gary labor market, which, as has been mentioned before, is dominated by the steel industry and provides very few part-time jobs.

The results for female heads are considerably larger than the 12 percent response found in the Seattle-Denver experiment, the only other experiment designed to estimate the response of female heads. The absolute magnitudes of the hours reductions in the two experiments are close to one another but, given the much lower employment rate and hours worked in the Gary sample, a smaller response should be expected. The difference does not appear to be a result of different racial populations between the experiments because no statistically significant differences among the races were found in Seattle-Denver. Alternatively, the much lower AFDC standard in Indiana ($205 per month for a family of four in 1972) compared to Seattle ($294) and Colorado ($242) could have made the net NIT stimulus (i.e., the NIT benefit minus the AFDC benefit) larger in Gary. However, the greater generosity of the Seattle-Denver NIT plans compensates for this difference. The net stimulus is about $100 per month for female heads in both experiments. Another possibility is simply that the tax and guarantee elasticities are stronger in Gary. To explore this hypothesis it is necessary to move to the estimates of equation (4).

A. BASIC MODEL

Table 2 shows the estimated budget-line coefficients of equation (4).[1/] For husbands, the results show a weak income effect (significant

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[1/] Mean Tobit coefficients are presented because they are the relevant explanators of the total hours responses in Table 1. See McDonald and Moffitt (forthcoming). The original beta coefficients can be obtained using the probabilities in the Table footnotes. Estimates of the other coefficients in the equation are available in Appendix E.

at the 20-percent level) with a mean elasticity of 5 percent, but an insignificant net-wage effect. These fairly inelastic responses are common for prime-age males and are not exceptional. They are also identical to the estimates of Burtless and Hausman (1978), who only examined males and also found an insignificant net-wage elasticity and a significant income elasticity of 5 percent. Although there are other important model differences between this study and theirs, the most important is the different treatment of the nonlinearity of the budget constraint. The conformity of result indicates that, at least on this sample, the average-tax-rate procedure is not a bad approximation. The conformity may again be a result of the highly structured Gary labor market. About 65 percent of the husbands worked in a steel mill and only 7 percent worked part-time (1 to 34 hours per week)--the others worked around 38 hours per week or not at all. This lack of flexibility could make marginal hours adjustments difficult and therefore could make the averaging procedure--which throws away information on many intra-marginal tax rates--a satisfactory approximation. It should also be noted that this aspect of the Gary labor market may also explain why no net-wage (i.e., tax) effects were found here, as they have been in other experiments. Decreases in the tax rate may not increase work effort if marginal hours increases cannot be made.

The results for wives show both insignificant net-wage and insignificant income effects. This confirms the total lack of response discussed above. Again, the low employment rate (15 percent) of the sample may be part of the explanation, together with the occupational structure of the local labor market. Another indication of this constraint is that part-time work was very infrequent--9 percent of the sample--compared to the amount of part-time work among wives in other samples.

For female heads, the response shows a significantly negative income effect with an elasticity of .23, but an insignificant net-wage effect. The lack of a response to the tax rate may again be a result of inflexibility in the Gary labor market. But, like husbands, female heads did respond to the guarantee. Indeed, the magnitudes of the husbands' and female heads' coefficients are rather close to one another. This may be because female heads also must often provide earnings for an entire family, similar to many husbands.

The size of the female-head guarantee effect goes a long way twoard explaining why the response here is larger than that in Seattle-Denver. The comparable income effect in Seattle-Denver is less than one-third of the Gary effect (-2.02 vs. -6.35). The effect of this difference on the response can be illustrated by applying the Seattle-Denver coefficients to the Gary guarantee-and-tax-rate stimuli. When this is done, the predicted Gary response of 26-30 percent falls by one-half (i.e., to 13-15 percent). This explains most of the difference with the Seattle-Denver 12-percent response.

Why the Gary female heads have a stronger income effect is unclear. The most likely explanation lies in the fact that the Seattle-Denver sample of female heads is fairly high-income, at least compared to that in Gary. For this reason Seattle-Denver female heads may simply have had a greater committed level of work effort. For example, Seattle-Denver female heads worked an average of 80 hours per month whereas

Gary female heads worked only 31 hours. If there is a nonlinearity in the response to an NIT, with greater responses at lower income and hours levels, this could explain the difference.

Table 2 also shows the coefficients on the non-NIT income variables variables. The most striking difference is in the magnitude and/or significance of the NIT and non-NIT coefficients. Relative to the NIT coefficients, the non-NIT coefficients show more significant and larger income effects for husbands, significant net-wage effects for wives, and more significant income effect for female heads. (However, wives have a significant but positive non-NIT income effect.) Moreover, the coefficient differences are significant at the 5 percent level. These results show the importance of estimating NIT tax and guarantee coefficients separately from those of non-NIT income. This could also further explain some of the above-mentioned differences between the NIT estimates in this study and those in other experimental studies where the coefficients were constrained to be identical.

Numerous sensitivity tests were performed on this equation. The equation was estimated with OLS and generalized least squares, instead of Tobit; preenrollment differences were controlled for by lagging the dependent variable instead of "netting out" preenrollment treatment differences; the data were averaged instead of pooled; only the middle year of the experiment was examined; a variety of wage instruments were used; and so on. We discuss these in the next section. The results show that the estimates of the basic equation are quite robust, especially given the insignificance of many of the coefficients. Perhaps most notable is that none of these tests revealed any hidden response of wives.

TABLE 1

ESTIMATED MEAN EXPERIMENTAL-CONTROL LABOR SUPPLY DIFFERENCES

Dependent Variable	Husbands Net	Husbands Lagged	Wives Net	Wives Lagged	Female Heads Net	Female Heads Lagged
Employment Status (1 if employed, 0 if not)	-.042**	-.023	.004	..003	-.055**	-.053***
Percent of Control Mean	-4.9	-2.7	2.8	2.1	-26.8	-25.8
Unconditional Hours per Month[b/]	-9.53	-4.24	1.18	.32	-9.35	-8.23
Percent of Control Mean	-6.5	-2.9	5.0	1.0	-30.0	-25.9

[a/] See Appendix E for full regression results.

[b/] No significance tests. See Appendix E.

**Coefficient significant at 95 percent level.

***Coefficient significant at 99 percent level.

TABLE 2

MEAN COEFFICIENTS IN TOBIT HOURS/MONTH REGRESSION[a/]

	Husbands		Wives		Female Heads	
	Coefficient	Elasticity	Coefficient	Elasticity	Coefficient	Elasticity
NIT:						
Income Effect[b/]	-5.85 (1.42)	-.05	4.07 (.58)	.07	-6.35* (1.91)	-.23
Net Wage Effect	-3.60 (.36)	-.01	12.67 (.65)	.16	-4.61 (.49)	-.12
Non-NIT:						
Income Effect[b/]	-24.53** (15.38)	-.13	1.61** (3.00)	.45	-4.94** (4.98)	-.20
Net Wage Effect	-.621 (.33)	-.01	5.55*** (3.15)	.46	-4.38** (2.46)	-.27

Notes: Unsigned t-statistics are in parentheses.

*Significant at the 10-percent level.

**Significant at the 5-percent level.

***Significant at the 1-percent level.

[a/] All coefficients evaluated at the mean value of the Tobit index. That is, if β is the vector of coefficients, X is the vector of independent variables, and F is the cumulative normal distribution function, the coefficients in the table are $\beta F(\bar{X}\beta/\sigma)$. $F(\bar{X}\beta/\sigma)$ equals .99, .15, and .21 for husbands, wives, and female heads, respectively.

[b/] Divided by 100.

B. SENSITIVITY TESTS

1. Dependent Variable

Table 3 shows the results of the various sensitivity tests, the first of which involves the use of a different dependent variable. The other dependent variable examined is employment status, defined as equal to one if the person was employed 50% of the month or more, and zero otherwise. As the probit-estimated coefficients in the table show, the pattern of the signs and significance level is the same for employment status as for hours. Furthermore, when the probit coefficients are multiplied by mean hours, the resulting implied hours effects are quite close to the Tobit hours effects (at least in those cases where the underlying coefficients are significant). This provides direct evidence that most of the response occurred in employment status and indirect evidence that hours of work are inflexible in the Gary labor market.

2. Estimating Technique

The basic model for hours worked is also estimated with ordinary least squares (OLS); the guarantee and net-wage coefficients are shown in Table 3. These estimates are very close to the (mean) Tobit estimates. Also, the employment status equations have been estimated both by OLS and by a generalized least squares (GLS) method designed to control for the cross-section time-series nature of the data set (it is an error-component model with a constant individual effect and a non-auto-correlated random effect). The table shows that the OLS estimates are very similar to the probit estimates. The GLS estimates are also close to the probit estimates

TABLE 3

SUMMARY OF SENSITIVITY TESTS ON AVERAGE-TAX-RATE MODEL

	Husbands		Wives		Female Heads	
	Guarantee Coefficient	Net-Wage Coefficient	Guarantee Coefficient	Net-Wage Coefficient	Guarantee Coefficient	Net-Wage Coefficient
1. Dependent variable[a/]						
H (Tobit)	-5.85	-3.60	4.07	12.67	-6.35*	-4.61
	(1.42)	(.36)	(.58)	(.65)	(1.91)	(.49)
ES (probit)	-.049*	-.057	.38	.066	-.040*	-.02
	(1.81)	(.89)	(1.47)	(.86)	(1.83)	(.35)
Hours effects derived from ES equation	-8.05	-9.40	5.59	9.70	-6.24	-3.51
2. Estimating technique[b/]	-5.46					
H (OLS)	-5.46	-3.68	4.15	5.15	-6.45*	-4.03
	(1.47)	(.41)	(.67)	(.27)	(1.83)	(.39)
ES (OLS)	-.034	-.032	.037	.109	-.032	.002
	(1.56)	(.62)	(.40)	(.86)	(1.45)	(.03)
ES (GLS)	-.031**	-.045	.036	.031	-.051**	-.072*
	(2.09)	(1.28)	(1.13)	(.33)	(3.61)	(1.93)
H with period dummies (Tobit)	-6.50	-5.28	4.33	13.29	-6.32*	-4.49
	(1.54)	(.52)	(.60)	(.67)	(1.90)	(.48)
3. Income underreporting (OLS)[c/]						
"Validated" ES	-.032	-.015	-.007	.027	-.041*	-.076
	(1.54)	(.31)	(.14)	(.19)	(1.79)	(1.13)
4. Sample Selection						
Readdition to sample of screened-out groups (OLS)[d/]						
H	-4.63	-5.54	-7.58	-19.57	-6.24	-3.20
	(1.20)	(.16)	(.89)	(1.01)	(1.58)	(.30)
ES	-.033	-.048	.034	.113	-.017	.023
	(1.40)	(.84)	(.85)	(.95)	(.85)	(.36)
Deletion from sample of attriters and discontinuous families[e/]						
H (Tobit)	-5.67	-2.47	-4.03	-14.55	-8.40**	-10.12
	(1.25)	(.22)	(.50)	(.60)	(2.54)	(1.10)
5. Preenrollment differences[f/]						
H (Tobit)	-3.81**	-5.47	-6.67**	-14.72*	-4.50**	-4.89
	(1.98)	(1.21)	(2.21)	(1.78)	(3.58)	(1.59)
ES (OLS)	-.016	-.012	.003	.012	-.028**	-.033
	(1.58)	(.49)	(.12)	(.19)	(2.92)	(1.35)
6. Time period[g/]						
Middle Year Only H (Tobit)	-7.54*	-7.95	4.13	11.43	-7.30**	-5.67
	(1.75)	(.76)	(.55)	(.55)	(2.11)	(.58)
Middle Year Averaged H (Tobit)	-7.71	-8.27	6.50	13.16	-7.49	-4.21
	(1.35)	(.61)	(.64)	(.46)	(1.55)	(.31)

TABLE 3 (continued)

	Husbands		Wives		Female Heads	
	Guarantee Coefficient	Net-Wage Coefficient	Guarantee Coefficient	Net-Wage Coefficient	Guarantee Coefficient	Net-Wage Coefficient
Pooled quarterly observations						
H (OLS)	-4.11 (1.06)	4.13 (.45)	-.72 (.12)	.06 (.003)	-4.72 (1.47)	-9.98 (1.05)
ES (OLS)	-.03 (1.14)	.01 (.15)	-.01 (.17)	.02 (.16)	-.04 (.53)	-.05 (.84)
Averaged quarterly observations						
H (OLS)	-5.54 (.81)	-8.25 (.61)	9.66 (.88)	13.25 (.40)	-13.03** (2.71)	-19.87 (1.18)
ES (OLS)	-.035 (1.17)	-.064 (.84)	.057 (.81)	.106 (.51)	-.074** (2.47)	-.097 (1.18)
Wage rates (OLS, ES)[h/]						
Mixed preenrollment wage wage	-.034 (1.56)	-.032 (.62)	.037 (.90)	.109 (.86)	-.032 (1.45)	.002 (.03)
Pure preenrollment instrument	-.032 (1.52)	-.029 (.54)	.049 (1.26)	.149 (1.23)	-.021 (.88)	.040 (.60)
Mixed contemporaneous wage	-.025 (1.14)	-.057 (.10)	.052 (1.41)	.177 (1.36)	-.009 (.45)	1.07 (1.62)
Pure contemporaneous instrument	-.023 (1.10)	-.003 (.06)	.035 (.92)	.113 (.86)	-.010 (.48)	.109* (1.67)
Actual Contemporaneous Husbands' Earnings (ES, OLS)	-	-	.051 (1.42)	.189* (1.68)		

NOTES: All guarantee coefficients multiplied by 100. Unsigned t-statistics in parentheses.

*Significant at the 90-percent level.

**Significant at the 95-percent level.

H = hours per month
ES = employment status (1 if employed 50 percent of month, 0 otherwise)
OLS = ordinary least squares

[a/]Tobit and probit coefficients evaluated at mean of index, ES hours effects obtained by multiplying ES coefficients by the difference in mean hours of those with ES = 1 and ES = 0.

[b/]GLS (generalized least squares) is an error-component technique with a constant individual effect and a non-auto-correlated random effect.

[c/]These equations were estimated by Caroline Roth.

[d/]Readdition of self-employed, aged, young, and disabled. These equations were estimated by David Horner.

[e/]Attriters are families who left the experiment before disenrollment. Discontinuous families are those who changed from husband-wife to single-headed or vice-versa. The two groups are simultaneously deleted because of the difficulty in distinguishing them, e.g., many husband-wife families may attrite as they split up.

[f/]The "Lagged" model is used: only during-experiment observations are included, and the preenrollment value of hours worked is included on the right-hand-side.

[g/]The basic model uses seven monthly interview observations. In the quarterly tests, observations defined as monthly averages for each 3-month period are used. In the first and third tests all observations are pooled. In the second and fourth tests they are averaged into a single observation per family.

[h/]The "mixed preenrollment wage" is constructed by using actual preenrollment wages for workers and a preenrollment wage instrument for nonworkers. The "pure preenrollment instrument" is constructed by using the preenrollment wage instrument for all observations. The "mixed contemporaneous wage" is constructed by using the actual during-experiment wage for workers and a during-experiment wage instrument for nonworkers. The "pure contemporaneous instrument" is constructed by using the during-experiment wage instrument for all observations.

except for female heads, where both coefficients are more negative; in fact, the compensated substitution effect is negative, an implausible result. Finally, the addition of period dummies to remove period effects from the pooled data also makes very little impact on the estimates.

3. Income Underreporting

The test using a "validated" employment-status dependent variable (i.e., one collected from government records) shows little effect of underreporting, for the coefficients are quite similar to those in the ES regressions using only interview data. However, it should be noted that Greenberg et al. (1979) have used this same validated data to investigate the question in more detail and have come to a rather different conclusion, at least for female heads. Therefore the effect of underreporting on the response is unclear. See the Greenberg et al. paper for details.

4. Sample Selection

The table also shows that the readdition of the screened-out groups (aged, young, self-employed, and disabled) has little effect on the estimated coefficients for husbands and female heads. The coefficients in the wives' hours equation are, however, considerably different, giving negative income effects and negative compensated substitution effects. Given the perversity of the latter and the low significance levels, little weight should be given to this result.

The Tobit hours equation was also reestimated with the attriters and discontinuous families deleted. The two groups are simultaneously deleted because of the difficulty in distinguishing them (e.g., many husband-wife families may leave the experiment as they split up). The estimates in the table show that the treatment coefficients for husbands and female heads are rather stable (although female heads have more negative income and substitution effects) and that the coefficients for wives are again unstable but highly insignificant statistically.

5. Preenrollment Differences

As indicated in section II-C above, the alternate method used to control for preenrollment differences is the estimation of the "Lagged" model. In this model only during-experiment observations are used and the preenrollment value of the dependent variable is included on the right-hand-side of the labor-supply equation. Both a Tobit hours model and an OLS employment-status model have been estimated, and the results are shown in the table. Once again, the treatment coefficients for husbands and female heads are relatively stable, although the effects on the employment status of husbands are smaller in the Lagged model. However, the results for wives are again very unstable. In the hours equation the wives' coefficients show a significant and negative guarantee effect but also a signficant and negative net-wage effect (the compensated substitution effect is also negative). This anomalous result may be caused by the lagged dependent variable, which may bias the other coefficients in the equation because the lagged value contains measurement error and because it is itself a function of the wage rate, nonwage income, and so on. On the other hand, the employment-status results for wives show even smaller and less significant coefficients in the Lagged model compared to the Net model. Therefore once again we find no evidence of a sensible wives' response.

6. Time Period

Estimation of the model on the middle year's data, whether by pooling the monthly observations or by averaging them, has very little impact. For husbands and female heads, income effects are somewhat stronger but substitution effects are correspondingly weaker. Changing the time period of the analysis by using quarterly data instead of monthly data and by averaging our observations rather than pooling also has little effect on the husbands' income coefficient, and little effect on the insignificance of the husbands' net-wage coefficient or either of the wives' coefficients. For female heads, quarterly pooled estimation makes little difference but averaging our periods forces both more negative income effects and more negative substitution effects. No ready explanation is apparent for this shift.

7. Wage Rate

The use of different wage rates has remarkably little effect on the coefficient estimates (the female heads' net-wage coefficient being the one exception). The lack of an important difference between the estimates using preenrollment and contemporaneous wage rates is no doubt a result of the lack of significant effects of the experiment on wage rates. But also, our separation of the net wage into NIT and non-NIT components eliminates to some degree the importance of the wage rate in affecting the NIT coefficients--that is, (-Wt) varies independently of W only through variations in t.

8. Household Interdependence

The use of actual, contemporaneous husbands' earnings in the nonwage-income variable for wives (rather than an instrument) does increase the net-wage effect and is significant. However, the income effect is still positive and a bit larger and more significant. Therefore this very simple test of the importance of household interdependence provides mixed results.

9. Other Tests

Other sensitivity tests were also performed which are not shown in the table. Addition of the guarantee and tax rate (actually, the net wage difference) for food stamps had no effect on the estimated coefficients. Also, estimation of a "local tax rate" model similar to that of Keeley et al. (1978) provided mixed results, showing some stronger effects for some groups but more perverse effects for others. Details of this test are discussed in Appendix G.

III. SUMMARY

The results we have found for the Gary NIT experiment show work disincentives for some groups but not for others. In particular, significant responses by prime-age married males ranged from 2.9 percent to 6.5 percent of hours worked, and those for female heads of families ranged from 25.9 percent to 30 percent. In addition, these responses seem to be primarily responses to the guarantee level rather than to the tax rate. On the other hand, no response at all was found for wives, either in the aggregate or by tax rate and guarantee level.

These findings require some interpretation. For husbands, at least, the findings are quite similar to those in other experiments, although a bit smaller. The finding of a guarantee response but not a tax-rate response may be a result of the inflexibility of the Gary labor market, as discussed in the paper. This hypothesis is reinforced by the seemingly larger response in employment status than in hours of work. For female heads, the response is a good bit larger than that in the Seattle-Denver experiment, where a negative response was also found but was considerably smaller in percentage terms. Our feeling is that this is largely a result of differences in the characteristics of the samples, for the female heads in the Gary experiment were lower in income and more dependent on AFDC than those in the Seattle-Denver experiment.

The findings for wives are the most dissimilar to those in other experiments, where it was often found that wives experienced the greatest work disincentives of any group. We believe that, in part, this is a result of the nature of the Gary labor market, which offered few work opportunities for females. In fact, the employment rate for wives in our sample was very low (15 percent). In part, it also is a result of the types of working women in the sample, many of whom were full-time, career-oriented workers rather than part-time, casual workers. Although in the final analysis these factors are not enough to expect a zero response, they do provide reason to expect it to be lower than that in other experiments.

The results also show that the measured effects of NIT and non-NIT income are very different. The differences may be explained by limited-duration biases; some evidence (Moffitt, 1979) suggests that it might be. Alternatively, the differences may be true differences--i.e., people may not actually respond identically to different income sources and tax-and-transfer programs--or they may be spurious, simply resulting from inherent biases in nonexperimental data. The nature of the difference suggests an important area for future research.

APPENDIX A

SAMPLE CHARACTERISTICS

TABLE A-1

MEAN CHARACTERISTICS OF HUSBANDS

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Labor-force status:						
Employment rate	.92	.87	.90	.86	.87	.86
Unemployment rate[a/]	.02	.03	.02	.03	.03	.03
Not-in-labor-force rate	.06	.10	.08	.11	.10	.11
Hours of work per week:						
Percentage distribution:						
0	.08	.13	.10	.12	.12	.12
1-5	–[b/]	–	–	–	–	–
6-14	–	–	–	.01	.01	–
15-24	–	–	–	.02	.01	.02
25-34	.02	.01	.02	.04	.05	.05
35-44	.86	.83	.85	.79	.80	.80
45-54	.03	.03	.03	.01	.01	.01
55+	–	–	–	–	–	–
	1.00	1.00	1.00	1.00	1.00	1.00
Mean for sample[c/]	36.9	34.8	36.0	33.8	34.3	34.0
Mean for working sample	40.1	40.1	40.1	38.4	39.0	38.6
Earnings:						
Hourly wage rate	3.51	3.49	3.50	3.52	3.57	3.54
Monthly earnings/100[c/]	5.76	5.50	5.63	5.81	6.06	5.91

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Industry of work (percentage distribution):						
Construction	.01	.02	.02	.01	.02	.01
Non-steel manufacturing	.12	.13	.12	.09	.10	.10
Steel manufacturing	.68	.61	.65	.65	.63	.64
Transportation, Communication, and Utilities	.03	.02	.03	.02	.01	.02
Wholesaling and retailing	.03	.04	.04	.02	.05	.03
Finance, insurance, and real estate	-	-	-	-	-	-
Services	.02	.05	.03	.02	.04	.03
Government	.03	.02	.03	.03	.02	.03
Other[d/]	.06	.11	.08	.15	.13	.14
	1.00	1.00	1.00	1.00	1.00	1.00
Occupation of work (percentage distribution):						
Professional and technical	.01	-	-	.02	.01	.02
Managers	-	.03	.01	-	.02	.01
Sales	-	-	-	-	-	-
Clerical	.02	.03	.03	.03	.03	.03
Craftsmen	.20	.19	.19	.18	.24	.20
Non-transport services	.30	.27	.29	.31	.25	.29
Transport services	.07	.06	.07	.10	.08	.09
Laborers	.27	.26	.27	.18	.22	.19
Service workers, exp. household	.06	.07	.06	.06	.05	.05
Private household	-	-	-	-	-	-
Other	.07	.09	.06	.12	.12	.12
	1.00	1.00	1.00	1.00	1.00	1.00
Age	39.6	39.7	39.6	41.9	41.8	41.9
Years of Education	9.8	9.9	9.8	4.7	10.0	9.8

TABLE A-1 (continued)

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Regression variables:						
No. adults[e/]	2.5	2.5	2.5	2.6	2.5	2.5
No. children	3.2	3.1	3.2	3.4	3.2	3.4
Presence of children:						
0-2	.34	.26	.31	.35	.26	.31
3-5	.41	.43	.42	.39	.44	.41
6-12	.69	.69	.69	.73	.74	.73
13-15	.47	.45	.46	.52	.46	.49
Multiple family	.05	.04	.05	.02	.03	.03
Season	.75	.83	.79	.31	.31	.31
Unemployment rate	3.56	3.48	3.52	4.80	4.82	4.80
$E_o/100$	3.16	0	1.77	2.76	0	1.66
(-Wt)	-1.25	0	-.70	-1.16	0	-.70
$B_o/100$-actual	0	0	0	2.76	0	1.60
(-Wt)-actual	0	0	0	-1.16	0	-.70
N/100	.72	.75	.73	.72	.79	.75
W(1-r)	3.12	3.12	3.12	3.14	3.16	3.15
Number of individuals in sample	293	232	525	1462	963	2425

NOTES:

a/ Unemployment = 1 if unemployed, 0 if employed or not in labor force.

b/ Less than 0.005 in absolute value.

c/ Zeros included

d/ Includes non-workers

e/ Includes principal person.

TABLE A-2

MEAN CHARACTERISTICS OF WIVES

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Labor-force status:						
Employment rate	.16	.16	.16	.14	.14	.14
Unemployment rate[a/]	.08	.11	.09	.03	.03	.03
Not-in-labor-force rate	.76	.73	.75	.83	.83	.83
Hours of work per week:						
Percentage distribution:						
0	.84	.84	.84	.85	.83	.84
1-5	- [b/]	-	-	-	.01	.01
6-14	.01	-	.01	.02	.02	.02
15-24	.02	-	.01	.02	.03	.03
25-34	.01	.03	.02	.02	.03	.03
35-44	.10	.12	.11	.08	.07	.08
45-54	.01	.01	.01	-	-	-
55+	-	-	-	-	-	-
	1.00	1.00	1.00	1.00	1.00	1.00
Mean for sample[c/]	5.39	5.98	5.66	4.63	4.76	4.68
Mean for working sample	33.7	37.4	35.4	30.9	28.0	29.3
Earnings:						
Hourly wage rate	2.40	2.34	2.37	2.33	2.33	2.33
Monthly earnings/100[c/]	.80	.81	.81	.51	.49	.50

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Industry of work (percentage distribution):						
Construction	-	-	-	-	-	-
Non-steel manufacturing	.04	.03	.03	.02	.02	.02
Steel manufacturing	.01	.02	.02	-	.01	-
Transportation, Communication, and Utilities	-	.01	-	-	-	-
Wholesaling and retailing	.05	.06	.06	.03	.03	.03
Finance, insurance, and real estate	.01	-	-	-	-	-
Services	.04	.07	.05	.05	.06	.05
Government	.03	.02	.02	.03	.01	.02
Other[d/]	.82	.81	.82	.87	.89	.89
	1.00	1.00	1.00	1.00	1.00	1.00
Occupation of work (percentage distribution):						
Professional and technical	.02	.02	.02	.01	.02	.01
Managers	-	-	-	-	-	-
Sales	-	-	-	-	.01	-
Clerical	.07	.06	.07	.04	.04	.04
Craftsmen	.01	.01	.01	-	.01	.01
Non-transport services	.03	.03	.03	.01	.03	.02
Transport operatives	-	-	-	-	-	-
Laborers	-	.01	.01	.01	-	.01
Service workers, exp. household	.07	.09	.08	.08	.07	.07
Private household	.01	-	.01	-	-	-
Other	.79	.77	.79	.85	.83	.84
	1.00	1.00	1.00	1.00	1.00	1.00
Age	35.7	36.1	35.9	38.2	38.3	38.2
Years of Education	10.7	10.8	10.7	10.5	10.7	10.6

TABLE A-2 (continued)

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Regression variables:						
No. adults[e]	2.6	2.5	2.5	2.6	2.5	2.6
No. children	3.1	3.1	3.1	3.4	3.2	3.3
Presence of children:						
0-2	.33	.26	.30	.34	.25	.31
3-5	.40	.42	.40	.38	.43	.40
6-12	.68	.69	.69	.72	.73	.72
13-15	.47	.45	.46	.52	.46	.49
Multiple family	.05	.05	.05	.02	.03	.03
Season	.76	.83	.79	.32	.31	.32
Unemployment rate	3.56	3.47	3.52	4.80	4.82	4.81
$E_o/100$	.99	.0	.55	.69	.0	.41
(-Wt)	-.33	0	-.18	-.26	0	-.15
$B_o/100$-actual	0	0	0	.69	0	.41
(-Wt)-actual	0	0	0	-.26	0	-.15
N/100	5.66	5.78	5.71	5.82	5.96	5.88
W(1-r)	1.79	1.72	1.76	1.76	1.71	1.74
Number of individuals in sample	304	242	545	1532	1022	2554

NOTES:

a/ Unemployment = 1 if unemployed, 0 if employed or not in labor force

b/ Less than 0.005 in absolute value

c/ Zeros included

d/ Includes nonworkers

e/ Includes principal person.

TABLE A-3

MEAN CHARACTERISTICS OF FEMALE HEADS

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Labor-force status:						
Employment rate	.26	.23	.25	.19	.19	.19
Unemployment rate[a/]	.14	.18	.16	.12	.12	.12
Not-in-labor-force rate	.60	.59	.59	.69	.69	.69
Hours of work per week:						
Percentage distribution:						
0	.74	.76	.75	.79	.79	.79
1-5	–[b/]	–	–	–	–	–
6-14	.02	.01	.01	.01	.01	.01
15-24	.01	.01	.01	.02	.03	.02
25-34	.03	.03	.03	.03	.02	.03
35-44	.19	.16	.18	.14	.14	.14
45-54	.02	.02	.02	.01	–	.01
55+	–	–	–	–	–	–
	1.00	1.00	1.00	1.00	1.00	1.00
Mean for sample[c/]	9.57	8.59	9.17	6.99	7.04	7.01
Mean for working sample	36.8	35.8	36.7	33.3	33.5	33.4
Earnings:						
Hourly wage rate	2.09	2.00	2.06	2.23	2.21	2.22
Monthly earnings/100[c/]	1.22	1.00	1.13	.73	.73	.73

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Industry of work (percentage distribution):						
Construction	-	-	-	-	-	-
Non-steel manufacturing	.03	.04	.03	.02	.02	.02
Steel manufacturing	.02	.03	.02	.02	.03	.02
Transportation, Communication, and Utilities	-	-	-	-	-	-
Wholesaling and retailing	.10	.05	.08	.04	.04	.04
Finance, insurance, and real estate	-	-	-	-	-	-
Services	.15	.14	.15	.08	.10	.09
Government	.03	.04	.03	.02	.03	.02
Other[d]	.67	.71	.68	.82	.79	.81
	1.00	1.00	1.00	1.00	1.00	1.00
Occupation of work (percentage distribution):						
Professional and technical	.03	.02	.02	.03	.02	.02
Managers	-	-	-	-	-	-
Sales	-	-	-	-	-	-
Clerical	.09	.06	.08	.06	.04	.05
Craftsmen	-	.01	-	-	-	-
Non-transport services	.05	.05	.05	.02	.03	.03
Transport operatives	-	-	-	-	-	-
Laborers	.02	.01	.02	.01	.02	.01
Service workers, exp. household	.15	.15	.15	.08	.09	.08
Private household	.02	.03	.02	.01	.01	.01
Other	.65	.68	.66	.79	.79	.79
	1.00	1.00	1.00	1.00	1.00	1.00
Age	36.0	34.9	35.6	36.7	36.2	36.5
Years of Education	10.2	10.3	10.2	10.2	10.4	10.3

TABLE A-3 (continued)

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Regression variables:						
No. adults[e/]	1.5	1.4	1.4	1.6	1.4	1.5
No. children	2.8	3.0	2.9	2.7	3.0	2.8
Presence of children:						
0-2	.21	.25	.23	.27	.31	.29
3-5	.32	.36	.34	.31	.36	.33
6-12	.62	.69	.65	.59	.66	.62
13-15	.40	.46	.43	.37	.43	.40
Multiple family	.07	.05	.06	.09	.06	.08
Season	.56	.62	.59	.29	.30	.29
Unemployment rate	3.88	3.78	3.84	4.82	4.84	4.83
E_o/100	2.50	0	1.47	2.18	0	1.31
(-Wt)	-.84	0	-.49	-.80	0	-.48
B_o/100-actual	0	0	0	2.18	0	1.31
(-Wt)-actual	0	0	0	-.80	0	-.48
N/100	1.93	1.91	1.92	.74	1.86	1.19
W(1-r)	1.97	1.97	1.97	2.04	1.92	1.99
Number of individuals in sample	444	311	755	2995	2006	5001

NOTES:

[a/] Unemployment = 1 if unemployed, 0 if employed or not in labor force

[b/] Less than 0.005 in absoulte value

[c/] Zeros included

[d/] Includes nonworkers

[e/] Includes principal person.

TABLE A-4

MEAN INCOMES OF HUSBAND-WIFE AND FEMALE-HEADED FAMILIES

	Preenrollment			During-Experiment		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Monthly income/100, husband-wife families:						
Earnings of husband	5.76	5.50	5.63	5.81	6.06	5.91
Earnings of wife	.80	.81	.81	.51	.49	.50
Earnings of others	.11	.08	.10	.15	.15	.15
NIT payment	-	-	-	1.50	.08[a]	.94
Other income	.12	.20	.15	.35	.39	.36
Total family income	6.79	6.59	6.69	8.32	7.17	7.86
Monthly income/100, female-headed families:						
Earnings of female head	1.22	1.00	1.13	.73	.73	.73
Earnings of others	.06	.04	.05	.06	.09	.07
AFDC payment	1.45	1.49	1.47	.13[b]	1.60	.72
NIT payment	-	-	-	2.67	.09[a]	1.65
Other income	.71	.51	.63	.58	.68	.62
Total family income	3.44	3.04	3.28	4.17	3.19	3.79

NOTES:

[a] Control families received a nominal fee for participating in the experiment and submitting to interviews.

[b] A few experimental families received AFDC, but only when a subfamily existed in the household with an eligible AFDC case. The experimentally-defined "female" head never received AFDC.

TABLE A-5

MISCELLANEOUS CHARACTERISTICS OF HUSBAND-WIFE AND FEMALE-HEADED FAMILIES

	Husband-Wife Families			Female-Headed Families		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Number of families at preenrollment	293	232	452	444	307	751
Distribution by experimental plan (percent):						
High guarantee, high tax	28	-	-	31	-	-
High guarantee, low tax	32	-	-	30	-	-
Low guarantee, high tax	20	-	-	18	-	-
Low Guarantee, low tax	21	-	-	21	-	-
	100			100		
Distribution by poverty level (percent):						
Level 1	5	4	5	41	44	42
Level 2	11	5	8	19	36	26
Level 3	23	33	27	24	7	17
Level 4	48	40	44	16	11	14
Level 5	14	18	16	1	2	2
	100	100	100	100	100	100
Number of attributing females	33	59	92	13	30	43
Percent of preenrollment	11	25	18	3	10	6
Number of discontinuous families	41	36	77	21	13	34
Percent of preenrollment	14	15	15	5	4	5

TABLE A-5 (continued)

	Husband-Wife Families			Female-Headed Families		
	Experimental Group	Control Group	Total	Experimental Group	Control Group	Total
Fraction of time spent above breakeven (percentage distribution)						
0	.35	-	-	.90	-	-
.01-.19	.17	-	-	.03	-	-
.20-.49	.18	-	-	.01	-	-
.50-.99	.14	-	-	.01	-	-
1.00	.17	-	-	.03	-	-
	1.00			1.00		
Mean fraction	.36	-	-	.05	-	-
Fraction of time dominated by AFDC payments (percentage distribution):						
0	-	-	-	.71	-	-
.01-.19	-	-	-	.09	-	-
.20-.49	-	-	-	.10	-	-
.50-.99	-	-	-	.05	-	-
1.00	-	-	-	.05	-	-
Mean fraction	-	-	-	.13	-	-

APPENDIX B

CALCULATION OF TAX AND TRANSFER AMOUNTS

This appendix is a brief description of the calculation of the NIT and non-NIT tax and transfer amounts. Non-NIT income equals earned income plus AFDC (female-headed families only) plus all other income minus the federal income tax and the payroll tax. The Indiana income tax is ignored because the amounts are too small and food stamps are ignored because they are in-kind income and hence difficult to value. (Also, very few experimental families received more than the minimum food-stamp bonus.) All other transfer income was assumed exogenous and included in "all other income."

Income amounts were calculated in accordance with the rules and regulations of each program as of the date of the observation. Since the data cover four years of time, these nominal amounts were converted to real amounts by adjusting for inflation. The resulting amounts used in the regressions are in 1970 dollars.

The main input variables in the calculation were the hourly wage rate, all other income, and family size. The wage rate used differed, as described in the text, according to whether it was contemporaneous or preenrollment and whether it was wholly instrumental or only partly instrumental. When the preenrollment wage was used, it was adjusted upward for inflation before being used in the calculations of tax and transfer amounts. All other income was always a preenrollment amount similarly adjusted for inflation. Family size was also taken as of the baseline period.

All income calculations were performed on an annual basis, although they were converted to monthly figures for use in the rgeressions. Earned income equals the wage rate times either zero hours or full-time hours per year (2076). All other income equals nonwage income plus the instrumental earnings of the spouse (equal to the wage rate times mean hours) plus the actual earnings of tertiaries.

The tax and transfer formulas used are the following:

FICA payroll tax. The payroll tax equals a fixed percentage of earnings below the maximum earnings level. The tax rate varied from 4.8 percent to 5.8 percent and the maximum earnings level varied from $7,800 per year to $13,200 per year over the period of the experiment. It was assumed that all workers were covered.

Federal income tax. The federal income tax was calculated according to the rules prevailing in each year, 1970-1974. It was assumed that husband-wife families filed jointly and that female-headed families filed as heads of households. It was also assumed that all families took the standard deduction or the low-income allowance, whichever was lower, and that their exemptions equaled their family size. Income was assumed to be equal to earnings plus "all other income" as defined above. The NIT payment and AFDC were excluded.

AFDC. The AFDC payment was calculated only for female-headed families, since AFDC-U was not available in Indiana. The AFDC payments at zero and full-time hours were calculated for any female-headed family who was on AFDC at any time during the 1970-1974 period. This procedure implicitly assumes that any family that was on AFDC at any time during the four years "faced" the AFDC budget constraint--in the sense of "taking it into account" in deciding their labor supply, whether or not that involved actually participating in AFDC--over the entire four years. Although this assumption clearly only approximates the true perceptions of our female heads, it is probably better than alternative assumptions. For example, calculating an AFDC payment at each point in time only if the family were on AFDC at that time would create a completely endogenous variable, since many families are on AFDC when they are not working and are off AFDC when they are working. On the other hand, there may be families who have never been on AFDC who nevertheless take its availability into account in their labor supply decisions. Our measure of AFDC underestimates this effect, but probably overestimates to some degree the effect for families who "take AFDC into account" for some of the periods and not for others.

The AFDC benefit formula was taken from Moffitt (1979), who estimated effective tax rates and guarantees in the Indiana AFDC program. The welfare department in Indiana sets a ceiling on the AFDC benefit, but reimburses taxes paid, some shelter expense, and some other work-related expenses. The average effective tax rate on AFDC is 0.38, considerably less than 0.67 because of all these factors.

Gary NIT payment. The Gary payment formula is the following:

$$B = G - t(E + N_1 + FS) - N_2 + TX;$$

where

B = NIT benefit,
G = guarantee level,
t = tax rate,
E = earnings,
N_1 = nonwage income category 1,
FS = food-stamp bonus,
N_2 = nonwage income category 2, and
TX = total federal and FICA taxes.

The experiment provided a guarantee that was scaled by family size and was adjusted upward semi-annually for increases in the cost-of-living. The tax rate on income was t (0.4 or 0.6) for all income except that in N_2, which included unemployment compensation, general assistance, and the adult categories of public assistance; for this income the tax rate was 100 percent. The food-stamp bonus was taxed at the treatment rate but, in fact, the receipt of food stamps by experimental families was minimal. Taxes were reimbursed at a 100-percent rate; this and the 100-percent taxation of N_2 was designed to eliminate their effects on the cumulative tax rate facing experimentals.

By the rules of the experiment, experimental families could not receive AFDC payments but were guaranteed that their NIT payments would be no less than what they would have received in AFDC payments. Consequently the experiment calculated both AFDC and NIT payments for a large number of families. However, because the NIT payment usually dominated the AFDC payment, this provision had little effect. Therefore, it was ignored in the NIT payment calculations in this paper.

To determine the average NIT and average non-NIT tax rate, these benefits and taxes were calculated at both zero hours and at full-time hours. Denote B_O and B_f as the NIT benefit at 0 and full-time hours, respectively, and denote Y_O and Y_f as the sum of all other income at the same two points. Then the average NIT tax rate equals

$$1 + \frac{B_f - B_o}{WH_f}$$

and the average non-NIT tax rate equals

$$1 + \frac{Y_f - Y_o}{WH_f}$$

where W is the hourly wage rate and H_f is full-time hours.

APPENDIX C

PROPERTIES OF DIFFERENT PREENROLLMENT CONTROL METHODS

Robert Moffitt

If experimental-control labor supply differences exist at preenrollment, they clearly must be controlled for when estimating the effect of the experiment. We have used two methods of such control. In the "Net" Model, the experimental effect is measured as the change in the experimental-control labor-supply difference from preenrollment to the during-experiment period. In the "Lagged" Model, the experimental effect is measured as the during-experiment experimental-control difference controlling for the preenrollment value of labor supply via regression methods.

The properties of the two estimators can be seen most easily by a simple model of unobserved variables. Unobserved variables must be considered because experimental-control differences at preenrollment must be proxying for such variables, since such differences cannot represent effects of the experiment. In this context, a correlation between treatment and "unobserved variables" merely corresponds to a correlation between treatment and the preenrollment labor-supply error term.

Let y be labor supply, z be an unobserved independent variable, and t be a variable representing generosity of the experimental treatment (i.e., a variable with low values for controls and high values for experimentals). If time period 0 is preenrollment and 1 is postenrollment, the model can be expressed as:

$$y_0 = \alpha z + u_0 \qquad (1)$$

$$y_1 = \alpha z + \beta t + u_1 \qquad (2)$$

with

$$E(u_0) = E(u_1) = E(u_0' u_1) = 0$$

$$E(u_0' u_0) = E(u_1' u_1) = \sigma_u^2$$

and where y_0, y_1, z, t, u_0, and u_1 are n x 1 vectors of observations. The effect of the experiment is β.

If equations (1) and (2) describe the "true" model and if a regression of y_0 on t yields a non-zero coefficient, then it is true by definition that t is correlated with z (that is, this is the way z is defined). Therefore the estimation of equation (2) without z will yield a biased treatment coefficient. The "Net" Model can be thought of as the method of subtracting equation (1) from equation (2); the "Lagged" Model can be thought of as entering y_0 into equation (2) in order to proxy for z.

The main difference between these methods is the nature of the presence or absence of bias. The primary result is simply the following: if t is correlated with z because t is correlated with y_0 -- as would be the case if the allocation model is causing the difference -- then the Lagged Model yields an unbiased treatment effect but the Net Model does not; but if t is correlated with unobserved variables z directly, then the Net Model yields an unbiased treatment effect but the Lagged Model does not. In general, of course, one does not know which of these obtains (or whether both do), for a non-zero correlation between y_0 and t would occur in either case. This is the reason for our estimating both models.

The proof of these statements is straight-forward. In the Net model, the first-difference estimating equation is:

$$y_1 - y_0 = \beta t + u_1 - u_0.$$

If t is correlated with z only, t will be uncorrelated with the error term. If t is correlated with y_0, then it will also be correlated with u_0, yielding a biased treatment coefficient. In the Lagged Model, we use y_0 as a proxy for z by solving equation (1) for z and substituting into equation (2), to obtain:

$$y_1 = \beta t + y_0 + u_1 - u_0.$$

If t is correlated with y_0 directly, t will also be correlated with u_0. However, as Cain (1975) and Goldberger (1972) have pointed out, β will be unbiased nevertheless. In words, this is because the coefficient on y_0 (which will be biased away from 1) "absorbs" all the bias itself. To prove this rigorously, note that the potential errors-in-variables bias in the estimate of the treatment coefficient, b, can be written as the following (see Goldberger, 1964, p. 282):

$$\text{plim}(b) - \beta = -D^{-1}\,(\sigma^2_{y_0}\,\sigma_{tu_0} - \sigma^2_u\,\sigma_{y_0 t}),$$

where $D = \sigma^2_{y_0}\,\sigma^2_t - \sigma_{y_0 t}\,\sigma_{y_0 t}$. Now suppose that we write the t-y_0 correlation in terms of the equation

$$t = \gamma y_0 + e$$

Then $\sigma_{tu_0} = \gamma\sigma^2_u$ and $\sigma_{y_0 t} = \gamma\sigma^2_{y_0}$ and $\text{plim}(b) - \beta = 0$.

APPENDIX D

WAGE PREDICTING EQUATIONS

John Friedmann

TABLE D-1

PREENROLLMENT HOURLY WAGE

REGRESSIONS

	Husbands	Wives	Female Heads
Years of Education (Dummies):			
0-8	-0.32 (.25)	-2.20** (.50)	-1.36** (.36)
9-11	-0.31 (.24)	-2.08** (.31)	-1.18** (.31)
12	-0.08 (.24)	-1.90** (.30)	-1.00** (.31)
Years of Age	0.15** (.03)	0.10 (0.07)	-.06 (.05)
Year of Age Squared	-0.002** (0.0004)	-0.001 (.001)	0.001 (0.001)
Family Size	-0.006 (.025)	-0.076 (.050)	-0.029 (0.039)
Constant	0.79 (.69)	2.25* (1.35)	4.48** (1.02)
R-squared	.04	.30	.06
N	566	148	332

Notes: Standard errors in parentheses

* = Significant at the 90-percent level

** = Significant at the 95-percent level

APPENDIX E

ADJUSTED-MEANS EQUATIONS

Robert Moffitt

and

John Friedmann

The estimation of adjusted experimental-control differences uses the same sample as that discussed in the text; the same set of right-hand-side variables in the X-vector; and both Net and Lagged Models are estimated. The primary differences are (a) that all net-wage and income variables are replaced by a treatment dummy equal to 1 if experimental and 0 if control, and (b) the estimation technique is generalized-least-squares (GLS). Both employment-status and conditional-hours (i.e., hours of workers only) are used as dependent variables, and unconditional-hours results are derived from these two.

The GLS technique is used because of the pooled time-series, cross-section nature of the sample. The error structure assumed is as follows. Let ε_{it} be the error term of the individual i at time period t:

$$\varepsilon_{it} = \mu_i + \nu_{it},$$

where

$$E(\mu_i) = E(\nu_{it}) = 0,$$

$$\begin{aligned} E(\varepsilon_{is}\varepsilon_{jt}) &= \sigma^2_{\mu} + \sigma^2_{\nu} && \text{if } i = j \text{ and } s = t \\ &= \sigma^2_{\mu} && \text{if } i = j \text{ and } s \neq t \\ &= 0 && \text{if } i \neq j. \end{aligned}$$

The error term is composed of an individual-specific component that remains constant over time, and an individual-time-specific component that varies randomly over time. Thus, the error terms for the same individual in different time periods are not independent and have a covariance σ_{μ}^{2}. The quantity $\rho = \sigma_{\mu}^{2}/\sigma_{\varepsilon}^{2}$ determines the fraction of the total error variance that arises from the constancy of μ. Consistent estimates of the regression coefficients are obtained by the two-step procedure of first estimating ρ from the residuals of an ordinary-least-squares regression, and by then using this estimate in the error covariance matrix when computing the regression coefficients.

Table E-1 shows the treatment effects obtained. As discussed in the text, responses are detected for husbands and female heads but not for wives. Table E-2 shows the treatment effects by period. For husbands, the response is largest at the beginning and end of the experiment rather than in the middle. For female heads, the response shows a slightly higher response in mid-experiment, as expected. For wives there is again no response at any period.

TABLE E-1

ADJUSTED MEAN TREATMENT DIFFERENCES

	Husbands		Wives		Female Heads	
	Net	Lagged	Net	Lagged	Net	Lagged
Employment Status	-.042**	-.023	.004	.003	-.055**	-.053**
Conditional Hours Per Month	-2.63**	-.44	3.03	-.53	-3.47	1.11
Unconditional Hours Per Month[a]	-9.53	-4.24	1.18	.32	-9.35	--8.23

NOTES: * - Significant at the 90 percent level

** - Significant at the 95 percent level

[a] Calculated from coefficients and means for employment status (ES) and conditional hours (CH) as

$$\overline{CH} \cdot \frac{\partial ES}{\partial T} + \overline{ES} \cdot \frac{\partial CH}{\partial T}$$

where T is a treatment dummy. No significance levels are shown.

TABLE E-2

ADJUSTED MEAN DIFFERENCES BY PERIOD

	Husbands		Wives		Female Heads	
	Net	Lagged	Net	Lagged	Net	Lagged
Employment Status:						
Period 1	-.039*	-.016	-.006	-.014	-.045**	-.047**
Period 2	-.034	-.017	-.012	-.004	-.065**	-.059**
Period 3	-.028	-.007	-.000	-.003	-.056**	-.051**
Period 4	-.030	-.014	-.002	.002	-.056**	-.051**
Period 5	-.061**	-.052**	.007	.011	-.054**	-.050**
Period 6	-.062**	-.034	.021	.017	-.062**	-.062**
Period 7	-.055**	-.035	.027	.021	-.049**	-.049**
Conditional Hours:						
Period 1	-2.64*	-1.08	3.50	-1.06	-3.04	1.52
Period 2	-2.65*	0.84	2.83	3.37	-3.57	-0.28
Period 3	-2.56	-0.56	0.08	-9.10	-2.84	2.64
Period 4	-2.46	0.20	1.30	-2.03	-2.06	1.20
Period 5	-2.71*	-0.06	3.60	0.89	-3.31	1.61
Period 6	-2.59	-1.22	5.81	1.87	-4.94	-0.31
Period 7	-2.86*	-1.62	5.14	1.53	-5.01	1.02

NOTES:

* - Significant at the 90-percent level

** - Significant at the 95-percent level.

APPENDIX F

FULL REGRESSION RESULTS:

BASIC MODEL

TABLE F-1

MEANS OF VARIABLES USED IN THE ANALYSIS[a]

	Husbands	Wives	Female Heads
H	148.50	20.98	31.61
(-Wt)D	-0.58	-0.13	-0.42
$B_0D/100$	1.37	0.34	1.14
W(1-r)	3.14	1.74	1.99
N/100	0.75	5.85	1.29
D	0.82	0.83	0.87
(-Wt)	-0.70	-0.16	-0.48
$B_0/100$	1.68	0.44	1.33
NADULTS	2.53	2.57	1.51
NKIDS	3.31	3.25	2.83
KIDS0-2	0.31	0.30	0.28
KIDS3-5	0.41	0.40	0.33
KIDS6-12	0.73	0.72	0.63
KIDS13-15	0.49	0.49	0.40
MULTFAM	0.03	0.03	0.08
SUMMER	0.40	0.40	0.33
UNEMP	4.58	4.58	4.70

[a] NADULTS = Number of adults in family
NKIDS = Number of children in family
KIDS0-2 = Presence of children, age 0-2 (1 if any, 0 if none)
KIDS3-5 = Presence of children, age 3-5 (1 if any, 0 if none)
KIDS6-12 = Presence of children, age 6-12 (1 if any, 0 if none)
KIDS13-15 = Presence of children, age 13-15 (1 if any, 0 if none)
MULTFAM = Dummy for presence of smaller subfamily unit in household (1 if present, 0 if not)
SUMMER = Dummy for summer month (1 if summer, 0 if not)
UNEMP = Gary SMSA monthly unemployment rate.

TABLE F-2

COEFFICIENTS ON OTHER VARIABLES IN BASIC MODEL[a/]

	Husbands	Wives	Female Heads
D	5.28	0.71	-1.71
	(1.00)	(0.20)	(0.48)
(-Wt)	-2.08	-22.60	-11.40
	(0.23)	(1.29)	(1.32)
$B_0/100$	2.09	11.12*	-2.52
	(0.58)	(1.81)	(0.87)
NADULTS	3.64***	0.97	-4.14***
	(2.91)	(1.11)	(4.24)
NKIDS	-2.42***	-3.72***	-1.84**
	(2.57)	(4.53)	(2.31)
KIDS0-2	0.58	-3.09	-17.19***
	(0.19)	(1.22)	(7.63)
KIDS3-5	-3.13	2.71	-13.21***
	(1.12)	(1.21)	(6.37)
KIDS6-12	6.93***	-0.77	-8.01***
	(2.10)	(0.32)	(3.60)
KIDS13-15	11.65***	7.63***	2.84
	(3.84)	(3.28)	(1.35)
MULTFAM	5.33	8.53*	-10.78***
	(0.78)	(1.84)	(3.36)
SUMMER	7.53***	1.67	-3.92**
	(2.96)	(0.87)	(2.24)
UNEMP	-2.40	-0.83	-2.50*
	(1.12)	(0.52)	(1.89)
CONSTANT	158.69	42.47	23.80

NOTES: All coefficients evaluated at mean of Tobit index. Unsigned t-statistics in parentheses.

*Significant at the 10-percent level.
**Significant at the 5-percent level.
***Significant at the 1-percent level.

[a/] Coefficients on (-Wt)D, B_0D, W(1-r) and N are shown in Table 2. See notes to Table F-1 for variable definitions.

APPENDIX G

ESTIMATION OF A LOCAL-TAX-RATE MODEL

As an additional test of the sensitivity of the results to the budget-constraint parameterization, a "local-tax-rate" model similar to that estimated by Keeley et al. (1978) was also estimated. In this model, hours of work is considered to be a function of the change in the net-wage rate and the change in disposable income at the individual's preenrollment hours of work. Hence only the "local" marginal tax rate is specified; other tax rates in the budget contraint are ignored. The specification is the following:

$$H_e = a + b(-Wt_e) + c\,B_p + d\,(Wt_p) + e(A_e - A_p) + fX + gH_p,$$

where

H_e = during-experiment hours

W = preenrollment wage rate

t_e = NIT tax rate

t_p = preenrollment, non-NIT tax rate

B_p = NIT benefit at preenrollment hours

$A_e - A_p$ = Change in AFDC benefit for female heads (0 for controls, $-A_p$ for experimentals)

X = Vector of other variables

H_p = Preenrollment hours.

Tobit estimates of coefficients b and c are shown in Table G-1.

TABLE G-1

ESTIMATES OF LOCAL-TAX-RATE MODEL

	Husbands	Wives	Female Heads
Net-Wage Effect	-1.47	-8.83	-4.60
	(.28)	(1.45)	(1.08)
Income Effect/100	-1.30	-1.82	0.32
	(.50)	(.73)	(.17)

NOTE: t - statistics in parentheses

As the table shows, much weaker income and net-wage effects are obtained here than were in the basic model in the text. No coefficient is significant at the 90-percent level, husbands' coefficients are again both negative but smaller in magnitude, wives now have a negative income effect but also a negative net-wage effect, and female heads have a near-zero income. Given the implausibility of these results and the defect of specifying only the local tax rate, we put little faith in these estimates. Recall that both the basic model of this paper and the model of Burtless and Hausman give identical, more sensible results and are based upon all tax rates in the budget constraint.

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