Non-Labor-Supply Responses to the Income Maintenance Experiments

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The concept of a negative income tax has been actively discussed and promoted, at least by economists, for over two decades. High on the list of motivations for this are the inefficiencies and inequities of patchwork welfare programs that make arbitrary distinctions among potential recipients and concentrate on specific consumption items. The possibility of extremely high marginal tax rates on benefits, resulting in part from enrollment in multiple programs, also has contributed to interest in a negative income tax. The majority of the policy discussion has focused on the labor supply effects, which have so much potential influence not only on program costs but also on public perceptions of the welfare system. The centerpiece of the analysis from the various income maintenance experiments has always been the statistical manipulation of labor supply data. Invariably, however, residual analyses, typically described as "non-labor-supply results," are also included, and a portion of these results that do not involve the structure of the family forms the subject of this paper.

Since the focus of the experiments was so confined to labor force issues, design features in the other areas were not given the same degree of attention. At the same time, the detailed data have provided a good base for a variety of analyses, heightening the benefits of the experiments per se. The tag-on nature of much of this research is understandable. First, in a wide variety of possible non-labor-force

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effects there is no clear idea of what might be desirable. Underlying much of the negative income tax philosophy has been the notion that categorical, restricted aid programs tend to be inefficient because they do not recognize the specific preferences of the recipients. A corollary of this is that we do not have good notions of what kinds of spending behavior are most desirable. Second, a wide variety of areas have no real benchmark. We know relatively little about specific consumption patterns and how they vary across households, and, most specifically, about the overall pattern of spending by the poor as compared to the nonpoor. These issues interact with the interpretative problems that naturally arise in complex experiments: problems of sample selection, limited time horizons, imperfect experimental design, and data collection and measurement difficulties clearly affect the ability to generalize from the specific results.

For expositional purposes, if not substantive ones, it is convenient to divide the analyses into "consumption" and "investment" outcomes.[1] The reason for this division is clear. We have few firm opinions about the desirability of any consumption bundles chosen, and even a complete understanding of the determinants of consumption decisions is unlikely to have much influence on policies. On the other hand, investment-type activities are presumed good since they might lead to longer-run beneficial effects in the alleviation of poverty.

These categories clearly have fuzzy boundaries. Consumption by children, for example, might in fact be viewed as an investment, since better nutrition or housing may lead to long-run improvement in their welfare. Indeed any expenditures on children are frequently lumped into the "investment" category, because they tend to facilitate the development and learning of the next generation. For the most part, however, precision in the categorization is not all that necessary.

The data base for this paper is the vast amount of research engendered by the experiments and conducted by both the principal contractors and others. Simply extracting significant coefficients where they are found would clearly be misleading, however; doing so would obscure the volumes of regression estimates produced and would not highlight the issues most central to program policy considerations. Further, the distinctly different approaches to the same problem make quantitative comparisons virtually impossible in many areas. (See Hollister 1978.) This review will consequently be restricted to a smaller number of key areas. The emphasis is on identifying common findings that might be generalized. Whenever possible, the review refers to the books and journal articles coming from the experiments, on the grounds that these are generally more accessible than unpublished research papers or even the final reports on the experiments.

What Do We Expect To Observe?

Several factors affect what we would expect to observe in terms of consumption and investment responses to a negative income tax. First, we generally feel more confident about understanding behavioral responses to differences in permanent income than responses to transitory changes. With an increase in permanent income, people are expected to increase their overall consumption standards. With an increase in transitory income, the responses are less predictable and less interpretable because transitory changes are not necessarily shifts in the budget constraint. Therefore, when families receive income supplements under a negative income tax, their responses would probably vary depending upon whether or not they considered this to be permanent or transitory income. This is in large part a question about how individuals respond in the experimental setting.

A second issue is the dynamics of consumption. When adjustment costs are significant, individuals may not adjust immediately to changed circumstances. The clearest place to see this is in housing consumption. The Housing Allowance Demand Experiment suggests that only about one-third of the full adjustment occurs in the first year (Hanushek and Quigley 1979). Such slow adjustments cause severe problems in analyzing the short data series from the negative income tax experiments, because substantial portions of the complete adjustment cannot be observed over the course of the experiment. Clearly such lags can be explicitly incorporated in any analysis.[2] But the experimental analyses, particularly outside of the area of labor supply, rarely have pursued these issues.

Both of these issues suggest that the short-run effects observed from the negative income tax experiments might be poor estimates of the longer-run effects that would be observed in the steady state under a permanent and fully operational negative income tax. In some cases, the direction of bias is clear; for example, with investment in quantity of schooling, discussed below. In other cases, such as marital dissolution or fertility, the issue is less definite.

Finally, through the experiments, analysts have discussed the possible biases introduced by such things as sample design or attrition. When the findings depend upon estimates of mean differences between experimentals and controls, the estimates are a function of the precise sample employed. With nonrandom samples, estimated experimental effects alone are insufficient for policy purposes. To generalize these findings to a larger universe—one that differs in systematic ways from the experimental families—one must understand more fully the underlying structure of these behavioral effects. This is a difficult task in general, and the sample characteristics become more important. The

problems are undoubtedly less when any generalizations are based upon more fundamental behavioral estimates, such as estimates of income elasticities or the effects of family size on educational outcomes. The problems do not, however, go away. In these latter cases, which are conditional upon the observed family circumstances, it is still necessary to ascertain whether or not the probabilities of being in the sample are related to the investment and consumption decisions being analyzed.

Consumption Patterns

Two issues arise when considering the impact of a negative income tax on consumption patterns. First, a concern about the consumption levels of the poor motivates many to support transfer programs, but little is known about their actual consumption patterns. Large gaps remain in our knowledge about the consumption choices of the poor and the resulting patterns of expenditures and well-being across families. Second, and more importantly, no criteria exist to rank alternative outcomes. If, for example, we observe that families under a negative income tax purchase more clothing, what should we think about that? Is that good or bad? In a few areas we at least bring some preconceptions to the problem, but in most we have nothing to go on.

One obvious motivation for the study of consumption aspects of income maintenance experiments is the suspicion by some that subsidies will be used for frivolous expenditures—color TVs and fancy cars—rather than for the necessities of life.[3] The measurement of consumption in the experiments is very difficult, and none of the survey efforts appeared to do very well on this score. Nevertheless, the results suggest no general increases in frivolous or outlandish expenditures. Indeed, expenditures induced by experimental treatments follow (at least in aggregate) the same patterns observed from nonexperimental income. In other words, for most expenditure categories such as food, clothing, health expenditures, and so forth, the results show nothing startling or unexpected.

The area where increased consumption by the poor is most commonly recognized as a positive outcome is housing. Housing has always received special attention in public policy matters. This may reflect a general view that housing is a basic necessity and that a just society would provide safe, decent, and affordable housing for all of its citizens. It may also simply be a reflection of the preferences of the donors—that is, dilapidated housing is offensive to others and something should be done to eliminate blight in housing markets. Part of this is an externality argument that poor housing conditions lower the property values of others in the community. Part of it is simply a desire not to be con-

fronted by poverty that is inescapably obvious, as is the case with slum housing.

Moreover, for any quality level of housing, homeownership is frequently rated as superior to rental. Owners are more likely than tenants to maintain their homes, thus providing a superior stock of housing for the poor over time. Moreover, home purchase provides a very common way to accumulate wealth, which might give the poor the means to escape poverty.

In fact, housing policies and negative income tax proposals have often been considered together, including the conduct of parallel experiments. The housing experiments, conducted with many variants on the basic formula for the Housing Allowance Demand Experiment in Phoenix and Pittsburgh and with a saturation design in the Housing Allowance Supply Experiments in Green Bay and South Bend, provide a useful benchmark for the housing consumption results in the negative income tax experiments. The most common form of housing allowance considered is a negative income tax subsidy formula, with the guarantee and tax rate scaled to reflect housing costs and the fact that housing represents about 25 percent of total expenditures. Housing quality and rent standards are added to this subsidy formula, generally as eligibility criteria. The housing standards ensure that only people living in "suitable" housing receive the subsidies. (See Bradbury and Downs 1981 for a thorough review of these experiments.)[4]

The results of the combined studies in terms of expenditures on rental housing were surprising at the time. Before the experiments, it was commonly presumed that income elasticities for housing were approximately one. The negative income tax experiments and the housing allowance experiments consistently indicate that income elasticities of housing are relatively low for the poor: a 10 percent increase in permanent income, from a subsidy or from another source, implies an increase in housing expenditures of 2 to 3 percent in the short run and around 5 percent in the long run.[5] To be sure, it is difficult to obtain precise estimates of these elasticities because of the short-run nature of the experiments and the lags in adjustments discussed previously, but plausible adjustments do not affect the conclusion that income elasticities are considerably lower than previously believed. In other words, one might infer that the poor do not appear to view quality of housing as their most important problem, because they tend to spend only a small part of any added income on housing.

A second finding is more surprising. Analyses of data from the experiments in Gary (Kaluzny 1979) and Seattle-Denver (Ohls and Thomas 1979) indicate that the income maintenance programs tend to encourage homeownership. In fact the estimated effects appear to be quite strong. For example, at the beginning of the Gary experiment, 23

percent of the experimental households owned homes; this rose to 34 percent three years later. Of the increase, 4 to 6 percentage points appears to be a treatment effect (Kaluzny 1979).

One would expect that the temporary nature of the experiments—something not included in the housing analyses—would mute any effect on housing ownership.[6] Nevertheless, the estimated homeownership effect, which is reasonably consistent across the Gary and Seattle-Denver experiments, suggests some noticeable experimental reactions that could potentially have long-term consequences.[7] This result, however, may simply reflect "timing" effects. The addition of transitory income during the experiment might move up the time when a household has the means to make a housing purchase that it would otherwise have made sometime later. (This is similar to the finding of Dynarsky and Sheffrin (1985) on the homeownership effects of transitory income.)

The expenditure evidence from the negative income tax experiments is quite similar to that from the housing allowance analyses, the largest difference being that the housing allowance experiments obtain lower participation rates. This is almost certainly related to the necessity in many cases to move in order to take advantage of the housing programs—something eligible households might be unwilling to do in a short-run program. In the longer run, participation in an ongoing program would undoubtedly be higher than that observed in the housing experiments, but the magnitude of adjustments in housing consumption would probably stay low.

Investments in Human Capital

Investment in human capital appears very relevant for negative income tax policy. A negative income tax program operates directly on households' work incentives and rewards from market activity, which in turn affect households' investments in skills. The analytical problems surrounding the experiments are, however, quite severe because the returns to any investments in human capital will accrue over the entire lifetime and for all practical purposes will not be observed during the experimental period.

Two aspects of schooling have received attention during the experiments. The first is the extent to which a negative income tax program alters the school-work choices of youths in experimental families. The second is the effect experimental treatment has on the scholastic performance of school-aged children. Another form of human capital investment decision—entering into vocational training programs—has received less attention.

Quantity of Schooling

The influence on quantity of schooling obtained by youth is the more direct and observable investment effect during the time of the negative income tax experiments. The decision about school attendance or job entry (or neither) is clearly affected by both the costs of attending school and the subsequent returns through the working lifetime. A negative income tax subsidizes schooling by reducing the cost of not working, where forgone earnings are the most important costs of attending formal schooling.[8] What Gary Burtless (this volume) called a "sale on leisure" can also be interpreted as an increase in general scholarship funds. This effect on the costs of schooling will be the same in both short-run and long-run program operations.

The effects on returns are more ambiguous. A short-term experiment will not involve any important effect on returns to schooling. Longer-run effects will depend importantly on the generosity of the program and on the level of skills acquired. A very basic program might have no effect on the returns to investment if the child were above the breakeven point both before and after any marginal investment. At lower levels of investment or with more generous programs, an ongoing negative income tax program would, however, operate to lower the potential returns from an investment in schooling. Because of the potential effect on returns, experts disagree about what should be expected in terms of investment incentives with an ongoing program; (see Venti and Wise 1984; Rea 1977; and Weiss, Hall, and Dong 1980). In the case of a basic program, the experimental evidence would give a fair indication of long-run effects. In the case of lower levels of investment or more generous programs, we would expect any observed increase in school attendance for negative income tax recipients in the experiments to be an exaggerated statement of the likely ramifications of an ongoing program.

The analyses of schooling decisions have been conducted in a variety of ways. The most interesting consider, in one way or another, a trichotomous choice: work, schooling, or leisure. In each case,[9] the experiment appears to have had a positive effect on school attendance by youth in experimental families, along with a reduction in work activity. In fact, the results are strongly consistent across analyses: youth tend to increase schooling by about the same amount that they decrease labor supply, leaving leisure essentially the same as it would be without a negative income tax.

Because of the different specifications of the models, it is very difficult to summarize the quantitative impacts. Nevertheless, the estimated effects appear quite large and significant. For example, Mallar (1976) estimates the probability of completing high school for families on

a "middle" negative income tax plan to be 25 to 30 percent higher, with a one-half year increase in schooling for 18- and 19-year olds during the three years of the New Jersey experiment. Venti and Wise (1984) find an 11 percent increase for youth in the Seattle-Denver experiments. They also find that increases in schooling for experimental individuals are smaller among blacks than whites and greater for females than males.

The long-run implications are, as mentioned previously, still subject to question. Nonetheless, significant increases in school attendance may well result from a negative income tax because of the substantial subsidies that arise from reducing the opportunity cost of attending school. The full implications of this would, of course, also consider the rate of return to any increases in schooling.

The Seattle-Denver experiments present an additional policy investigation. One set of experimental treatments involved counseling and subsidies directly related to education and training. All participants received free counseling, while other groups received half or full payment of tuitions and other direct costs of training.[10] Thus, in these experiments it was possible to distinguish general "income" effects related to program subsidies from direct training allowances. The idea behind these experimental treatments is clear; through training inducements, it was hoped that individuals' human capital could be augmented sufficiently to offset some of the adverse labor supply effects. (A good description of these experimental treatments can be found in Hall 1980.) The explicit training subsidy of the Seattle-Denver experiments, however, appeared to have little effect on school attendance beyond those previously noted.[11] This finding undoubtedly reflects the relatively small direct costs of schooling for most of these potential students. (Note, however, that the effects and costs of such a subsidy program might differ dramatically from the sample observations if the program were opened up to unattached youth not living with their parents.)

Scholastic Performance

The analysis of educational performance in the experiments has been conducted within the general framework of educational production functions (compare Hanushek 1986b).[12] Various output measures are related to characteristics of families, friends, and schools. Additionally, within the experiments, an independent experimental treatment effect is estimated.

Before considering any specific evidence, it is useful to review why we might expect any effects from the experiments. Previous studies of educational production have invariably found that family background is extremely important in determining the scholastic achievement of

children. These studies have typically included some measures of socioeconomic status of the family as an indicator of the educational inputs in the home. But this work has for the most part not been very concerned about the details of the family effects or the underlying causal structure—things that are more important for evaluations in the negative income tax context.

The most common interpretation of the relationship between scholastic performance and socioeconomic status of the family is that socioeconomic status proxies a set of attitudes, abilities, and patterns of learning within the home. These would not be expected to change very quickly with short-term changes in economic circumstances. Thus, to the extent that the negative income tax experiments lifted the current economic situation of the family without changing these more fundamental factors, one would not expect to observe much effect on children's performance.

Nevertheless, a negative income tax might affect school performance through several routes. The most direct impacts on school performance might come from the tax's labor supply effects. Inputs of parents' time into children's learning have been a central concern of many researchers looking at the education of children. (See, for example, Leibowitz 1974, and Hill and Stafford 1974, 1980.) It is frequently asserted that inputs by the mother are most important, and, if so, this links education closely to a negative income tax, where secondary workers seem particularly sensitive to the labor supply incentive effects. The evidence, however, suggests that the relationship between mother's labor supply and children's achievement is weak (Murnane, Maynard, and Ohls 1981; Hanushek 1986a). Similarly, if a negative income tax encourages marital dissolution, the removal of one parent may well have direct educational effects.

Beyond direct time input of the parents, one would naturally look to direct improvements in the health and environment of the families and children. If a negative income tax leads to better nutrition, more effective expenditures on health, and to generally improved housing, the overall capacity of children to learn could be improved.[13] Improving housing may also involve shopping for better schools. Consumption expenditures that cut down on the time required to do household chores could also free time for parenting and educational purposes. Finally, in the more long-run category, any impacts on the number of children in the family could also filter back into educational performance. The extensive literature on family size and achievement supports the general notion that average achievement is lower in larger families.[14]

Systematic evaluations of school performance were conducted in the rural and the Gary experiments, each of which collected school data to

supplement the already available household data. The Seattle-Denver experiments made a much less serious attempt at collecting data that would be useful in the analysis of educational performance. Specifically, they did not have good measures of the characteristics of the schools attended by the children.[15] The discussion here concentrates on the rural and Gary experiments.

In each case, the methodology was straightforward. Standardized test data, absences, and school grades—collected from school records—were used to measure performance. Regression equations were estimated to explain individual student variations in performance as a function of preenrollment characteristics of the families and preenrollment performance on tests for the measure of outcome considered. A variety of school characteristics were also included to account for nonrandom differences in school and classroom assignments. A dummy variable was then included to indicate experimental status.[16] The interpretation of this experimental effect is simply the average performance change of students in experimental families compared to control families.

The direct experimental evidence on any relationship between treatment and scholastic performance is mixed. For the three separate experimental groupings (Gary, rural Iowa, and rural North Carolina), the most systematic experimental effects related to test score performance in the lower grades. Children in experimental families tended to improve relative to children from other families. In higher grades and in non-test score measures of performance, no generally significant experimental effects were found, although results differed somewhat across the samples.[17] Further, the experimental effects and consistency of the findings were greater in North Carolina (Maynard and Crawford 1976) than in Gary (Maynard and Murnane 1979); experimental effects on school performance were nonexistent in Iowa. Maynard and Murnane explain the different results by the generally more deprived backgrounds of children in North Carolina, but this hypothesis is not tested directly. In the Gary analysis, time in the experiment influenced achievement gains. Children who had been in the experiment for three or four years did significantly better than children in control families or children who had not been receiving the experimental treatment for as long a period of time.

The explanations of direct experimental effects on scholastic performance emphasize parental time effects. However, as noted above, these must come from fairly subtle factors since direct testing found no relationship between labor supply of parents and children's achievement. Other evidence on schooling plus the implausibility of inducing general changes in the educational environment of the home suggest

that any estimated effects of experimental treatments on educational performance should be discounted. Experimental evidence simply provides little information about the long-run, steady-state effects on scholastic achievement.

Generalizing from the Experiments

When prices and incomes vary across geographical areas, it is difficult to generalize to a national experience. The housing analyses provide the simplest example. Behavior varied significantly across the sites of the housing allowance experiments, particularly with respect to program participation. Changing conditions in the housing markets could account for the variation (compare Hanushek and Quigley 1981), but this explanation leaves some question about how to make generalizations in terms of expenditures, quality, and adjustments. Moreover, since housing represents a large fraction of a typical household's expenditures, such variation will filter through to other aspects of the consumption bundle.

Variations across sites also show up in the analysis of education. For example, the experimental effects estimated for scholastic achievement varied dramatically across sites. Maynard and Murnane (1979) hypothesize that the differences reflect the proportional differences in the amount transferred relative to initial incomes, but they do not test this directly. Venti and Wise (1984) find very different estimates of the college attendance induced by the income maintenance experiments in Denver and Seattle. In particular, Seattle youth—who are more likely to attend college in the first place—are found to react much more to the experimental treatment than Denver youth. No attempt is made to explain this difference.

Variations in behavior across regions are not easy to explain by economic theory and leave tremendous uncertainty about generalization from the experiments. Neither program costs nor participant behavior can be extrapolated easily. With the small number of experimental sites, there is no reason to presume that the sites are representative of the population or that the observed reactions in any way bound the range of behavior that would be observed in a national program. To all this must be added the previously discussed issues about limited duration experiments and time of adjustments. Certainly progress has been made on understanding some aspects of the dynamics, particularly with the Seattle-Denver variations in experimental length. But the uncertainty about results that arises from this source is difficult to eliminate.

Conclusions

The issues of the negative income tax experiments considered here are less central to the overall policy deliberations than the issues of labor supply or family composition. Moreover, in virtually every area considered here, cheaper and more direct ways exist to construct data bases and do analyses than through an experiment.[18] To the extent that major policy concerns remain about consumption or investment aspects of a negative income tax, a supplementary research program would provide more definitive estimates of behavioral reactions.

Consumption effects of a negative income tax program are difficult to observe or estimate from the experimental data. Besides the general analytical difficulties in this area, the limited duration of the experiments inhibits making many inferences about lasting consumption effects. Moreover, even if the research were to provide definitive results about behavioral effects, they would have little direct relevance for policy.

Potential effects on investment are a somewhat different story. As in the case of labor supply, there are some general policy preferences. Specifically, if the poor under welfare programs can make investments that lift them out of poverty, that would be desirable. The most likely place for a negative income tax to affect investment behavior is the area of human capital. Human capital investments operate to alter the returns to market work and thus are intertwined with the effects of a negative income tax that also alters the net benefits of market labor. Within the experiments, analyses have considered both the school attendance decisions and the scholastic performance of children in experimental families. The former seems much more relevant for policy purposes.

A negative income tax will lower the costs of continuing schooling, by lessening the cost of not being in the labor force. Further, the reduction in costs observed in the experiment will be the same as that from an ongoing program. The uncertainty in evaluating the experiments and projecting to ongoing programs arises in considering the potential effects on the returns to more schooling. A negative income tax could potentially lower the benefits to more schooling, but this would depend upon the generosity of the program and the potential earnings of the individual with and without any added schooling. For the experimental time period, at least, a negative income tax does appear to induce more schooling. In fact, for youth the reduction in labor supply brought about by the negative income tax is almost perfectly offset by increased school attendance. Thus the encouragement of skill development by youth may be one of the positive sidelights of a negative income tax.

[1]Such a division obviously reflects my economics background. Other taxonomies are plausible, and this taxonomy leaves out a variety of possible concerns such as delinquency rates, political behavior, or psychological factors. See, for example, Rossi (1975), Hannan (1978). While there were some attempts to analyze such noneconomic outcomes, no significant and consistent results emerged. Therefore, the limited focus of this paper does not distort the findings of the experiments.

[2]Incorporating dynamic adjustment processes in models estimated from the experimental data requires imposing an intertemporal structure on the models. In general, given the limited time dimension of the data, this structure cannot be tested or evaluated in any satisfactory manner.

[3]This might be interpreted as donors having preferences over the consumption that results from altruistic transfers.

[4]Almost exclusively, analyses of housing have focused on expenditure relationships, as opposed to real components of housing. This is clearly a result of both measurement difficulties and the heterogeneity of the housing bundle: an increase in the number of bedrooms in housing units is difficult to compare with an improvement in the quality of a unit. It does, however, mean that evaluation is more difficult because it is not possible to ascertain whether increases in housing consumption involve improvements in external conditions (which are most closely related to externality arguments), better space that might be beneficial to the study behavior of children, or other changes.

There is a certain ambivalence in evaluating outcomes on the basis of expenditures. All other things equal, we would surely like the poor to spend less on housing, not more. In fact, the housing allowance experiments evaluated increases in both spending (generally labeled a good thing) and rent burden or proportion of income going to housing (generally labeled a bad thing). In well-functioning markets, we are willing to presume that increased spending connotes improved conditions. However, since the housing allowance at times gives people an incentive to simply spend more even if the quality doesn't change, there are some questions about the interpretation of expenditures.

One way to consider improvements in real quality is through the analysis of hedonic price models. While this was done in both the negative income tax and housing allowance experiments, the findings appear to be quite sensitive to model specification.

[5]Estimates of income elasticities from the experiments tend to be quite low. The precise estimates depend very much on model specification, on the definition of income, and so forth. An elasticity of 0.5 is an estimate related to permanent income of the poor (Hanushek and Quigley 1982). The comparable elasticity from the demand experiments for current income is around 0.2. Comparisons of direct estimates (nonexperimental) and of those from the experiments are found in Hanushek and Quigley 1981.

[6]Ohls and Thomas 1979 do find that income maintenance dollars have a lesser effect on homeownership probabilities than dollars of income from other sources. This may well be a reflection of the discounting of negative income tax payments in individuals' calculations of their permanent income.

[7]The New Jersey experiment provides mixed evidence on homeownership (Wooldridge 1977 and Poirier 1977). In particular, any experimental effects disappear when disaggregated by ethnic group in Poirier's estimates.

[8]As Venti and Wise (1984) point out, the strength of this subsidy depends on whether the person making the schooling decision is a child in a family unit receiving a subsidy or is in a separate household and, in the former case, on the character of household decision-making.

[9]The central studies are: Mallar (1976) for New Jersey; McDonald and Stephenson (1979) for Gary; Weiss, Hall, and Dong (1980); and Venti and Wise (1984) for Seattle-Denver.

[10]Training subsidies were supposedly only for training directly related to occupational or job choices. All discussions of the program operations, however, emphasize that application of this criterion was very loose. Of those accepting subsidies, a majority went to community colleges, but there was considerable variation in this.

[11]Weiss, Hall, and Dong (1980) suggest an effect for heads of household already in school, but a small effect for other youths. Venti and Wise (1984) simply state in a footnote that this subsidy had no effect.

[12]This is an example where the experiments have offered a vehicle for pursuing research that is only tangentially related to the experiments. Because the experiments collected such detailed, longitudinal data on families, they provided key information for

investigating educational performance. By adding a side data collection effort at the schools, a unique data set on schooling was created.

[13]While most people are willing to accept the basic plausibility of these notions, it should be noted that the direct research on these matters does not allow very precise statements about their relationship to scholastic performance.

Further, there is no consistent evidence of health effects or even of increases in health expenditures from the experiments. For example, the findings by Kehrer and Wolin (1979) on birth weights have not been replicated elsewhere.

[14]See Lindert (1977); Belmont and Marolla (1973); Zajonc and Markus (1975); Hanushek (1986a).

[15]Analyses of Seattle-Denver data on school performance—concentrating on home environment—can be found in Manheim and Minchella 1978 and Knickman 1979. Neither finds much in the way of significant home environment effects.

[16]As discussed below, the rural analyses contained much more elaborate measures of potential treatment effects, including among other things interactions of treatments with a variety of measures of family and student characteristics.

[17]The specific output measures analyzed varied by site. For Gary, performance on standardized reading tests, academic grade point average, and days absent were considered; for Iowa and North Carolina, comportment grades were also considered. Absences were significantly reduced and comportment grades were significantly increased in the early grades in North Carolina (Maynard 1977). In Gary, there were some significant differences in academic grade point averages in later grades.

[18]Possible exceptions are analyses of the effects of direct training incentives or, in the case of the housing allowance experiments, estimates of price elasticities of housing demand.

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Discussion

*Katharine L. Bradbury**

Research from the income maintenance experiments relating to subjects other than labor supply and family composition seems to suffer from an inferiority complex. One sees this in similar summary papers from previous conferences as well as in Eric Hanushek's paper for this conference. Although the design and research of the income maintenance experiments focused on labor supply and much research also looked at the family composition effects of income maintenance, what Hanushek calls the "residual" analyses add to our knowledge in many ways. In particular, they may have important policy implications, from informing concerned taxpayers how income maintenance benefits are spent, to helping to choose between in-kind and cash benefits. The relevance of these findings is heightened in the current policy context where both the right and the left have focused attention on the "culture of poverty" as a major stumbling block to efforts aimed at improving the opportunities and well-being of the poor.

Hanushek presents the experimental findings regarding housing and education. These comments will briefly summarize the research relating to some other areas of consumption and investment, including health, as well as the social and psychological investigations.

Why should we be interested in the effects of income maintenance on behavior other than labor supply and family composition? William Baumol laid out the issue very clearly in an early paper summarizing the consumption, health, and social behavior results of the New Jersey-Pennsylvania experiments. He said:

> Those who fear the worst of a [negative income tax] system may hold the hypothesis that a large part of the payments will be wasted by the recipients—

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> either being spent on drugs, drinks, and gambling or being dissipated in increased leisure time unproductively used. Those espousing such an extreme view would say that a program of unconditional cash payments should be avoided, perhaps to protect the potential recipients from their own folly, and certainly to prevent the use of public funds for such purposes.
>
> At the other extreme is the view that such a program might well improve the life style of the recipients. The assurance of financial support and the increase in expected income might lead to a modification in attitudes that could, perhaps, be described as the adoption of middle-class values and a reconciliation with the goals of the bulk of society. Such attitude changes might be expected to lead, among other things, to political activity, increased interest in education and quality of neighborhoods, lower crime rates, and reduced neurosis and psychosis.

While economists prefer to avoid making value judgments about how people spend their money, taxpayers appear to have no such reluctance. The experimental evidence on consumption provides some facts that can replace the stereotypes which seem to dominate in taxpayer reactions.

A second reason to be interested in these results is that they may provide useful information to policymakers faced with a choice between cash assistance and in-kind or targeted forms of aid. For example, the finding of lower income elasticities of rental expenditures in the income maintenance experiments than in the housing allowance experiments led Seattle-Denver researchers to conclude that targeting has important benefits if the aim is to encourage housing consumption.

Aside from labor supply and family composition, the experimental results are fairly easily summarized: recipients of cash payments do not change their behavior noticeably except in reflection of their increased income. Some specific results are described below.

Regarding consumption, increased income, in general, enabled recipients of negative income tax payments to consume more and they did so in rough proportion to their consumption in the absence of such payments. In other words, the source of income is not particularly important in its allocation among possible uses, and income levels were not increased so much by the experiments that basic consumption patterns shifted in response. The results are mixed in terms of whether payments were used to increase net assets and net savings other than through changes in housing, although net worth certainly rose for some subgroups in some sites. The limited duration of the experiments raises the question of permanent versus transitory income changes and hence issues of timing with respect to the accumulation of durables, which the researchers investigated. But in any case, the experiments do not suggest that payments were used in ways that taxpayers would view as purely frivolous or immoral.

The area of health is of particular interest to those concerned with welfare policy, since poor health is thought to be one of the immediate causes of poverty and one of the ways poverty is perpetuated from generation to generation. It is difficult to measure actual health status, so most of the analyses focused on such measurables as utilization of medical care, nutritional adequacy, and infant birth weight.

As far as the researchers could determine, medical care utilization did not increase and health status did not improve as a result of income maintenance payments. A major caveat regarding the finding of no effect is that in most cases investigators were unable to control for the availability of Medicare or health insurance, which could confound the experimental effect. In the Rural Experiment, children's health status appeared to improve, but the differences were not statistically significant. Some evidence surfaced in New Jersey that recipients altered patterns of medical care utilization, shifting toward private physicians from hospitals and clinics. In Seattle and Denver, recipients spent slightly more on health care than the control group. A general conclusion one might draw from these results is that programs aimed directly at health care have a better chance to have an effect on health status than do cash transfers.

Low infant birth weight can be an indicator of poor health in the mother, and is often associated with later poor health and developmental difficulties of the child. Birth weight was studied only in the Gary and Seattle-Denver experiments. Seattle-Denver researchers found no significant effects of experimental status on low birth weight and hence infant health status. In contrast, payments in Gary were associated with significant declines in the prevalence of low birth weight among those mothers at the highest risk. The researchers found no difference in the frequency or type of prenatal care received by experimental and control mothers in Gary, so they hypothesized that the improvement resulted from improved nutrition, although they had no direct evidence to test the hypothesis.

Nutrition was directly studied only in the Rural Experiment. There was no ascertainable effect of payments on nutrition in Iowa, but a small persistent positive effect in rural North Carolina, where the baseline nutrition levels were noticeably more deficient. This positive effect showed up for 9 of 10 basic nutrients examined. The researchers inferred that those families with deficient nutrition used their payments to bring nutritional levels closer to minimally adequate levels, while those with adequate nutrition pursued other goals with their increased income.

Researchers also studied a number of measures related to recipients' attitudes, mental health, community involvement, political activity, social integration, and the like. In the Rural Experiment, researchers concluded that payments had no negative effects on psychological well-

being and overall perhaps some slightly positive effects. In addition, experimentals were more likely to vote and otherwise participate in electoral activities than controls. In New Jersey, researchers found no effects on psychological distress or social integration. While they failed to find that reduced levels of distress were associated with additional income, they point out that they also failed to find any psychologically deleterious effects on participants, such as a decline in self-esteem. In the Seattle-Denver studies, a few subgroups of experimentals showed slightly higher levels of psychological distress than controls, but the conclusion again was "no effect."

Overall, these results suggest that the lives of recipients were not dramatically altered by the payments offered for a limited time period in the income maintenance experiments. Consumption rose modestly, as would be expected with a modest rise in income. Most other indicators of well-being showed little if any change. In terms of long-term consequences, the improvements in education (noted in the Hanushek paper) and the improvements in birth weight and nutrition among those with the worst initial deficits are probably the most promising. In these areas there is no normative confusion about what constitutes an improvement.

These results allow us to reject, as Baumol did, his two extreme hypotheses: that payments will be "squandered" or that recipients will be transformed into members of the middle class. Since these views are part of the underlying differences between camps in the welfare reform debate, the lack of noticeable effects is, in itself, a notable finding.

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Discussion

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Hanushek has given us a thoughtful essay about the studies from the negative income tax experiments on subjects other than labor supply and marital behavior. His decision, however, to concentrate on two topics of interest to policymakers leaves many of the studies by the research teams unreferenced here. Consequently, Katharine Bradbury and I, as discussants, independently chose to amplify Hanushek's comments rather than critique them. Fortunately, we selected different subsets of results for discussion and so among the three of us we may have provided a reasonably complete sketch of the studies undertaken.

I wish to make two general points in these remarks. First, important suggestive results in a variety of areas should not be ignored in any overall review of what was learned from the negative income tax experiments. Second, a negative income tax experiment, by its nature, is ill-suited to yield high quality data for the analysis of a wide range of behavior. The remainder of my remarks will elaborate on these two points.

Hanushek summarizes the results on housing and schooling under the appropriate taxonomy of consumption and investment. He indicates that his selection of results was made in part on the basis of "common findings that might be generalized." Additional findings in one or another of the four experiments, not reinforced by the other experiments, should also be mentioned. The absence of corroboration of findings from other experiments was often—not always—attributable to a difference in design, circumstance or method of analysis.

On the topic of health, which Katharine Bradbury has reviewed, I

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wish only to emphasize an important suggestive finding from the North Carolina rural experiment. Among the very poor in that survey the experiment seemed to have small but consistently positive effects on the nutrient intake of the family members, which appeared to be stronger over time.[1]

Child care was studied in the Seattle-Denver income maintenance experiments (see Kurz, Robins, and Spiegelman 1975, and Robins and Spiegelman 1978). Substitution toward market forms of child care was observed, replacing family care and other forms of nonmarket care.

Migration is another subject investigated using the Seattle-Denver data. That experiment, unlike its predecessors, permitted the families that received payments in the Seattle or Denver area to move and retain their rights to receive the payments. Keeley (1980) found the rate of migration nearly 50 percent higher for those in the experiment than for the controls. Keeley found as well that the locations to which these families moved were relatively rich in amenities, such as low variation in temperature or relatively high average January temperature. As he suggests, this is what one might expect, given the negative income tax's high rate of taxation on money income relative to amenities.

Keeley (1980) investigated the fertility effects of the negative income tax using the Seattle-Denver data. For couples married at the outset of the experiment, the effects differed by race-ethnic group and duration of the experiment. For whites the effect was negative for the group in the five-year duration experiment, but for Hispanics it was positive for the three-year duration group. There was no statistically significant effect for blacks. Keeley points out that the net effect of a negative income tax on fertility is not clear in theory, because of potentially competing income, price and subsidy effects. The lack of consistency across these three groups of married women leaves us with no convenient generalization about fertility effects, but Keeley's paper suggests the effects may be more complex than his model was able to sort out. For women not married at the outset, Keeley reports no discernible effect of the experiment on fertility.

In all, the income maintenance experiments had a wide range of results on consumption of specific items, on investment in children, migration, health and other forms of human capital, and on other aspects of social behavior and attitudes. These results are not in general strong or clearcut.[2] Elsewhere (Michael 1978) I have criticized the studies from the rural experiment for focusing far too single-mindedly on the regression coefficient on the experimental effect. In many cases the opportunity to provide some useful descriptive information about the qualitative nature of the lives of these low-income families was lost in the rush to report a negative income tax coefficient. The research results on consumption and other non-labor-supply, non-marital effects

from these experiments are not one of the best features of the experiments. The results are broader than Hanushek's paper would suggest, but they are generally weak methodologically. They mostly report mixed, often puzzling and inconsistent findings, and they do not offer us a good guide to the various demographic, social and economic (non-labor-supply) repercussions of introducing a nationwide negative income tax.

One of the reasons for the weak results in these studies is implied by my second point: an income maintenance experiment is poorly suited for the study of a wide spectrum of social behavior. This is true for at least two reasons. First, the conduct of the experiment is sufficiently taxing that a secondary purpose, such as the collection of high-quality data on consumption or social behavior, is not given adequate resources.

Income maintenance experiments are by their nature quite costly. In addition to the cost of planning, conducting and analyzing the survey, there is the cost of the transfer income. Also, the funding agency interacts frequently with the survey and analysis teams in a social experiment, and that interaction tends to keep attention focused on the policy-relevant issue. As the explicit purpose of the experiment is to observe labor supply response, that response deservedly receives most of the attention of the planners. Any effort to divert resources of time or money to ancillary topics is resisted.

Another factor is political involvement, both with Congress and with the local agencies of government whose cooperation in the experimental survey enterprise is so essential. Given the costliness, the funding-agency involvement and the political oversight, it is little wonder that the inherent riskiness and high stakes of a negative income tax experiment keep the survey organization in a state of some anxiety throughout the experiment. That tenseness does not foster a receptive atmosphere for suggestions about improving the quality of data on secondary research topics.

On top of these pressures is the second obstacle to the study of these ancillary topics: the sampling for an income maintenance experiment requires the selection of a control group and an experimental group and necessarily these are concentrated in one or a few localities. Neither of these features is useful from the point of view of most secondary study topics.

Other large-scale studies using omnibus questionnaires and longitudinal surveys have proven themselves excellent vehicles for adding on topical modules of high quality. The pressures on the experimental surveys, however, coupled with their sampling design, do not, I am convinced, foster good data on these less essential topics.

If my second point is correct, the diversity of research on non-labor-supply effects in the negative income tax experiments is quite

remarkable and reflects well on the ingenuity and intellectual curiosity of scholars involved in their design and analysis. Yet, the generally weak and often conflicting results in these areas reflect, I am afraid, the quality of the data on these subjects. One does not see the experimental data on these other topics used subsequently in general analyses, a further indication of their quality. The data tapes are not typically placed in national data archives after the project report is prepared, and the general research community has shown little interest in obtaining these data for general analyses. Any assessment of the negative income tax experiments should include the fact that the data typically are not of very great value for studies other than their principal, explicit purpose.

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[1]See O'Connor, Madden and Prindle 1976. In an essay summarizing the consumption studies from the rural experiment, I suggest reasons why the results in that study may understate the impact of the income transfer on the health-relevant nutritional intake. See Michael 1978.

[2]For a good review of the Seattle-Denver income maintenance experiments results see Davis and Kehrer 1983; the Fall 1980 issue of *The Journal of Human Resources* has several summary papers from the Seattle-Denver experiments as well.

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