The Labor Supply Response of
Wage Earners in the Rural
Negative Income Tax Experiment *

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* This paper was written during my stay at the London School of Economics and I am indebted to Bettie Jory and Sally Cooper for assistance, and to Richard Layard for very helpful conversations. I am also indebted to Lee Bawden and James Heckman for their comments on an earlier draft and to Lee Bawden for his extraordinary help in the analysis of the data contained herein. An earlier version of the paper was presented at a conference on the Rural Income Maintenance Experiment sponsored by the Brookings Institution on January 13-14, 1977.
An interesting party game for economists is to question whether anyone can think of a behavioral economic response about whose empirical magnitude there is wide agreement. My candidate for such an empirical relationship is always Engel's law, but the fact that the share of a family's income spent on food declines with income probably never drew much controversy and certainly does not do so now. Measuring labor supply behavior, on the other hand, is now big business and I can think of more than one organization whose capital value would shrink if the subject became moribund. Although there is hardly a consensus on the magnitude of the income and substitution effects in the average labor supply function, in looking over the results of the Rural Negative Income Tax Experiment one cannot help feeling that there has been considerable scientific progress over the last few years in the determination of the behavioral responses to the wage and unearned income changes that are induced by such a program. The difficulty now is to integrate this information into a consistent framework so that conflict and agreement amongst its various parts can either be reconciled or, where it is genuine, highlighted. Given the diversity of data, the growth in econometric methods to deal with this diversity, and the tendency for different investigators to highlight different results, this is a formidable task.

Since interest in the effects of a negative income tax has provoked much of the research on labor supply behavior, it is natural to proceed with this framework in mind. My own approach has been to examine the data and analysis of the rural experiment with a view to modifying my prior views about what the effects of such a program were likely to be
on the basis of this new information. These prior views are based primarily on earlier cross-section studies and the results of the Urban Negative Income Tax Experiment in New Jersey and Pennsylvania. Of course, all of this empirical material requires interpretation in the context of some theoretical framework and in this regard I have depended heavily on the usual application of the classical theory of choice to the family's decisions regarding non-market time and consumption goods. Because there may be some confusion regarding precisely who will choose to accept payments under a negative income tax program, and because this issue ended up being treated differently in the design of the Urban and Rural Experiments, I have spelled out the conventional theory of labor supply and what it implies for this choice in Appendix A to this paper.

What Is the Theoretical Framework?

A negative income tax offers a harmful and a beneficial change in the opportunity set that a family faces. The harmful change is the decrease in the wage rate that each family member faces as a result of the implicit tax \( t \) the program places on earnings. The beneficial change is the guaranteed income level \( G \) that the family now has. The family that is offered the opportunity to participate in a negative income tax program presumably will not do so if the harmful effect of participating outweighs the beneficial effect. What this means, of course, is that offering the plan to a family does not necessarily imply that it will participate. It is natural to ask, then, what determines which families will participate.
It is easy to see that the answer to this question must depend on
the preenrollment earnings level of the family. The higher the
preenrollment earnings level the less likely it is that the family will
participate. First, it is clear that all those families with preenrollment
earnings below the breakeven earnings level will be better off participating
in the program. Because these families receive a positive subsidy under
the plan, they can have both higher consumption of goods and/or leisure
by participating. Second, some families with preenrollment incomes above
the breakeven income level may also decide to participate. Although these
families will end up with fewer consumption goods by deciding to participate
they will be compensated for this by additional leisure. At a high
enough preenrollment income level, however, the sacrifice of consumption
goods will eventually become too great. Intuitively, the level of income
above which families will cease to fall into this second group of
participants depends on how strong the family's preferences for leisure
are. Of course, these arguments must be modified to the extent that there
is a stigma attached to the mere act of participating in the program or
to the extent that there are burdens of paperwork or other difficulties
involved in securing participation.

In Appendix A I show that as an approximation and in the absence
of stigma or other burdens, it is rational for a family with only one
worker to choose participation in a negative income tax program if its
preenrollment income is less than the opting-in income level $w_{1l}^O$, where

$$w_{1l}^O = (G/c)[1 - .5et]^{-1},$$

and $e$ is the income-compensated percentage change in labor supplied per

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1 I use the term "preenrollment income" here to mean the income that the
family would have in the absence of being offered participation in a negative
income tax scheme. This would correspond to preenrollment income exactly
only at the very outset of the experiment, and would no doubt be greater
than actual preenrollment income in the later years of the experiment when
wage rates had drifted up.
percentage change in the wage. Where labor is very inelastically supplied and consequently work attachment is great, the opting-in income level is essentially equal to the breakeven income level and only those families whose preenrollment earnings levels would justify positive subsidies will participate. Where labor is very elastically supplied, however, the opting-in income level may be considerably above the breakeven income level.

This issue is illustrated in Table 1, where I have calculated values of the ratio of the opting-in income to the breakeven income for various values of e and t. The top panel takes up the case where only the husband works, while the bottom panel examines the two earner case under the assumption that the wife's earnings are 20 percent of the total. I have chosen values for the labor supply elasticities that, on the basis of my own prior views, would be considered large in order to explore the upper limits of the likely opting-in income. For a fifty percent tax rate it seems that the opting-in income level might be as much as $4 to 10 percent above the breakeven income level. Of course, how many additional participating families this implies for the program depends on the distribution of preenrollment earnings around the breakeven level.

For those families who become participants in the negative income tax program there are then the conventional income and substitution effects on labor supply. On the usual presumption that leisure is a "normal" good, the subsidy resulting from the program operates to reduce labor supply just as does the substitution effect of the induced wage change. When there is only one worker in the family this is the end of the story,
### Table 1
Values of the Ratio of the Program Opting-in Income to the Breakeven Income

#### Husband Works Only

<table>
<thead>
<tr>
<th>Compensated Labor Supply Elasticity ($e_{11}$)</th>
<th>Tax Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>.3</td>
</tr>
<tr>
<td>.15</td>
<td>1.023</td>
</tr>
<tr>
<td>.25</td>
<td>1.039</td>
</tr>
<tr>
<td>.35</td>
<td>1.055</td>
</tr>
</tbody>
</table>

#### Husband and Wife Work

<table>
<thead>
<tr>
<th>Compensated Labor Supply Elasticities ($e_{11}$ $e_{22}$)</th>
<th>Tax Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>.3</td>
</tr>
<tr>
<td>.15 .6</td>
<td>1.037</td>
</tr>
<tr>
<td>.25 .8</td>
<td>1.057</td>
</tr>
<tr>
<td>.35 1.0</td>
<td>1.078</td>
</tr>
</tbody>
</table>
whereas the presence of two workers may lead to a redistribution of these two workers' time as between market work and leisure. The upshot of this is that family earnings must decline for participating families although this may be accomplished without a decline in the hours worked by all family members.

Now one way to think about the purpose of a negative income tax experiment is as an effort to estimate the total transfer costs of adopting such a program for a previously uncovered group. Without knowledge of any behavior at all it is always possible to make a rough first round estimate of the cost of such a program by simply calculating the subsidy that each family would receive based on the known values of preenrollment income. Let us denote by $D^0$ the average subsidy per family calculated in this way. If $N^0$ is the number of families with positive subsidies at preenrollment, then $N^0D^0$ is the first round estimate of the transfer cost of the program.

Clearly the quantity $N^0D^0$ is an underestimate of the eventual total cost of the program for two reasons. First, some families with incomes above the breakeven income level will opt to work less and participate in the program. These families are not counted in $N^0$. Suppose there are $N'$ of such families and that their average subsidy while participants is $X^0$. Accounting for this additional participation then adds $N'X^0$ to the total transfer cost of the program. Second, the average subsidy $D^0$ does not include the induced decline in the earnings of the participants with preenrollment incomes below the breakeven that results from the change in the opportunity set the family faces. This average decline in
earnings multiplied times the tax rate (tAE) will also end up as part of the average subsidy. If N families are then offered the option of participation in the program these two adjustments taken together imply that the ultimate transfer cost of the negative income tax program will be \( ND' = N^0(D^0 + tAE) + N^1Y^0 \). In this framework \( N - N^0 - N' \) families will have opted against participation in the program and the average cost per eligible family is \( D' \).

Next consider designing an experiment to estimate the full transfer cost of adopting a negative income tax plan for a previously uncovered group. A natural way to do this would be to draw a random sample of the entire population to be offered the plan and to implement it with this group. This would provide an estimate of \( D' \) which, when multiplied by the number of eligible participants, would provide an estimate of the total costs of adopting the program. Of course, it would also be possible to estimate the components of \( D' \), which include the induced declines in earnings \( AE \), the number of actual participants with preenrollment incomes above the breakeven level, \( N' \), and the subsidy of this last group, \( X^0 \), by the use of data from some form of control group.

As it turns out, the experimental framework I have just described is the basic arrangement that was apparently used in the Urban Negative Income Tax Experiment. Of course, the population put at risk by the various plans in that experiment was restricted to those husband-wife families who were below 1.5 times the poverty level. This merely means, however, that the estimates of experimental response must be understood to apply only to that segment of the total population that might be eligible for a particular program.
The design of the experimental group in the Rural Experiment was slightly different, however. Primarily because of requirements from the agency sponsoring the experiments, the two plans with the lowest breakeven income levels were dropped from the experimental design and families were assigned to the experimental group only if, on the basis of their preenrollment income, they were expected to receive a positive subsidy. As we have seen, apart from stigma or other burdens, such families should always opt to participate in a negative income tax program.

In fact, as Lee Bawden [6] notes, in only 13 percent of all the quarterly observations were experimental families above their breakeven income levels during any part of the three experimental years. As a result it must be understood that the data from the Rural Experiment are not likely to be of much use in estimating the terms $N'$ or $x^0$ that represent the part of the full costs of a negative income tax plan that result from participation by those families who would otherwise have incomes above the breakeven income level.

To this point I have treated the analysis of a negative income tax experiment just as if it were a permanent negative income tax program and consequently I have depended heavily on the conventional static single-period analysis of labor supply behavior. This would perhaps be acceptable in the analysis of a permanent program, but the experiment was clearly designed to last only three years. In a more general model of labor supply, where families plan their work decisions for the current
period based on the wage rates and unearned income they expect to receive in current and future periods, the transitory nature of the experiment has important implications for the interpretation of the results.

In particular, the measure of the income subsidy that will determine current period labor supply in an intertemporal model is the discounted present value of all future subsidies received during the planning horizon. Thus, a subsidy promised for only three years will be merely a fraction of the discounted present value of a subsidy promised for all future years. It follows that the income effect on labor supply of a transitory program must generally be less than the income effect of a permanent program. How much less will depend first on the length of the planning horizon. If families consider only the opportunities they will face in the near future in making current decisions, then the income effects of permanent and transitory programs will be very similar. In a similar way, high rates of discount (interest) will reduce the importance of future subsidies for current-period labor supply and also tend to make the income effects of permanent and transitory programs more similar.

At the same time, the tax rate in a permanent program causes non-market time to be less expensive relative to consumption goods in both the current and all future periods. The tax rate in a transitory program, on the other hand, causes non-market time in the current period to be less expensive relative to consumption goods in all periods and less expensive relative to non-market time in all future periods.
If non-market time in the current and future periods are substitutes, the transitory program then sets up an additional incentive above that of a permanent program to reduce labor supply during the period of the experiment so as to take advantage of the "fire sale" on leisure the experiment offers. The upshot of this discussion if that it is not possible to determine the effects of a permanent negative income tax program from the results of a transitory experiment without making additional strong assumptions or obtaining additional evidence. Nevertheless, explicit consideration of these issues may be helpful in the interpretation of the actual empirical results of the experiment.

Finally, it is necessary to qualify the analysis of labor supply to the extent that the stated and actual changes in the family's opportunity set differ. For example, there is now considerable evidence that the presumed (and to some extent statutory) tax rates in many actual welfare programs are considerably higher than the effective tax rates actually in existence in many of these programs. Though far from definitive, analyses by Rowlatt [14] for Alberta, Canada, Williams [15] for ten U.S. states and Barr and Hall [5] for nine US cities suggest that these differences may in some cases be very large. Analysing the effects on labor supply of a negative income tax program in the presence of opportunities for the under-reporting of family income to the experimenters raises some difficult issues. In Appendix B I set out a very simple example of such an analysis and this may provide some guidance, but further analysis would clearly be useful.

Perhaps the most disturbing implication of the possibility of systematic income under-reporting is that most of the incentives for such under-reporting
tend to work in the same direction as the purely behavioral responses to a change in unearned income and the tax rate on income that are the result of a fully enforced negative income tax experiment. For example, the presence of the tax rate in a negative income tax program raises the subsidy a family receives by t dollars for each dollar of income that is under-reported by a participating family. However, the gain from income under-reporting does not vary with the size of the guarantee level the participating family faces. It follows that the incentive to under-report income will be greater for participating families and that it will be greater for those families facing high tax rates than for those facing low tax rates. These predictions of the effect of the presence of a negative income tax on "reported" income are very similar to the predictions from a conventional analysis of labor supply, and this is particularly the case for the predictions of the effect of a transitory negative income tax program.

Of course, whether income under-reporting for the purpose of "tax evasion" is of any quantitative importance for the interpretation of the results of the Rural Experiment depends on whether the likelihood of a participating family being caught under-reporting was great and what penalties, psychic or otherwise, were imposed for this behavior. So far as I can determine, this issue is not discussed in the report on the experiment. It seems likely that there were some incidents of under-reporting discovered and it would be useful to have at least a superficial discussion of the experimenters' experience with this issue
and the practical efforts that were taken to cope with it. In the absence of any formal evidence I have simply examined the data on family unearned income that is comparable to the data on family wage income reported in Table 3 below. In a model of tax evasion where labor supply is exogenous there is an incentive to under-report all forms of income regardless of source, while in a model of labor supply it is expected that only earned income would decline. Moreover, it may be difficult for a family to subsequently under-report unearned income that has already been revealed at the time of pre-enrollment, but it is probably not so difficult to simply "forget" increases in such income during the experimental period. As it turns out, reported unearned income among the experimental group increased by some 62 percent during the experimental years, and declined by only 8 percent relative to the control group over the period. Although this is far from conclusive, there does not seem to be any clear evidence of widespread income under-reporting among the wage-earners in the Rural Experiment and I am inclined to the presumption that the data from the experiment reflect primarily behavioral labor supply responses. Of course, if the experimental framework is a mirror image of the way in which the enforcement procedures of a universal negative income tax program would operate, the estimates of the total financial costs of a universal program derived from the experiment, NDI, may still be accurate even if they reflect tax evasion behavior alone or some combination of that behavior and a labor supply response.
What Were the Expected Results?

With the stage set in this way it is a straightforward matter to examine how previous estimates of income and substitution effects based on cross-sectional data as well as the previous results from the Urban Experiment should shape expectations regarding the results of the Rural Experiment. As we have seen, in principle the estimates of behavioral relationships from cross-sectional data could be used to measure the decline in earnings among those families initially below the breakeven level (ΔE) and the proportion as well as earnings response of those families initially above the breakeven level who would opt into the program. In practice this seems rarely to be done. Following the more conventional practice, I have recorded in Table 2 some crude estimates of the expected wage income decline (ΔE) that might have been expected as a result of the Rural Negative Income Tax Experiment. In making these calculations I have assumed a guarantee level of $3200 per year and a tax rate of .5, which seems to be approximately the average plan used in the Rural Experiment. I have chosen to present these results as the proportional decline in earnings that might be expected among the participants with preenrollment incomes below the breakeven. Assuming that cross-substitution effects in family labor supply are negligible, which I do because of general ignorance about these effects and not because of any good evidence, these proportionate changes are the sum of a substitution effect and an income effect. The substitution effect is the negative of the product of the compensated labor supply elasticity for a family member and the tax rate, while the income effect is the negative of the marginal
Table 2
Expected Percentage Effects of a Negative Income Tax Program on Earnings

<table>
<thead>
<tr>
<th>Effect on:</th>
<th>Based on Cross-Sectional Evidence</th>
<th>Based on the New Jersey Experimental Results for Whites</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Rees-Watts Estimates</td>
<td>Hall's Estimates</td>
</tr>
<tr>
<td>Family Earnings</td>
<td>-16.2%</td>
<td>-8% to -12%</td>
</tr>
<tr>
<td>Husband's Earnings</td>
<td>-9.5%</td>
<td>-2.4% to -7.0%</td>
</tr>
<tr>
<td>Wife's Earnings</td>
<td>-45.0%</td>
<td>-45.5%</td>
</tr>
</tbody>
</table>

1 Calculated assuming $e_{11} = .1$, $e_{22} = .8$, $w_1(\delta h_1/\delta y) = w_2(\delta h_2/\delta y) = -.1$, and that the wife's earnings are 13 percent of the total earnings. The guarantee is taken to be $3200 and the tax rate to be .5.

2 Line 1 is taken from Rees and Watts [13], Table 2, results for earnings; while line 3 is from their Table 1 for hours expressed as a percentage of the control group mean. Line 2 is the implied estimate from lines 1 and 3 assuming the wife's earnings are 13 percent of the total.

3 For lines 2 and 3 these results are Hall's [9] GGS and GGLS estimates for hours as percentages of the control groups mean hours. Line 1 is the implied estimate assuming the wife's earnings are 13 percent of the total.
propensity to consume leisure times the subsidy per hour of work
calculated at preenrollment. Of course, it is not possible for any family
member to work a negative number of hours and so I have assumed that the
forty percent of wives that did not work during the preenrollment period
would be unaffected by the experiment.

A crucial component of these calculations is the values assumed for the
compensated labor supply elasticities and the marginal propensities to
consume leisure (or non-market time) from unearned income. In making
the calculations I could not help realizing how arbitrary a task it is
to assign particular values to these behavioral relationships based on
the literally dozens of studies that now exist. The values I have used
reflect two general findings that seem to emerge from most studies. First,
the estimated compensated labor supply elasticities are considerably larger
for wives than for husbands. Second, it seems very likely that the
uncompensated labor supply elasticity for husbands is negative or close
to zero. Although the cross-section studies play a role in establishing
this last finding, I find more convincing the fact that both across areas
or individuals and through time higher real wages tend to be associated
with shorter hours of work. The final values selected and listed in
Table 2 reflect my own crude earlier survey [3] and the later and more
complete survey by Cain and Watts [7]. They imply uncompensated labor
supply elasticities of zero for husbands and .7 for wives.

As can be seen from the table, the application of these cross-sectional
results to the preenrollment data suggest a decline in family earnings of
16.2%. The decline for husbands is expected to be 9.5%, while the decline
for wives averaged over workers and non-workers is 45.0%. It is important to recognize that conceptually these are estimates of the proportionate decline in earnings for those families with preenrollment incomes below the breakeven who are therefore expected to receive actual payments. The response of families above the breakeven is not included.

The second and third columns of Table 2 contain my interpretation of the results for white families from the Urban Negative Income Tax Experiment in New Jersey and Pennsylvania. As best I can determine, the average experimental plan in the Urban Experiment was roughly comparable both to the plan I have used to calculate results based on the cross-section evidence and to the plan used in the Rural Experiment. However, as should be clear from the earlier discussion, the results of the Urban Experiment are estimates of the decline in earnings resulting both from the labor supply reductions of those families initially below the breakeven income level and of those families initially above the breakeven level. In general, this earnings response, averaged as it is over both experimental families who opted to participate and those who did not, will be less than the average response of only those families who were initially below the breakdown income level. One should not be surprised, therefore, to find that the average response in the Urban Experiment is less than would be expected based on the cross-sectional evidence.

The results of the Urban Experiment that I have reported in Table 2 are only those for white families, even though there were both black and white families in the experiment. I have chosen to use only the results for whites for two reasons. First, the results for the groups other than
whites are very peculiar and have been criticized by many for both the unreliability of the data and the problem of attrition. Second, I am told that there has been considerable re-analysis of the data for these groups and that these later results are very much more like those for whites. It is presumably for this reason that many of the reported results of the Urban Experiment by the original analysts seem very much smaller (in absolute value) than those reported in table 2. Bees and Watts [13] suggest an estimate of an overall reduction in family earnings of 6 percent, while Aaron [1] summarizes his view of the results as a 4 to 5 percent reduction in hours worked by husbands and a 25 to 50 percent reduction in hours worked by wives. Some readers may prefer to accept these estimates as preferable to those recorded in table 2.

As can be seen by comparing the results from the cross-sectional evidence and the Urban Experiment in Table 2, the former tends to overstate the latter. Whether this difference can be attributed to the different conceptual bases of the two measures, or to something else, is a topic for future research.
What were the Actual Results?

In a first reading of the experimental results for wage earners it is easy to get lost in the maze of analysis for the various subgroups and specifications of the experimental effect. In order to see what lay behind the analysis I assembled the data in Table 3 on wage income for both the control and experimental groups in the year before and in the three years following the onset of the experiment. These are the annual averages of the data that made up the grist for the components-of-variance regression analysis performed by Lee Bawden and his colleagues. Annual averages are useful in this context because they eliminate the very considerable seasonal fluctuations in the quarterly data for these rural families but still provide some idea of how any experimental effects vary over time.

In Table 4 I have computed some very simple estimates averaged over all three sample groups (blacks and whites in North Carolina and whites in Iowa) of the effect of the experimental treatment on wage income. Wage income seemed the most conceptually appropriate measure of labor supply response, even though from the point of view of the ultimate transfer cost of a negative income tax program family income is actually the variable of interest. In the event, both of these measures gave essentially the same results.

The estimate of experimental response in Tables 4 and 5 is just the arithmetic difference between the change from pre-enrollment to post-enrollment in the wage income of the experimental and control groups. That is, if \( \bar{R}_t^e \) and \( \bar{R}_t^c \) are the average wage incomes of the experimental and control groups in period \( t \), then the measure of experimental response is merely \( \Delta \bar{E}_t = (\bar{R}_t^e - \bar{R}_0^e) - (\bar{R}_t^c - \bar{R}_0^c) \), where the pre-enrollment period is denoted by 0 and \( t = 1, 2, 3 \) refer to post-enrollment. Sample attrition reduces the sample sizes slightly in each consecutive year so that these are 264, 251 and 230 respectively, with about 44 percent of the sample in the experimental group in each year. The overall average response in Table 5 is based on a pooled regression with these data that includes a single dummy variable for presence in the treatment group and separate intercepts for each year of the sample.
### Table 3

**Annual Wage Income of Various Groups**

#### Annual Wage Income of the Family

<table>
<thead>
<tr>
<th>Group</th>
<th>Preenrollment</th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>$4,462</td>
<td>$5,163</td>
<td>$5,694</td>
<td>$6,061</td>
</tr>
<tr>
<td>Experimental</td>
<td>$4,505</td>
<td>$4,702</td>
<td>$5,048</td>
<td>$5,343</td>
</tr>
</tbody>
</table>

#### Annual Wage Income of Husbands

<table>
<thead>
<tr>
<th>Group</th>
<th>Preenrollment</th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>$3,669</td>
<td>$3,931</td>
<td>$4,238</td>
<td>$4,451</td>
</tr>
<tr>
<td>Experimental</td>
<td>$3,882</td>
<td>$3,924</td>
<td>$4,169</td>
<td>$4,412</td>
</tr>
</tbody>
</table>

#### Annual Wage Income of Wives

<table>
<thead>
<tr>
<th>Group</th>
<th>Preenrollment</th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>$633</td>
<td>$858</td>
<td>$954</td>
<td>$1,161</td>
</tr>
<tr>
<td>Experimental</td>
<td>$499</td>
<td>$532</td>
<td>$501</td>
<td>$546</td>
</tr>
</tbody>
</table>
Although such an estimator is crude, there are two strong arguments for its use. First, it is simple to explain. After all, the primary purpose of generating the effects of the labor supply response to a negative income tax program by the use of an experiment is the education of a wide variety of people who come from many different backgrounds. Designing an experiment so this information can be easily conveyed has a lot to be said for it. Second, there is considerable evidence from the regression results on the data from the experiment that there is some correlation between a number of exogenous variables and observed wage income. In effect, the estimator used in Tables 4 and 5 models this problem with a permanent component for each family, a trend component for each time period, and a transitory component. Moreover, in this framework the permanent component is not assumed to be uncorrelated with experimental status. Hence, the effect of the computation of differences is a simple control for unobservable factors that, because of sample attrition or other reasons, might be correlated with the presence of treatment status and hence bias the estimated treatment effects. Of course, whether this is an efficient estimation scheme remains an open question.

When all is said and done it is quite remarkable that the effects of the experiment can be seen clearly even in the raw data contained in Table 3. For example, the average pre-enrollment total wage income levels of the experimental and control families were nearly identical. In the first year of the experiment, however, the average income of the experimental group was about $461 or 9 percent below that of the control group. More systematic but equally simple estimates are contained in Tables 4 and 5. These difference-in-means estimates suggest that, averaged over the three years of the experiment, family wage income declined by 12.3 percent. This was a result primarily of wage income declines for husbands of 7.9 percent
Table 4
Simple Estimates (and Estimated Standard Errors) of the Effect of Participation in the Negative Income Tax Program on Wage Earnings

Effect on Family Wage Earnings in:

<table>
<thead>
<tr>
<th></th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect in Dollars</td>
<td>-$504 (184)</td>
<td>-$751 (253)</td>
<td>-$839 (281)</td>
</tr>
<tr>
<td>Percentage Effect*</td>
<td>-9.8%</td>
<td>-13.2%</td>
<td>-13.8%</td>
</tr>
</tbody>
</table>

Effect on Husband’s Wage Earnings in:

<table>
<thead>
<tr>
<th></th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect in Dollars</td>
<td>-$220 (143)</td>
<td>-$387 (141)</td>
<td>-$404 (223)</td>
</tr>
<tr>
<td>Percentage Effect*</td>
<td>-5.6%</td>
<td>-9.1%</td>
<td>-9.1%</td>
</tr>
</tbody>
</table>

Effect on Wife’s Wage Earnings in:

<table>
<thead>
<tr>
<th></th>
<th>1st Year</th>
<th>2nd Year</th>
<th>3rd Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect in Dollars</td>
<td>-$182 (79)</td>
<td>-$255 (109)</td>
<td>-$377 (135)</td>
</tr>
<tr>
<td>Percentage Effect*</td>
<td>-21.2%</td>
<td>-26.7%</td>
<td>-32.4%</td>
</tr>
</tbody>
</table>

*The dollar effect as a percentage of the control mean.
Table 5
Overall Estimates (and Estimated Standard Errors) of the Effect of Participation in the Negative Income Tax Program on Wage Earnings

**Effect on Family Wage Earnings**

<table>
<thead>
<tr>
<th>Effect in Dollars</th>
<th>Difference in Means Estimate</th>
<th>Regression Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>- $690</td>
<td>- $765</td>
</tr>
<tr>
<td></td>
<td>(136)</td>
<td></td>
</tr>
<tr>
<td>Percentage Effect$^1$</td>
<td>- 12.3%</td>
<td>- 13.6%</td>
</tr>
</tbody>
</table>

**Effect on Husband's Wage Earnings**

<table>
<thead>
<tr>
<th>Effect in Dollars</th>
<th>Difference in Means Estimate</th>
<th>Regression Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>- $333</td>
<td>- $248</td>
</tr>
<tr>
<td></td>
<td>(107)</td>
<td></td>
</tr>
<tr>
<td>Percentage Effect$^1$</td>
<td>- 7.9%</td>
<td>- 5.9%</td>
</tr>
</tbody>
</table>

**Effect on Wife's Wage Earnings**

<table>
<thead>
<tr>
<th>Effect in Dollars</th>
<th>Difference in Means Estimate</th>
<th>Regression Estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>- $266</td>
<td>- $302</td>
</tr>
<tr>
<td></td>
<td>(62)</td>
<td></td>
</tr>
<tr>
<td>Percentage Effect$^1$</td>
<td>- 27.1%</td>
<td>- 30.5%</td>
</tr>
</tbody>
</table>

1 The dollar effect as a percentage of the control mean.

2 These results are derived from Bawden (6). In each case the estimates are weighted averages of separate coefficients for the Iowa, North Carolina-Black, and North Carolina-White samples, with weights of .261, .485, and .254, respectively; these being the proportions of the total sample from each location.
and for wives of 27.1 percent. Table 4 reveals that the experimental effect on the wage income of both husbands and wives tended to increase in each year of the experiment. Moreover, the statistical hypothesis that the true experimental effects are zero is firmly rejected by these data.

In Table 5 I have compared the average over the three experimental years of the simple estimates contained in Table 4 with the more complicated regression estimates contained in the draft report of the results of the experiment. Remarkably enough, for the family as a whole the difference-in-means estimates are very similar to the regression estimates. The decline in wage income for husbands is slightly larger and for wives it is slightly smaller in the difference-in-means estimates than in the regression estimates. One difference between the results that is not contained in the table refers to additional earners in the family. The difference-in-means estimate implies a decline in the wage income of these secondary workers as a result of the experiment of $91 or 20.8 percent of the control group mean, while the regression estimates imply a decline of $215 or 49.0 percent of the control group mean. Which of these estimates is to be preferred is not clear at this point. In any case, the differences seem very small and are probably of little consequence compared to the sampling error of the estimates.

Since the design of the Rural Negative Income Tax Experiment was meant to ensure that members of the treatment group would receive positive subsidies it should be clear that the Rural and Urban Experiments measure conceptually different parameters. In practice, however, some of the treatment families in the Rural Experiment were above the break-even income level for their plans. It follows that one might therefore expect the largest average response to a negative income tax plan to be predicted from the cross-sectional estimates, and that the response in the Rural Experiment would fall
between the cross-sectional prediction and the results of the Urban Experiment. A comparison of Tables 2 and 5 suggests that this is what did happen. However, the extent of the difference between the results of the Urban and Rural Experiments depends somewhat on whose estimates of the Urban Experiment's effect are used. Hall's estimates of the effect of the Urban Experiment on labor supply are nearly identical to the estimates from the Rural Experiment contained in Table 5. On the other hand, the results from the cross-sectional predictions are between one-third and one-half larger than those from the Rural Experiment. Whether this should be taken to imply that the effects of a permanent negative income tax program might differ from those of a transitory program such as the Rural Experiment or whether it should be taken to indicate something else will require further analysis and evidence.

Finally, it is necessary to take up the question of the effect of variations in the tax rate and guarantee level of the various plans that were found in the experimental data. The importance of accurate estimates of these effects cannot be overstated. First it is necessary to have estimates of the effect of variations in tax and guarantee levels so that the impact of alternative potential programs can be considered. Given that the tax and guarantee levels to be a part of any actual negative income tax program are a subject for public decision, it could be argued that obtaining reliable estimates of the effect of variations in these parameters on labor supply is the entire rationale for the experiment and that failure to obtain these estimates constitutes a major failure of the experiment. Second, it is important to verify through variation in the tax and guarantee level that it is income and substitution effects that lay behind the observed response to the experiment. Otherwise, there always remains the possibility that the experimental effect is due to come other causal mechanism altogether.
It is important to have this issue clarified since it has wide implications both for the study of labor supply behavior and for many normative aspects of both taxation and welfare policy. In effect, the experimental data offer an extraordinary opportunity to put some quite basic theoretical structures to a test and this opportunity should not be missed.

At it turns out, this issue does not seem to have received nearly the attention it deserves by the authors of either the Urban or the Rural Negative Income Tax Experiments. There are many difficulties with the handling of this issue in the report on the Rural Experiment. First, there is very little discussion of the appropriate functional form for examining the effect of either the tax rate or the guarantee level on labor supply behavior. The tax rate is simply entered linearly into each of the regressions for the various response variables, while the guarantee level as a proportion of the poverty level is entered linearly as well. In some places it seems to be assumed that higher tax rates must lead to lower labor supply, even though there is nothing in the theory of labor supply to suggest this should happen and considerable evidence that for husbands the opposite is likely to occur. Why the guarantee level relative to the poverty income level is the relevant transformation for the guarantee variable is never made clear and the implicit interaction between the guarantee level and the family's size that this implies for the estimating equation seems never to have been investigated. Finally, in every regression that contained the tax rate and guarantee variables there is also included a dummy variable indicating presence in the experimental group. Often this variable's coefficient is large in size and estimated quite precisely while the tax and guarantee effects are not. Since the experimental effect of the program is supposed to operate through the guarantee and tax rate effects, the power of this additional variable should be considered
disturbing. Certainly it should have troubled the investigators enough to have caused them to search for specification and other errors that may be present.

In order to pursue this issue in more detail I report the results in Table 6 of two very simple regression schemes for the estimation of the tax and guarantee level effects on wage earnings. In the first scheme I have simply used the change in wage earnings as dependent variable and the tax rate and dollar guarantee as independent variables in a pooled regression of all 745 observations that allows separate intercepts for each experimental year. This method essentially treats the uncompensated labor supply effects and the marginal propensity to consumer non-market time as parameters for the purpose of estimation and forces the experimental response to work through tax and guarantee effects. (3) In the second scheme the independent variables are the tax rate and the estimated subsidy calculated on the basis of the family's pre-enrollment income. This method essentially treats the compensated labor supply effects and the marginal propensity to consume non-market time as parameters for the purpose of estimation.

(3)

In the notation of Appendix A the coefficient on the tax rate is an estimate of \(-w_i [w_i]h_i/\partial w + w_i h_i/\partial w_f\) for the ith individual and \(-w' h_i/\partial w_f\) for the family, where \([h_i/\partial w_f]\) is the matrix of uncompensated labor supply derivatives. Elasticities are computed by dividing by the appropriate measure of wage income, and multiplying by -1. The coefficient on the guarantee level is an estimate of \(w_i h_i/\partial Y\) for the ith individual and \(w_f h_i/\partial Y\) for the family.
<table>
<thead>
<tr>
<th>Parameter Scheme 1</th>
<th>Tax effect</th>
<th>Mean uncompensated labor (L)</th>
<th>Supply elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>~0.88</td>
<td>~0.94</td>
<td>~0.94</td>
</tr>
<tr>
<td></td>
<td>(3.12)</td>
<td>(2.93)</td>
<td>(1.39)</td>
</tr>
<tr>
<td></td>
<td>~0.67</td>
<td>~0.84</td>
<td>~0.84</td>
</tr>
<tr>
<td></td>
<td>(3.67)</td>
<td>(3.93)</td>
<td>(1.23)</td>
</tr>
<tr>
<td></td>
<td>~1.44</td>
<td>~1.44</td>
<td>~1.44</td>
</tr>
<tr>
<td></td>
<td>(5.84)</td>
<td>(5.86)</td>
<td>(5.86)</td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>Parameter Scheme 2</th>
<th>Tax effect</th>
<th>Mean uncompensated labor (L)</th>
<th>Supply elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>~1.05</td>
<td>~1.05</td>
<td>~1.05</td>
</tr>
<tr>
<td></td>
<td>(0.87)</td>
<td>(0.93)</td>
<td>(0.93)</td>
</tr>
<tr>
<td></td>
<td>~0.74</td>
<td>~0.94</td>
<td>~0.94</td>
</tr>
<tr>
<td></td>
<td>(2.78)</td>
<td>(2.56)</td>
<td>(2.56)</td>
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<tr>
<td></td>
<td>~1.49</td>
<td>~1.49</td>
<td>~1.49</td>
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<tr>
<td></td>
<td>(5.14)</td>
<td>(5.15)</td>
<td>(5.15)</td>
</tr>
<tr>
<td></td>
<td>~0.10</td>
<td>~0.10</td>
<td>~0.10</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.04)</td>
<td>(0.04)</td>
</tr>
<tr>
<td></td>
<td>~0.14</td>
<td>~0.14</td>
<td>~0.14</td>
</tr>
<tr>
<td></td>
<td>(0.96)</td>
<td>(0.96)</td>
<td>(0.96)</td>
</tr>
</tbody>
</table>

Note: (1) Evaluated at the mean of the control and experimental group earnings levels in the three experimental years.
and also force the experimental response to work through income and substitution effects.\(^{(4)}\) The top and bottom panels of Table 6 report the results of the first and second estimation schemes respectively. Both methods have the advantage of simplicity, but they suffer a number of disadvantages. Perhaps the most serious is the failure to account in estimation for the fact that many wives in the sample have no earnings and for the obvious possibility that what is treated as parametric in estimation is likely to vary within the sample. Although more sophisticated schemes should perhaps be explored, for reasons given below I doubt whether much more can be accomplished.

Consider first the results for husbands where the presence of non-zero earnings for all workers might suggest that a linear approximation to labor supply responses would be satisfactory. In the top panel the labor supply elasticity is positive while the guarantee effect is also positive. Although the former is perfectly consistent with the theory of labor supply the latter is not consistent with the presumption that non-market time is a normal good. In the bottom panel both tax and income effects

\(^{(4)}\)In the notation of Appendix A the coefficient on the tax rate is an estimate of \(-w_i[s_{i1}w_1 + s_{i2}w_2]\) for the ith individual and \(w'S'w\) for the family. Elasticities are computed by dividing by the appropriate measure of wage income and multiplying by \(-1\). The coefficient on the subsidy calculated on the basis of the family's pre-enrollment income is an estimate of \(w_i\delta h_i/3Y\) for the ith individual and \(\Delta w_i\theta h_i/3Y\) for the family. This scheme for the parameterization of the effects of a negative income tax experiment is related to a suggestion in Ashenfelter and Heckman \(^{(4)}\) and is a straightforward attempt to fit equation (4) in Appendix A. This method has an interesting interpretation
Continuation of Footnote (4)

in the context of forecasting the fiscal outlay of adopting a negative income tax program. In effect, the subsidy calculated on the basis of pre-enrollment income is the estimated cost of such a program if there is no labor supply response, while the regression is a forecasting scheme for determining incremental costs because of a labor supply response as a function of the tax rate and pre-enrollment subsidy level. Such a regression scheme would thus fit naturally into a model for simulating the total costs of hypothetical negative income tax programs.
have the expected signs, but in neither the top or bottom panel can the statistical hypothesis that both effects are zero be rejected. Now in the report of the experiment these results would be recorded simply as the inability to find "statistically significant" tax and guarantee effects for husbands. Surely this is not the end of the story, however. Recall that a plausible summary of previous cross-sectional estimates of the labor supply functions of husbands would suggest mean compensated labor supply elasticities of 0.1 and mean income effects of -0.1. Suppose, then, that we proceed to test whether the results in Table 6 are consistent with these expectations. In the bottom panel of Table 6, for example, it is easy to verify that the experimental results suggest a substitution elasticity slightly larger than expected and an income effect one-tenth the size expected.

Nevertheless, it is not possible to reject at any conventional level of confidence the hypothesis that the parameters estimated from the experimental data are identical with those derived from previous cross-sectional work. It would be just as correct, therefore, to record the results of the experiment as showing the inability to find tax and guarantee effects for husbands that are significantly different from what would have been expected on the basis of previous analyses of non-experimental data. The basic problem, of course, is that there is simply not enough information in the data from the experiment to be very confident about the estimates of the tax and guarantee effects for husbands. It seems unlikely that anything but a larger sample size, in one form or another, could surmount this difficulty.

The experimental results for wives in Table 6 do show a tax effect
that is statistically different from zero. Moreover, the estimated tax effect is very close to the effect expected on the basis of previous cross-sectional studies. The income effect, on the other hand, is of an unexpected sign, although for both estimation schemes the estimated effect is very close to zero. Moreover, for wives it is just barely possible to reject the hypothesis that the income effects estimated from the experimental data are equal to those expected from previous cross-sectional results. It may be reasonable to conclude, therefore, that the income effects for wives that result from a temporary negative income tax are, in fact, very close to zero.

The experimental effects of the tax rate and guarantee level on family wage earnings do not only reflect the responses of husbands and wives. In fact, the estimated tax rate effect on the earnings of other family members that is implicit in Table 6, is extremely large. The estimated guarantee effect on the earnings of family members that is implicit in Table 6 is also very large, and of an unexpected sign. Since the number of these other family members was very small, and since their earnings were a small fraction of the total, it is unclear whether these results for other secondary workers should be taken at face value. In particular, both of these problems suggest that the simple estimation schemes used in Table 6 might lead to serious problems and further work in this area may be useful.

The upshot of this simple analysis of tax and guarantee effects is that the former are larger than might have been expected based on previous evidence and the latter are negligible. As noted at the outset, these findings are consistent with two very different hypotheses. First, they are consistent with the presence of considerable incentives and opportunities for the
under-reporting of income by experimental families. Currently available evidence does not suggest, however, that this effect is likely to be the predominant one. These findings are also consistent with the view that the experimental families treated the experiment as only a temporary change in their opportunities, and that they behaved accordingly. If this latter interpretation is correct, however, it does imply that time horizons in family decisions must be fairly long so as to account for the very small income effects observed in the experiment, and that inter-temporal substitution of non-market time must not be very great so as to account for the closeness of the actual and expected substitution effects. Only additional analysis and evidence will resolve this issue.

Conclusion

The results of the Rural Negative Income Tax Experiment show an unambiguous average decline in the work effort of all family members in the experimental group of wage earners. Family wage income declined by an average of 12 per cent, while the wage income of husbands declined by 8 per cent and the wage income of wives by 27 per cent. These results imply that any first round estimate of the transfer costs of adopting a negative income tax experiment that ignores the behavioral response to the change in the opportunity set that will face family members will underestimate the ultimate transfer cost. For the data in the Rural Experiment the first round estimate would have been 78 per cent of the ultimate transfer cost. Whether this seems "large" must ultimately depend on one's individual values, but it is considerably larger than some of those writing about the experiment seem to have implied. The estimates of overall labor supply response for wage earners seem to be quite well determined in the Rural Experiment. They are smaller than would be predicted by a crude application
of the results of cross-sectional studies to this problem, but larger than some, but not all, of the estimates of behavioral responses from the Urban Experiment.

There are several lessons to be learned from the Rural Experiment although there remains a considerable agenda for further research. First, it is very important to specify the nature of the behavioral parameters and responses it is desired to estimate prior to the design of the experiment. The Urban Experiment was designed, in principle, to estimate the ultimate transfer cost among a particular sample of the population of adopting various possible negative income tax programs. This transfer cost is composed both of the subsidies received by families initially below the break-even income level and those who choose to fall below it as a result of their response to the presence of the program. In the Rural Experiment, on the other hand, it is only possible to estimate the ultimate transfer cost resulting from the former behavior alone. It is probably for this reason, and because the Rural Experiment did not suffer the vagaries of a dramatic change in the welfare laws confronting its participants, that the results of the Rural Experiment are so clear cut in comparison with those of the Urban Experiment. Perhaps also the investigators in the Rural Experiment had the benefit of the considerable practical experience that resulted from the operation of the Urban Experiment.

Second, one of the major reasons for using expensive experimental methods to measure behavioral responses to negative income taxation is to make it possible to apply simple analytical methods to the analysis of the results. The educational value of such simple methods is incomparably greater than the complicated methods invented to overcome the problems of non-experimental data and which, I might add, are sometimes suspect even to their inventors. The experimental nature of the data should be more
fully exploited.

Finally, my own prior views about the likely effects of a permanent negative income tax program on labor supply behavior have not been changed very much by the results of the experiment. This results from the failure of the experiment's analysts to show that the effects of the experiment are operating through the variations in the tax rates and guarantee levels that families in the experiment are facing. The simple analysis that I have performed suggests that, to the extent they can be disentangled, the bulk of the labor supply response is a result of the tax effect alone. This suggests that the experimental families may have treated the experiment as merely a temporary change in their opportunities, and that they behaved accordingly. In the absence of further evidence there must therefore remain serious doubts about the implications of the experimental results for the adoption of any permanent negative income tax program.
Appendix A

The application of the classical theory of consumer demand to the analysis of labor supply is by now a familiar problem. I want here only to spell out a slightly different approach to this analysis that is useful both in the prediction of the effects on earned income or hours of a negative income tax plan and in the examination of the issue of which of those families above the break-even income level will choose to be covered by such a plan. As in the usual framework, the optimal choices of a family of (say) two individuals that acts as if maximizing a conventional utility function may be described by the labor supply functions

\[ h_i = h_i(w_1, w_2, Y) \quad (i = 2), \]

where \( h_i \) is the hours of work of a family member and \( w_i \) is that family member's wage, while \( Y \) is the family's unearned income. The actual level of satisfaction obtained by the family is obtained by the substitution of the labor supply and commodity demand functions into the original utility function. Clearly this level of satisfaction depends on the wage rates, price of consumption, and unearned income level the family faces, so that, ignoring consumption prices because they are assumed unchanging, \( v = v(w_1, w_2, Y) \) is the indirect utility function of the family. \( v \) is the highest satisfaction level that the family can attain for a given set of wage rates and unearned income.

Of course, if \( v \) is the highest utility level attainable at wage rates \( w_1 \) and \( w_2 \) with unearned income of \( Y \), then the solution of the equation \( v = v(w_1, w_2, Y) \) for \( Y = E(w_1, w_2, v) \) must give the smallest level of unearned income necessary to reach the utility level \( v \) at those same wage rates. The function \( E(w_1, w_2, v) \) thus gives the minimum quantity of unearned income necessary to reach the utility level \( v \) at the wage rates \( w_1 \) and \( w_2 \). Since \( Y \) is by definition the excess of expenditure over
earnings, it is convenient to call \( E(\omega_1, \omega_2, \nu) \) the family's excess expenditure function. A very convenient property of this function is the result of the fact that the effect of a change in a wage rate on minimum unearned income is just
\[
\frac{\partial E(\omega_1, \omega_2, \nu)}{\partial \omega_1} = E_1 \equiv -h_1(\omega_1, \omega_2, \nu).
\]
Consequently \(-E_{ij} (i, j = 1, 2)\) gives the full set of utility-constant (or income-compensated) labor supply derivatives.

Now the effect of a negative income tax program is to provide a subsidy of

\[
D = G - t(\omega_1 h_1 + \omega_2 h_2 + \nu)
\]

to every family with total income below the break-even level \( B = G/t \), but nothing to those above this level. As it turns out, this kink in the relationship of the subsidy to income raises some special issues that would not be present if, after reaching the break-even income level, a family then moved on to pay positive taxes at the rate and guarantee in equation (1). Indeed, the original idea of the negative income tax was perhaps to eliminate such kinks. As it turns out, the implementation of an actual negative income tax would undoubtedly result in an abrupt change in the tax rate at the break-even income level, and the effect of the presence of this notch on labor supply would have to be analyzed just as it must be analyzed in the context of an experimental negative income tax.

The effect of small changes in the wage rates and utility or unearned income on labor supply are merely

\[
dh_i = -E_{i1} d\omega_1 - E_{i2} d\omega_2 - E_{iv} d\nu
= S_{i1} d\omega_1 + S_{i2} d\omega_2 + \frac{\partial h_i}{\partial \nu} h_1 d\omega_1 + h_2 d\omega_2 + d\nu,
\]

where I have used the facts that the \(-E_{ij} = S_{ij}\) are the utility-constant
derivatives of the labor supply functions, \( -E_i \frac{\partial v}{\partial Y} = h_i / \partial Y \) \( \{ \text{as may be verified by differentiating the identity } -E_i(\omega_1, \omega_2, v(\omega_1, \omega_2, Y)) = h_i(\omega_1, \omega_2, Y) \} \), and that \( dv = \frac{\partial v}{\partial Y}(h_1 d\omega_1 + h_2 d\omega_2 + dY) \). For a family that actually receives payments under the negative income tax, wage rates are changed from \( \omega_i \) to \((1-t)\omega_i \) and unearned income is increased from \( Y \) to \( G-tY \). A first order approximation to the change in labor supply resulting from the program is thus

\[
dh_i = -t \left( S_{i1} \omega_1 + S_{i2} \omega_2 \right) + (\partial h_i / \partial Y) D,
\]

where \( D \) is the subsidy evaluated at the pre-enrollment income level. Of course, \( S_{ii} > 0 \) and since leisure is taken to be a normal good \( \partial h_i / \partial Y < 0 \).

It is interesting to note, as Killingsworth [10] has pointed out, that so long as the cross-substitution term \( S_{ij} = S_{ji} \) is not zero, equation (3) implies only that at least one family member must reduce their labor supply. Calculating \( \omega_i dh_i \) in (3) and then adding shows that

\[
\omega_1 dh_1 + \omega_2 dh_2 = -t \omega' S^* \omega + \omega_1 (\partial h_1 / \partial Y) + \omega_2 (\partial h_2 / \partial Y) D
\]

where the vector \( \omega' = \begin{pmatrix} \omega_1 & \omega_2 \end{pmatrix} \) and the matrix \( S^* = \begin{pmatrix} S_{ij} \end{pmatrix} \). Since \( S^* \) must be positive definite by the logic of the utility maximization problem, the value of (4) must be negative. The message of this exercise is clear. The family earnings of those participating in a negative income tax experiment must decline. This suggests one reason why it is appropriate to concentrate empirical analysis on the effect of the experiment on family earnings. It is this quantity about which the classical theory is useful in making predictions.

A second reason for concentrating on the analysis of family earnings is that the change in family earnings induced by the program is proportional to the excess transfer cost of a negative income tax program over the cost calculated on the basis of pre-program incomes alone. Thus, if \( D^0 \)
is the average subsidy calculated from (1) on the basis of the pre-enrollment income of participants, \( b^1 = -t \left( w_1 d h_1 + w_2 d h_2 \right) + b^0 \) is an approximation to the ultimate transfer cost. Although there is surely interest in the composition of the labor supply responses of family members to the negative income tax, the ultimate size of the total transfer cost of such a program will no doubt loom very large in any public discussions of the adoption of such programs.

Of course, the preceding analysis is incomplete until it is specified which families will be participants in the program. This problem has been discussed by Greenberg and Kusters \([8]\) and the issue incorporated into their simulations of the cost of a negative income tax plan. Its importance has been emphasized more recently by Hall \([9]\). Of course, any family eligible for the plan will presumably opt into it if, at their pre-enrollment income level, they are entitled to a positive subsidy. After all, the family may then have more of consumption goods and/or leisure and therefore is unambiguously better off participating in the program. There will also be some families at pre-enrollment who are above the break-even income level and who will opt to work less and thus become participants also. The question naturally arises as to what determines who these latter families are.

To start with the simplest case, assume that only one family member works. Then if the family is in the program it needs unearned income of \( E((1-t)w_1, v) \) to reach the utility level \( v \), while it needs unearned income of only \( E(w_1, v) \) to reach the same utility level if it remains a non-participant. On the other hand, as a participant it has unearned income of \( G + (1-t)Y \) dollars while as a non-participant it has \( Y \) dollars. Clearly, the family will choose to participate in the program if

\[
E((1-t)w_1, v) - E(w_1, v) < G - tY,
\]
that is, if the extra unearned income needed to compensate the family for the damaging effects of the tax rate is less than the extra unearned income actually transferred to the family as a result of the program. A natural procedure is to approximate the difference \( E(1-t)w_1, v) - E(w_1, v) \) by a second-order Taylor series around pre-enrollment equilibrium. In this case we have

\[
(6) \quad E((1-t)w_1, v) - E(w_1, v) = (\partial E/\partial w_1)dw_1 + \frac{1}{2} (\partial^2 E/\partial w_1^2)(dw_1)^2
= -h_1 dw_1 - \frac{1}{2} S_{11}(dw_1)^2.
\]

Putting \( dw_1 = -tw_1 \) into (6) and the result into (5) shows that a family will participate if

\[
(7) \quad b^0 + \frac{1}{2} S_{11}(tw_1)^2 > 0.
\]

Since \( S_{11} \) is positive it follows immediately that any family with a positive subsidy at pre-enrollment will participate, but also that some families above the break-even will too. The pre-enrollment earned income level below which all families will participate may be called the opting-in income level and is simply

\[
(8) \quad w_{11}^0 = (G-tY)/t(1-.5te),
\]

where \( e \) is the compensated elasticity of labor supply. Since unearned income is very small for the negative income tax experimental group, the opting-in income for them is merely \((G/t)/(1-.5te)\), so that the ratio of the opting-in income level to the break-even income level is just \((1-.5te)^{-1}\). As might be expected, the opting-in income level increases with the compensated labor supply elasticity and equals the break-even income level when the compensated labor supply elasticity is zero. As an example, for a tax rate of .5 and \( e = .1 \), \((1-.5te)^{-1} = 1.026\), so that participants will be those with pre-enrollment incomes no greater than 2.6 per cent of the break-even income level. Other calculations for this case are contained in Table 1 of the text. By a similar argument it is
straightforward to establish that the opting-in income level for a family with two workers is

\[ w_1 h_1^0 + w_2 h_2^0 = (G-tY)/t \left[ 1 - .5 t (\theta_1 e_{11} + \theta_2 e_{22}) \right], \]

where \( e_{ij} \) is the income-compensated elasticity of the \( i \)th family member's labor supply with respect to the \( j \)th family member's wage rate and \( \theta_1 = w_1 h_1/(w_1 h_1 + w_2 h_2) \) and \( \theta_2 = 1-\theta_1 \) are the respective shares of each family member's earnings of total earnings at pre-enrollment. For the case where participants have negligible unearned income and the cross-substitution elasticity is negligible also, the ratio of the opting-in income level to the break-even income level is merely \( \left[ 1 - .5t(\theta_1 e_{11} + \theta_2 e_{22}) \right]^{-1} \).

As an example, with \( \theta_1 = .8, e_{11} = .15, e_{22} = .6 \) and a tax rate of .5, only families with incomes less than 6.4 per cent above the break-even level would participate.

One of the obvious difficulties in the application of the preceding analysis to the results of a negative income tax experiment is that it takes no account of the temporary nature of such experiments. In order to remedy this difficulty it is necessary to recognize that families must plan their work decisions for the current period based both on the wage rate and unearned income to be received in the current period and the wage rates and unearned income to be received in future periods. To take the simplest possible case, consider a family with only one worker and, since the expected relative wages in future periods will remain unchanged, represent future periods by a single composite measure of labor supply. More detail is contained in Metcalf's [11] discussion of this problem. Utility is now defined over lifetime consumption and, supposing that consumer-workers can borrow and lend freely at the interest rate \( r \), their budget constraint requires that
(10) \[ p_1 x_1 + (1+r)^{-1} p_2 x_2 = w_1 h_1 + (1+r)^{-1} w_2 h_2 + Y_1 + (Y+r)^{-1} Y_2 \]

the discounted present value of present and future consumption must equal the discounted present value of present and future income. In (10) \( p \) and \( x \) are the price and quantity of consumption goods, while the subscripts on all variables denote the period of consumption or supply. Putting \((1+r)^{-1} w_2 = \hat{w}_2, Y_1 = \hat{Y}\) and ignoring the unchanging consumer prices it should be obvious that the formal analysis of this problem is identical to the analysis of the two worker family above and that \( \hat{w}_2 \) and \( \hat{Y} \) play the roles that \( w_2 \) and \( Y \) played in the preceding case.

It is now a simple matter to distinguish between the effects of permanent and temporary negative income tax programs on labor supplied in the current period for those who participate. In the permanent program all wage rates are changed from \( w_1 \) to \((1-t)w_1\), while in the temporary program only the current wage rate is so changed. Likewise, in the permanent program the discounted present value of unearned income is increased by \( G + (1+r)^{-1} G \), while in the temporary program this increase in only \( G \). Referring to equation (2) and recalling the new interpretation of each variable, it follows that a first order approximation to the change in labor supply in the current period resulting from a permanent program is

(11) \[ dh_1 = -t(s_{11} w_1 + s_{12} \hat{w}_2) + (3h_1/\hat{Y}) \hat{D}, \]

where

\[ \hat{D} = G - t(w_1 h_1 + Y_1) + (1+r)^{-1}[G - t(w_2 h_2 + Y_2)] \]

is the discounted present value of the current and future subsidies evaluated in the absence of the program. Likewise, an approximation to the change
in labor supply in the current period resulting from a temporary program is

\[ dh_1 = -\tau(s_{11}w_1) + (\partial h_1/\partial Y)D, \]

where \( D \) is the subsidy in the current period evaluated in the absence of the program. The decline in labor supply resulting from the income effect of the temporary program is only the proportion \( D/\hat{D} \) of the decline resulting from a permanent program. Since \( D < \hat{D} \), the income effect of the temporary program is smaller than the income effect of the permanent program by an amount that depends on the length of the worker's planning period and rate of interest. On the other hand, the decline in labor supply resulting from the substitution effect of the temporary program is in the ratio \( s_{11}w_1/(s_{11}w_1 + s_{12}w_2) \) to the change resulting from a permanent program. As we have seen from equation (3) the current period substitution effect on labor supply of a permanent program need not even be negative. In any case, if current and future leisure are substitutes so that \( s_{12} < 0 \), the substitution effect on current-period labor supply of a temporary program is smaller (in absolute value) than that of a permanent program.
Appendix B

It is a straightforward matter to set out the implications of the possibility of tax evasion for the likely measured responses to the institution of a negative income tax experiment. Here I take up only the simplest possible case and concentrate on its implications for variations in reported income with variations in guarantee levels and tax rates.

For a more detailed analysis that concentrates on the implications of various attitudes toward risk, for example, see Allingham and Sandmo [2].

To simplify matters it is convenient to take the case where labor supply is exogenous and the family has a true total income of N. If X is the income stated by the family and accepted as correct by the experimenters, Z = N - X is the income on which tax is evaded. Presumably the probability of being caught evading, P, will depend positively on the amount of evasion, so that P = P(Z) and P' > 0. It is unclear what penalties are assessed for those caught evading, but suppose that any pecuniary or non-pecuniary penalties are independent of the amount of evasion and may be represented as θ. Total income if the family is not caught evading is then G - tX + N, while it is G - tN + N - θ if caught. Expected income is merely

\[ E = P(Z)[G - tN + N - \theta] + [1 - P(Z)][G - tX + N] \]

and will be maximized by choosing X so that

\[ P'(\theta + t(Z)) = (1 - P)t \]

if the second order condition is satisfied (P'' > 0 being sufficient, for example). This implies that at the optimum the amount of evasion is
\[ Z = (1-P)/P' - \theta/t \]

There are three points worth drawing from this analysis. First, the optimum amount of evasion for a participating family does not depend on the guarantee level in the family's program. This result is quite specific to the simple way this model has been set up, but it no doubt contains a kernel of the truth even in more complex cases. Of course, for a family that is initially above the breakeven income level the higher the guarantee and the lower the tax rate the more likely it is that evasion will be profitable for the purpose of obtaining participation. Second, implicit differentiation of the equilibrium condition (1) establishes that \( dZ/d(\theta/t) = 1/(ZP'' + P' + P) < 0 \), so that increases in the penalty and decreases in the tax rate tend to decrease evasion. Finally, in this framework it is equally profitable to decrease the reporting of all income regardless of its source, so long as all income is taxed at the same rate and the probability of being caught does not depend on the source of reported income. Thus, tax evasion provides a rationale for expecting a decline in reported income not only from earnings, but from other sources as well.
References


