STAFF PAPER
SP-78A-06

ESTIMATING LABOR SUPPLY DISINCENTIVES OF A NEGATIVE INCOME TAX: SOME RESULTS AND LESSONS FROM THE EXPERIMENTS*

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December, 1978

*This paper is a revised version of a paper presented at the meetings of the American Economic Association in New York, December 1977. The authors would like to thank Milton Friedman and Guy Orcutt for comments at those meetings. The authors of course bear all responsibility for errors.

ABSTRACT

This short paper reviews the basic findings of the negative income tax experiments and discusses the modeling difficulties encountered in estimating the labor supply response to tax rates. This discussion shows clearly that experimentation per se is not a substitute for advances in the modeling of behavior. However, the advances that have been made in experimental analyses should have wide applications in the analysis of all tax and transfer programs.

ESTIMATING LABOR SUPPLY DISINCENTIVES OF A NEGATIVE INCOME TAX: SOME RESULTS AND LESSONS FROM THE EXPERIMENTS

One of the most important areas of recent research in the economics of labor supply is the analysis of an experimental negative income tax (NIT). The most well-known of the NIT experiments was the initial study in New Jersey and Pennsylvania, but there have been three others in the U.S.[1/] Now that many major analyses have been completed on these experiments, it seems appropriate to evaluate the results.

This paper has two primary purposes. One is to report the basic work disincentives that were found in the experiments. The results of the New Jersey experiment--namely, that the disincentives seemed to be relatively small--are rather well-known, but the results from the others are not. The other purpose is to review the wide variety of labor-supply models used in analyzing the data. Models of the response to NIT tax rates and guarantees have proven very difficult to estimate, creating one of the main stumbling blocks to fully understanding the experimental NIT response. A number of different models have been developed to estimate the response, many of which have not been published and/or circulated widely, but as yet no fully satisfactory solution is available. However, the work that has been done has important implications, not only for estimating the labor supply response to an NIT, but for the analysis of the work disincentives of virtually all tax rates and transfer programs, such as the income tax, wage subsidies, employment

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[1/] The Rural experiment was conducted in selected counties in Iowa and North Carolina, and urban experiments took place in Gary and Seattle-Denver. In addition, a Canadian NIT experiment is in progress in Manitoba.

tax credits, the payroll tax, unemployment insurance, social security benefits, etc. Thus the spillover benefits from the NIT research are potentially large.

A secondary purpose of the paper is to address the question of the relative value of experimentation as a research methodology in this area, for the value of the experimental method depends upon the degree to which it gives us different and better estimates of NIT effects than nonexperimental data can provide. Thus we will preface our review of results of the experiments with a brief discussion of the relative advantages and disadvantages of experimentation, and will later make observations on the experimental results in this light. Our main point in this regard is that although experimentation does provide great statistical advantages by its very nature, it is not a substitute for modeling advances and it cannot circumvent most of the modeling problems encountered in nonexperimental analyses. In fact, it is rather ironic that the difficulties that have been encountered in modeling the experimental NIT generally have nothing to do with experimentation per se, and are difficulties that could just have easily (and should have) been addressed by nonexperimental analyses. Indeed, we assert that proper analysis of the experimental data requires the modeling of nonexperimental tax and transfer programs.

The first section below discusses the relative value of non-experimental and experimental data. The second section presents the basic results of the four experiments, and the third section discusses the models that have been developed to analyze the data.

I. THE ADVANTAGES OF EXPERIMENTAL DATA

The value of experimental data lies in its potential to give us better estimates of price and income elasticities than nonexperimental data do. Nonexperimental data inherently suffer from the possibility of unobserved-variables bias, simply because the observed variation in right-hand-side variables is rarely completely exogenous. The true labor-supply function can be written as $H=H(X; \eta) + \varepsilon$, where H is hours worked, X is a vector of observed price and income variables and measured taste variables, η is a vector of unobserved variables, and ε is a random error term originating from labor-market disturbances and other random events. Cross-section data generally contain nonzero correlations between η and X, and time-series data may (in addition) contain nonzero correlations between ε and X.[1/] These problems are part of the reason for the dismayingly wide ranges of income and price elasticities estimated in the labor supply literature. For example, in a survey of a number of such studies by Cain and Watts (1973), substitution elasticities ranged from 0 to 0.50 for males and from 0.2 to 1.2 for females; total income elasticities ranged from -0.7 to 0 for males and from -2.1 to -0.1 for females; and uncompensated wage elasticities ranged from -0.45 to 0.25 for males and from -0.5 to 0.9 for females.[2/]

[1/] Cross-section data may also of course contain nonzero X and ε correlations, but labor-market disturbances are usually more randomly distributed across individuals than across time periods.

[2/] Female heads and wives were usually not separated in these studies.

These ranges imply that an NIT with a 50 percent tax rate and a guarantee level set equal to the poverty line would reduce male labor supply by anywhere from 0 to 60 percent and female labor supply by 13 to 100 percent. Obviously, this range of predicted outcomes is too wide to be useful to policymakers.

Experimental data, by definition, overcome an important aspect of the unobserved-variable problem: by randomizing eligibility for an NIT, unobserved taste variables are equally distributed between experimental and control groups; and by following both groups over time, labor-market disturbances and random shocks affect both groups equally. Thus such unobserved variables will be uncorrelated with the NIT price and income variables. In other words, experimental data gives the analyst a right-hand-side variable (of policy interest) whose source of variation is truly exogenous.

On the other hand, experiments are not free of problems themselves. Foremost among these is the limited duration of the experiments--generally only three or five years long. However, this problem can be overemphasized, for (1) the basic result of Metcalf (1973) shows that the direction of bias is indeterminate becasue of two offsetting biases (substitution effects are likely overestimated as individuals substitute present leisure for future leisure, while income effects are underestimated); and (2) Metcalf's (1974) empirical work showed the bias to be small--2 to 6 percent in the substitution effect and 8 to 27 percent in the income effect. Additional evidence on the importance of this problem should be forthcoming from the one experiment that varied the experimental duration (in Seattle and Denver). In any case, proper specification of the labor supply

response in a life-cycle model should allow the limited-duration problem to be circumvented without sacrificing the advantage of having exogenous price and income variables.

There are a number of other problems in experiments, none of which are fatal to experimental analysis. Attrition from the experiment can bias the results if attrition is related to experimental payments; Hausman and Wise (forthcoming) have developed a method with which the bias can be estimated, and, in their application to the Gary data, the bias turned out to be small. Also, the selection of a sample truncated by income can also bias the results, but there are again straightforward methods of correcting for such bias (Hausman and Wise, 1976). In any event, these problems are just as omnipresent in nonexperimental data sets as experimental and do little to lower the relative value of experimental data.[1/]

[1/] Space constraints do not allow us to discuss other problems: the use of a sample allocation model that stratifies the sample by income level; the generalizability of the findings; the Hawthorne effect; and others. See Moffitt and Kehrer (1977).

II. MEAN EXPERIMENTAL-CONTROL DIFFERENCES

Table 1 shows the basic labor-supply results of the four U.S. NIT experiments. These results were obtained from equations of the general form $H = H(E, X) + \varepsilon$, where H is hours worked, E is an experimental-control dummy variable, ε is an error term, and X is a set of exogenous variables controlling for age, race, preenrollment labor supply, and other characteristics. The numbers in the table are the experimental responses expressed as a percentage of the mean hours worked of the control group in each experiment. The average tax rate across all four experiments was around 50 percent and the average guarantee was about equal to the poverty line.

There are two notable features of the results. One is that the work disincentives implied by the figures are substantially smaller than predicted from the nonexperimental studies cited earlier, suggesting a considerably more inelastic response than expected. The maximum responses are 7 percent, 27 percent, and 30 percent for husbands, wives, and female heads, respectively. Even the 30 percent reduction for female heads in the Gary experiment only equals 2 hours per week in absolute magnitude.

The other notable feature is a surprising similarity in the results across experiments. The experimental-control differences for husbands range from 2 percent to 7 percent, a very narrow range compared to those obtained from nonexperimental studies (0 to 60 percent). For wives, excluding the anomalous Gary finding of a positive (but statistically insignificant) response of 5 percent, the disincentives range from 17 percent to 27 percent, a wider spread than for husbands but still

TABLE 1

LABOR SUPPLY RESPONSE TO THE NIT:

PERCENTAGE EFFECTS ON HOURS WORKED

IN THE INCOME MAINTENANCE EXPERIMENTS[a/]

	Husbands	Wives	Female Heads
New Jersey-Pennsylvania	-2	-24	--[b/]
North Carolina-Iowa (Rural)	-3	-27	--[b/]
Gary	-7	5	-30
Seattle-Denver	-6	-17	-12

[a/] Experimental response as a percent of hours worked by control families.

[b/] The New Jersey-Pennsylvania and Rural experiments were not designed to provide labor supply response estimates for female heads of household.

Sources: New Jersey: U.S. HEW (1973, pp. 22-25), weighted average of all races (blacks, whites, and Spanish-speaking). Rural: U.S. HEW (1976, pp. 23-29), wage workers only, all races (blacks and whites). Gary: Friedman (forthcoming), only blacks in experiment. Seattle-Denver: U.S. HEW (1978, p. 13), all races (blacks, whites and chicanos).

narrower than comparable nonexperimental ranges (13 to 100 percent). The disincentive effects for female heads, 12 and 30 percent, also fall into a similar range. Thus, these figures seem to represent a considerable reduction in the range of our uncertainty about the labor supply effects of an NIT with a tax rate of 50 percent and a guarantee equal to the poverty line.[1/]

On the other hand, there are some vexing aspects of these and other results obtained in the experimental studies. One is the narrowness of the range of estimates despite large differences across experimental sites in the nature of the local labor markets, the presence and generosity of local welfare systems for the control group, differences in local labor market conditions and the state of the national economy (the experiments covered different years), and other factors. The similarity in the experimental results seems to imply a marked inelasticity of labor supply with respect to all these variables. This may indeed be the case, but should probably be tested by making more formal cross-experimental comparisons or by combining the data from all four experiments.

Another troublesome result that has emerged from the experimental analyses is a considerable instability in the results when they are estimated by experimental plan. Each of the experiments tested several NIT plans with different tax rates and guarantees (dispersed around the means referred to before), with random assignment of subgroups

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[1/] Of course, the wide nonexperimental ranges may be narrowed if one attempts to discriminate among the various studies (Heckman and Borjas, 1978). Also the experimental results are arguably wider than Table 1 implies, for the overall disincentives sometimes mask potentially important variations between subgroups. For example, the New Jersey and Rural reductions (but not those in Seattle-Denver) were different for the different races involved.

of experimental families to each plan. When the mean labor supply differences between plans are estimated, tax and guarantee effects can be inferred. Although the numerous results from all the studies are difficult to summarize, the overall impression they convey is of generally insignificant and/or effects in unexpected directions. Tax effects are often insignificant (Watts and Rees, 1977; U.S. HEW, 1976 for black males in North Carolina and white males in Iowa; Moffitt, 1978b) and sometimes statistically significant but in an unexpected direction, such as a positive effect of the tax rate on labor supply (Cain et al., 1977; U.S. HEW, 1976 for Iowa male earnings).[1/] Guarantee effects are also frequently insignificant (Watts and Rees, 1977; U.S. HEW, 1976; Moffitt, 1978b).

Although one could reasonably conclude that the true tax and guarantee elasticities are indeed near-zero, this conflicts with the picture given by the overall experimental-control differences of a fairly stable (but small) response to an NIT. Or one could hypothesize that the sample sizes are simply not large enough (with the exception of the Seattle-Denver experiments) to generate significant coefficients, although one would not expect on this basis to find tax or guarantee effects of the unexpected sign that are statistically significant. But perhaps the strongest argument is that the estimates fail to adequately control for other tax rates and guarantees faced by the families, such as those of the existing welfare system, social insurance programs such as social security and unemployment insurance, or even

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[1/] Of course, a positive correlation between the tax rate and labor supply is not necessarily "wrong," since the income effect may outweigh the substitution effect.

the tax rates imposed by federal state income and payroll taxes. These other programs could confound the interpretation of both simple mean differences and estimates of tax and guarantee effects.

Still another troublesome aspect of the interpretation of the results in Table 1 relates to the population to which they pertain. Rather than enrolling random samples of the entire population, the experiments enrolled families at the lower end of the income distribution. However, significant numbers of middle-income families were often enrolled in the process--families who were not initially receiving payments and who would not be expected to respond as strongly as lower-income families. Thus the estimates in Table 1 are probably greater than the aggregate disincentives for the entire population, but smaller than those for the very poor subpopulation.

Much, if not most, of the labor supply research on the experiments has been addressed to one or more of these problems, especially the two concerning the estimation of tax and guarantee effects among experimental plans and the estimation of the response of middle-income families.[1/] In fact, it turns out that these two problems are analytically identical when they are stated more formally in a labor supply model, for both are problems involving the estimation of utility-maximizing labor supply choices in the presence of nonlinear budget constraints. However, estimation in this situation has also turned out to be very difficult, and has led those working with experimental data far afield

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[1/] There has been little cross-experimental work that could address the first question discussed above. However, construction of a cross-experimental data file is currently underway at Mathematica Policy Research which will make the analysis of site differences in the response possible.

from issues intrinsically related to experimentation. One of the ironies of the experiments is that their central analytic problem is essentially unrelated to experimentation per se, and could have been analyzed with nonexperimental data.

III. LABOR SUPPLY MODELS OF TAX-RATE RESPONSE

Models for the estimation of tax-rate and guarantee effects and the estimation of the response of middle-income individuals are difficult to develop because both involve estimating utility-maximization solutions in the face of nonlinear, nonconvex budget constraints. In the absence of tax and transfer programs, utility $U(L, Y)$ is a function of leisure hours (L) and disposable income (Y), and is maximized subject to a linear constraint $Y = W(T - L) + N$, where T is total hours available, W is the hourly wage rate, and N is unearned income. In the presence of an NIT that pays a benefit equal to $G - tY$, where G is the guarantee level and t is the tax rate, the budget constraint consists of two segments:

$$Y = W(T - L)(1 - t) + [G + N(1 - t)], \text{ if } Y \leq G/t$$

$$= W(T - L) + N \quad , \text{ if } Y \geq G/t.$$

The budget constraint is shown in Figure 1 as $\overline{CDB}$, where the point D (the "breakeven point") determines the kink at which NIT benefits are zero. The nonconvexity of the constraint clearly implies that local maxima need not be global maxima. Thus if middle-income families who are located above the breakeven point are enrolled in the experiment, the estimation of their response requires modeling their choice of budget-line segment.

Controlling for other tax and transfer programs facing families in the experiment is essentially the same problem. Control families face the AFDC (Aid to Families with Dependent Children) program, which has the same formal structure as the NIT, and thus creates a nonconvex budget

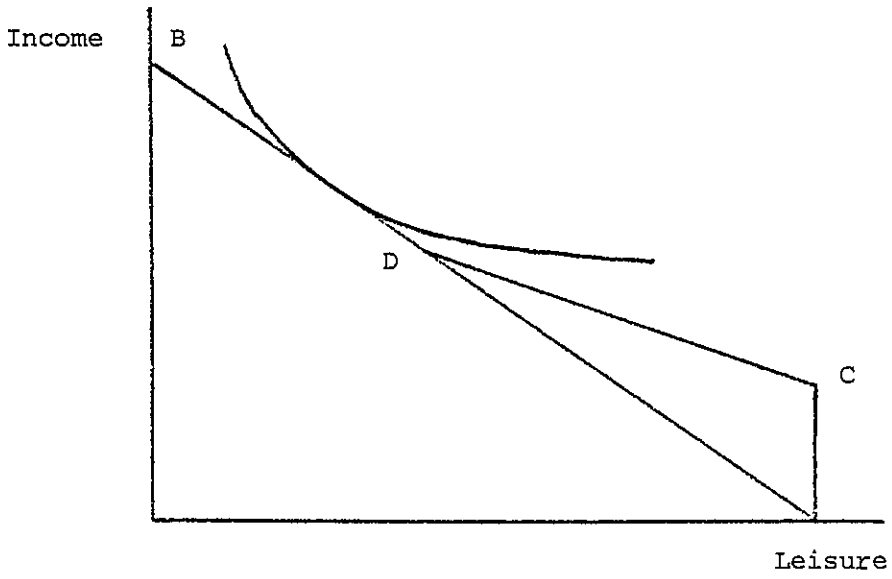

Figure 1.

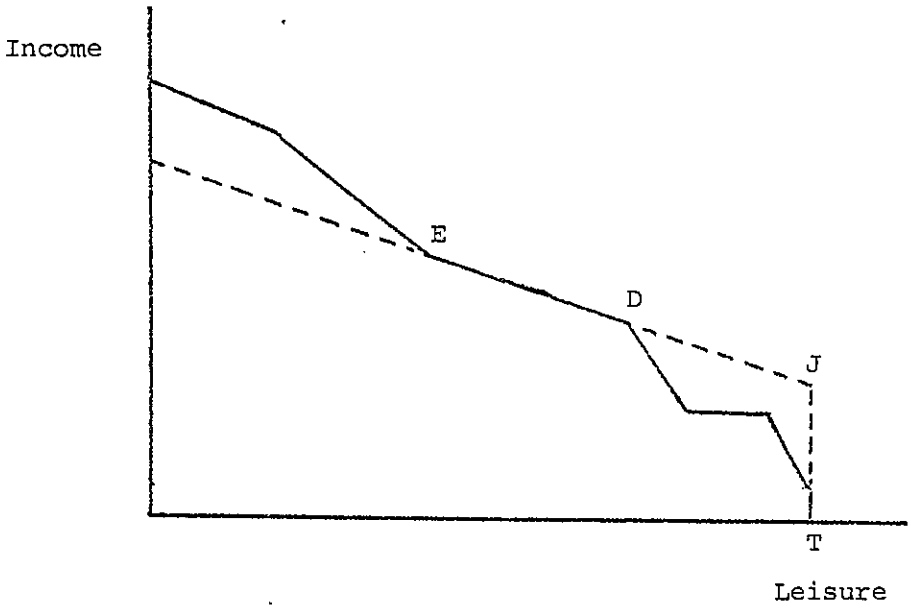

Figure 2.

constraint.[1/] Likewise, unemployment insurance and social security benefits contain positive guarantees and substantial tax rates below certain breakeven points, thus creating the same problem. The payroll tax also creates a nonconvex budget constraint, since the tax rate falls to zero beyond a certain level of earnings. The federal income tax creates convex budget constraints that are kinked, because the tax consists of a series of tax brackets with increasing marginal tax rates.

In general, an individual confronted with several tax and/or transfer programs at the same time faces a series of segments such as those portrayed in Figure 2. Each segment i is associated with a marginal tax rate, t_i; a net wage, $W(1 - t_i)$; and an (imputed) intercept N_i at zero hours of work (e.g., segment $\overline{DE}$ has N_i equal to $\overline{TJ}$). Thus the hours of leisure function can be written in general form as

$$L = L[W(1 - t_1), N_1, W(1 - t_2), N_2, \ldots, W(1 - t_n), N_n] \qquad (1)$$

for a budget constraint with n segments. Thus equation (1) must be estimated to determine the NIT response of middle-income families and to disentangle NIT tax-rate and guarantee effects from those of other tax rates and guarantees facing the families.

The difficulty in estimating equation (1) lies partly in the large number of segments faced by many low-income families, but mostly in the

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[1/] In fact, some experiments allowed families in the experimental group to receive AFDC, but not at the same time as NIT payments. Since the NIT payments were generally (though not always) higher than AFDC payments, this was not a major problem except perhaps in the New Jersey experiment.

obvious nonlinearities in the function, for the kinks and nonconvexities in the budget line clearly create nonlinearities and discontinuities in the labor supply function. Unfortunately, the estimation problem involved has not been addressed in the nonexperimental literature until recently (Hall, 1973; Heckman, 1974; Rosen, 1976; Wales, 1973; Wales and Woodland, forthcoming), although the problem is just as clear in the case of the income tax, unemployment insurance, social security benefits, and AFDC as it is for the NIT. Thus the (by-now) substantial literature on the problem exists almost entirely in experimental studies.

There are three main approaches to the problem that encompass most of the experimental studies attempting to address the problem. One approach is to assume a particular functional form of the (direct or indirect) utility function and a particular distribution of the error term, and to estimate the unknown parameters by the method of maximum likelihood. A second approach is to use some type of two-stage or instrumental-variables procedure to estimate the probability of being located on a particular segment, and to then estimate the value of hours worked conditional upon the choice of segment. A third approach is simply to smooth out the budget constraint by averaging the marginal tax rates along it.

The first approach (Burtless, 1977; Burtless and Hausman, forthcoming) is by far the most consistent with the economic theory of labor supply choice, but has the disadvantage of requiring faily restrictive assumptions to be tractable. Taking each linear segment along the budget constraint in turn, suppose that the indirect utility function is $V[W(1 - t_j), N_j]$,

giving the maximum utility along the segment with slope $W(1 - t_j)$ and intercept N_j. Then the probability that an individual will locate on segment k is

$$\text{Prob}\left\{V[W(1 - t_k), N_k] > V[W(1 - t_j), N_j]\right\}, \quad j \neq k. \tag{2}$$

If a functional form of V is assumed, and if at least one of its parameters is given a stochastic specification, a functional form for these probabilities can be described and used to state the probability of observing an individual on each segment. The unknown parameters can then be estimated by maximizing a likelihood function equal to the product of these probabilities.

The simplifying assumptions necessary to make the problem tractable are illustrated by the work of Burtless and Hausman. Only two segments are assumed for the experimental group and the control group, although more in fact exist. Also, a strongly separable indirect function is used, based upon a constant-elasticity labor supply function that does not allow zero hours of work.

A second approach to the problem, which gains tractability but loses some theoretical appeal, is to use some type of instrumental-variables approach to estimate the labor-supply function <u>conditional</u> upon choice of segment. Suppose that the endpoints of segment j are at hours of work H_j and H_{j+1}. Then the conditional labor-supply function is

$$H = H[W(1 - t_j), N_j] + \varepsilon, \text{ if}$$
$$H_j < H[W(1 - t_j), N_j] + \varepsilon < H_{j+1}. \tag{3}$$

If it were not for the error term, the function could be estimated with least squares by assuming each individual observation is at his or her

utility-maximizing point and by estimating (3) using the values of $W(1 - t_j)$ and N_j of the segment upon which the individual is located. However, given the unavoidable stochastic nature of the problem, resulting from both heterogeneous preferences and random error, such estimates will be biased, for t_j and N_j will be correlated with H (and hence ε) through the tax formula, making $W(1 - t_j)$ and N_j endogenous. In the approach under discussion, some type of two-stage approach is used instead by developing instrumental variables for t_j and N_j.

In most of the studies, instruments for t_j and N_j are developed by using the t_j and N_j corresponding to an individual's <u>preenrollment</u> hours of work. Since such t_j and N_j are predetermined, they are assumed to be uncorrelated with the contemporaneous hours-equation error term. If there is any correlation between preenrollment and postenrollment error terms, however, such instruments will not generate unbiased coefficients.[1/] The most direct example is the work on the Seattle-Denver experiment by Keeley et al. (1978), who estimated a variant of equation (3) and evaluated $W(1 - t_j)$ and N_j at an individual's preenrollment hours of work. They also entered separate right-hand-side (RHS) variables for whether the individual was above the breakeven point at preenrollment, thus requiring the same assumption. Using a less formal specification of the budget constraint, numerous of the earlier studies of the New Jersey and Rural experiments used a similar approach by entering such a preenrollment

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[1/] A different tack altogether was taken by Hausman and Wise (1976), who used New Jersey data and followed the approach of Rosen (1976) by evaluating $W(1 - t_j)$ and N_j at a <u>fixed</u> hours of work.

breakeven-status (i.e., dummy) variable, or some function of it, on the RHS of the labor supply equation (e.g., Watts and Rees, 1977). In a way, this can be thought of as partially stratifying the sample at a prior point in time according to which segment observations are on at preenrollment. In further attempts to remove any endogeneity of the tax rate and guarantee variables, Kerachsky and Mallar (1976) estimated probabilities of being below breakeven and of being on AFDC for the New Jersey experiment, and used predicted probabilities on the RHS; and Robins and West (1978) similarly used a predicted breakeven probability on the RHS in their Seattle-Denver work.[1/]

Aside from the question of how well the endogeneity of the RHS variables is removed in these studies, there are clear conceptual problems involved, usually recognized by the authors. First, it is obvious that the "choice of segment" and the "choice of hours along a segment" are not separate decisions, but part of a single decision to pick a point on the entire budget constraint. Second, even if the two choices could be estimated separately, estimation of equation (3) is not enough to make predictions; the equation predicting "choice of segment" is also necessary. Yet the latter equation is often not reported, and is usually (when estimated) ad hoc and not based upon as rigorous a theory as equation (2). Thus

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[1/] These approaches contrast strongly with the New Jersey study of Cogan (1978), who evaluated the t_j and N_j for the NIT and AFDC at an individual's contemporaneous hours of work and used no instrument at all. Thus his estimates suffer from an endogeneity bias. As one would expect, his disincentive estimates are larger than those in the above studies because those individuals with high tax rates and guarantees are exactly those with low hours of work (i.e., below the breakeven point); given the nature of the tax structure (including transfer programs), individuals with low hours of work face higher tax rates even if there is no experimental response. That is, individuals with lower hours of work would have faced higher NIT tax rates even at preenrollment.

the approach must be considered as an empirical approximation to only part of the total labor supply function.[1/]

A third approach that must also be viewed as an empirical approximation is that of Moffitt (1978a, 1978b), who simply smoothes the budget line by averaging the tax rate, estimating $H = H[W(1 - \bar{t}), Y_0)$, where $\bar{t}$ is the average marginal tax rate and Y_0 is income at $H = 0$. The average tax rate is calculated by weighting the marginal tax rate in each segment by the fraction of hours worked covered by the segment. This approach has the advantage of estimating the "choice of segment" and "choice of hours along a segment" all in one step without any artificial separation, but has the clear disadvantage of only approximating the budget constraint and (hence) the labor supply function. Thus, like the other two approaches, this approach makes a tradeoff between empirical flexibility and theoretical rigor.

Interestingly, however, Moffitt's study is the only one to estimate separate income and substitution elasticities for NIT income, AFDC, earnings, and taxes. It thus provides the only available direct evidence on the difference between experimentally-estimated and non-experimentally-estimated elasticities. His results on Gary data indicate that, although experimental NIT elasticities are somewhat similar to those for AFDC, they are significantly different and (hence) not predictable from wage and

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[1/] An alternative approach that may be relevant is to follow the model of Heckman (1976), in which the component of the error term in equation (3) which causes endogeneity bias is estimated with probit analysis, and is then used in equation (3) to obtain initial consistent coefficient estimates. In the present case, the probability of locating on a segment would have to be estimated in a multichotomous choice model.

unearned-income elasticities, thus suggesting that most previous studies--which have constrained the coefficients on W and -Wt to be equal--may have given erroneous results. In any case, an important topic for future research is the investigation of the causes of such coefficient differences, and hence the determination of whether the experimental or nonexperimental coefficients are more accurate.

IV. SUMMARY

The relative value of experimentation in the estimation of NIT work disincentive effects should be judged by how well they improve our estimates over those based on nonexperimental data. The income maintenance experiments have indeed improved our knowledge of the response to an NIT with a 50 percent tax rate and a guarantee level about equal to the poverty line, but our knowledge of the response to other potential plans has not been greatly improved. This is because the experiments have been no substitute for advances in techniques to analyze labor supply in the presence of tax and transfer programs. Rather severe estimating problems in this situation have given rise to a number of approaches, none of which is both theoretically ideal and empirically flexible. Clearly, better models are needed. However, the work that has been done to date should have spillover benefits in the analysis of tax and transfer programs.

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