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Hanushek, Eric A.

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by Eric A. Hanushek*
University of Rochester

The concept of a negative income tax (NIT) 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 that concentrate on specific consumption items. The possibility of extremely high marginal tax rates on benefits that results in part from enrollment in multiple programs also has contributed to interest in such a program. The majority of the policy discussion, however, has focused on the labor supply effects. This is quite natural because labor supply effects have such powerful potential effects on program costs. This policy interest is mirrored in the analyses and reports flowing from the various experiments that have been conducted over the past two decades. The center piece of the analysis is always the statistical manipulations of labor supply data. Invariably, however, there is the residual analysis, typically labelled "nonlabor supply results." A portion of these everything-else analyses--those that don't involve the structure of the family--is 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. Indeed, it would appear that the data collection and

* I am indebted to Edward Gramlich, Bruce Jacobs, Charles Metcalf, Walter Oi, Charles Phelps, John Quigley, and Susan Silverman for valuable comments and suggestions.

analyses tended to be more closely related to notions of ease of data collection than any underlying view about importance or systematic interrelationship with the transfer system. This immediately put these analyses into the class of "research targets of opportunity." (At the same time, the detailed panel 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 explicable. First, in a wide variety of possible nonlabor force effects there is no clear idea of what might be desirable. An underlying notion of much of the NIT philosophy is 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 in general have good notions of what kinds of spending behavior are most desirable. Second, in a wide variety of areas there is no real benchmark. We actually tend to know relatively little about specific consumption patterns, how they vary across households, and, most specifically, what the overall pattern of spending by the poor is (as compared to the nonpoor). Therefore, if we observe a specific pattern, we have trouble knowing how it should be interpreted.

These issues interact with the interpretative problems that naturally arise in complex experiments like each of those that we have seen. 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 roughly into "consumption" and "investment" outcomes.¹ The reason for this division is clear. As mentioned, we do not have many real opinions about 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, there is a general presumption that investment type activities are good. Such activities 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 is not all that necessary, and there should be no need to quibble about the categorization as long as the general notions are recognized.

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. Indeed, each of the separate experiments has been the subject of its own overall evaluation by outside academics. The analyses of the experiments have produced mountains of material, apparently following the theory that, if the data were collected, they are worth a regression equation. Simply extracting significant coefficients where they are found would clearly be misleading, however, because that both would obscure the

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].

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 (cf. 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. As much as possible, I have attempted to reference 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 NIT. First, we generally feel more confident about understanding behavioral responses to differences in permanent income. 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 NIT, 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 variant of this notion is the analysis of labor supply responses in a temporary experiment (Metcalf [1973, 1974], Burtless and

Greenberg [1982]). In these formal models the labor supply of an individual is analyzed in a two period world where a NIT is received in the first but not the second period. The optimal response of the individual will be less in this world of temporary subsidies than in a world of consistent subsidies over time. This has a direct analog to the analysis of specific investment and consumption decisions. Households would be expected to evaluate their spending and saving decisions to be consistent with labor supply decisions, even when these latter decisions reflect the specifics of the NIT experiments.

A second issue is the dynamics of consumption. When there are significant adjustment costs, individuals may not adjust immediately to changed circumstances. The clearest place to see this is in housing consumption. Over time, as the income and family circumstances change, families adjust their desired levels of consumption. But, when there are significant fixed costs associated with consumption, as through moving costs, we would not expect either instantaneous or continuous adjustments in consumption. Indeed, from the Housing Allowance Demand Experiment, only about a third of the full adjustment to any changed circumstances occurs in the first year (Hanushek and Quigley [1979]). Such slow adjustments cause quite severe problems in analyzing the short data series from the NIT 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,

² Incorporating dynamic adjustment processes in models estimated from the experimental panel 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.

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 NIT treatments might misestimate the longer run effects that would be observed in the steady state under a permanent and fully operational NIT. In some cases, the direction of bias is clear; for example, with investment in quantity of schooling. In other cases, the issue is less definite; for example, with marital dissolution or fertility (which of course has a built in lag).

Finally, throughout 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 that is 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 will 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 explicitly the tangible consumption patterns of an NIT. First, there is a dearth of information about the consumption bundles of the poor. A concern about the consumption levels of the poor--particularly in terms of shelter and health--motivates many to support transfer programs to the poor. Yet there remain large gaps 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, there is no obvious way to express desired outcomes. If, for example, we observe that families under an NIT 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. In most we have nothing to go on.

One obvious motivation for the study of consumption aspects of NIT 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 area of consumption where increases are most commonly recognized as a positive outcome of the experiments 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

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

of its citizens. It may also simply be a reflection of the preferences of the donors--that is, that 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 lowers the property values of others in the community. Part of it is simply a desire not to be confronted by poverty that is inescapably obvious with slum housing.

Moreover, for any quality level of housing, homeownership is frequently rated as superior to rental. First, owners are certainly more likely than tenants to maintain their homes, thus providing a superior stock of housing for the poor over time. Second, home purchase provides a very common way to accumulate wealth. Therefore, if the poor can be encouraged to purchase homes, they might have the means to escape poverty.

There has in fact been a direct link between the consideration of housing policies and NIT ideas. The consideration of housing allowances has gone along with the consideration of negative income taxes, including the conduct of parallel experiments with housing allowance experiments. These experiments, conducted with many variants on the basic formula for the demand experiments (HADE) in Phoenix and Pittsburgh and with a saturation design in the supply experiments (HASE) in Green Bay and South Bend, provide a useful benchmark for the NIT housing consumption results. 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

insure that only people living in "suitable" housing receive the subsidies. (See Bradbury and Downs [1981] for a thorough review of these experiments.)

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: There is no obvious way to compare an increase in the number of bedrooms in housing units to an improvement in the quality of a unit. It does, however, mean that evaluation is a bit more difficult because it is not possible to ascertain whether an increase 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 so forth.⁴

The results of the combined studies do suggest, however, some consistency in terms of expenditures. Most importantly, one might infer that the poor do not appear to view quality of housing as their most important problem because they tend to spend a small part of any added income on housing. The results tend to indicate that income elasticities of housing are relatively low for the poor: A 10 percent increase in

⁴ There is a certain ambivalence in evaluating outcomes on the basis of expenditures. All other things equal, we would surely like for the poor to spend less on housing, not more. In fact, the housing allowance experiments evaluated both increases in spending (generally labelled a good thing) and rent burden, or proportion of income going to housing (generally labelled 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 NIT and housing allowance experiments, the findings appear to be quite sensitive to model specification.

income, from a subsidy or from another source, implies around a 5 percent increase in housing expenditures.⁵ To be sure, it is difficult to obtain precise estimates of these because of the short run nature of the experiments and the lags in adjustments discussed previously. Nevertheless, even when these issues are taken into account, the estimated elasticities of housing consumption remain low.

Analyses of data from the experiments in Gary (Kaluzny [1979]) and Seattle-Denver (Ohls and Thomas [1979]) do indicate that the income maintenance programs tend to encourage home ownership. In fact the estimated effects appear to be quite strong, and are puzzling because of their strength. 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 percent appears to be a treatment effect (Kaluzny [1979]).

One would believe a priori that the temporary nature of the experiments--something not included in the housing analyses--would mute any effect on housing ownership.⁶ This estimated home ownership effect, which is reasonably consistent across the Gary and SIME/DIME experiments, suggests nevertheless some noticeable experimental reactions which might

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 .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 .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 do have a lesser effect on home ownership probabilities than dollars of income from other sources. This may well be a reflection of the discounting of NIT payments in individuals' calculations of their permanent income.

have long term consequences.⁷ It may simply reflect "timing" effects, however, since the addition of transitory income during the experiment might have the effect of moving up the time when a household has the means to make a housing purchase that it would otherwise have made sometime later.

The expenditure evidence from the NIT experiments is quite similar to that from the HADE and HASE analyses. The largest difference across programs is probably that the housing allowance experiments obtain somewhat low participation rates in the program. This is almost certainly related to the necessity to move in many cases in order to take advantage of the programs--something eligible households might be unwilling to do in a short run program. In the longer run, participation of an ongoing program would undoubtedly increase from those observed in the experiments, but the magnitude of adjustments in housing consumption would probably stay low.

Human Capital Investments

Human capital investment appears potentially to be very relevant for NIT policy. A negative income tax program operates directly on the work incentives and rewards from market activity that are faced by households. Investments in skills are also focused on these. The analytical problems

⁷ The New Jersey experiment provides mixed evidence on home ownership (Woldridge [1979] and Pourier [1979]). In particular, any experimental effects disappear when disaggregated by ethnic group in Pourier's estimates.

surrounding the experiments are, however, quite severe because the returns to any human capital investments 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. First, does the NIT program alter the school-work choices of youths in experimental families? Second, does the experimental treatment have any discernible effect on the scholastic performance of school aged children? A final 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 NIT experiments. While such schooling effects could hold for the whole population, the incentives to invest in schooling early in a career suggest that attention should be focused on the youth population.

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 NIT subsidizes schooling by reducing the cost of not working, where foregone earnings are the most important costs of attending formal schooling.⁸ This effect on the costs of schooling will be the same in both short and long run program operations. The returns are more ambiguous. During a short term experiment

⁸ As Venti and Wise [1984] point out, the strength of this subsidy depends on whether the person making the schooling decision is 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.

there will not be any important effect on returns to schooling. In the longer run, there could be effects on the returns, but this 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 would be above the breakeven point both before and after any marginal investment. At lower levels of investment or with more generous programs, an on-going NIT program would, however, operate to lower the potential returns from an investment. Because of the potential effect on returns, there has been disagreement about what should be expected in terms of investment incentives with an on-going program; see Venti and Wise [1984], Rea [1977], and Weiss, Hall, and Dong [1980]. In the former case, the experimental evidence would give a fair indication of the long run effects of a NIT program. In the latter case, we would expect any observed increase in school attendance for NIT recipients in the experiments to be an exaggerated statement of the likely ramifications of an on-going 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 of the experimental analyses,⁹ there appears to be a positive effect of the experiment on school attendance by youth in experimental families along with a reduction in work activity. In fact, there is a strong consistency across the analyses: youth tend to increase schooling by about the same amount that they decrease labor supply, leaving leisure essentially the

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 SIME/DIME.

same as it would be without a NIT.

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 [1977] estimates the probability of completing high school for families on a "middle" NIT plan to be 25-30 percent higher with a one-half year increase in schooling for 18 and 19 year olds during the three years of the New Jersey. Venti and Wise [1984] find an 11 percent increase in schooling probabilities for youth in the SIME/DIME experiments. They also find that increases in schooling for experimental individuals are relatively less for blacks than white and relatively greater for females than males.

The long run implications are, as mentioned previously, still subject to question. There is reason to believe, nonetheless, that significant increases in school attendance may result from a NIT because of the substantial subsidies that arise from reducing the opportunity cost of attending school.

The SIME/DIME experiments do 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

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 criteria was very loose. Of those accepting subsidies, a majority went to community colleges, but there was considerable variation in this.

these experimental treatments is clear; through training inducements, it was hoped that individuals' human capital could be lifted 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 SIME/DIME experiments, however, appeared to have little effect on school attendance (beyond those previously noted).¹¹ This latter 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 (cf. 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 experiment. The studies of

¹¹ Weiss, Hall, and Dong [1980] suggest an effect for heads of household already in school, but a small effect for nonhead 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.

educational production have invariably found that family background is extremely important in determining the scholastic achievement of children. These previous studies have typically included some measures of socioeconomic status (SES) 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 NIT context.

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

Although the evidence from direct analyses of educational performance is thin in many areas, one might expect an operational NIT to affect school performance through several routes. The most direct impacts on school performance might come from the labor supply effects of an NIT. Time inputs by parents into children's learning have been a central concern of many researchers looking at the education of children. (See, for example, Leibowitz [1974], Hill and Stafford [1974, 1980].) It is frequently asserted that inputs of the mother are most important, and, if so, this links education closely to an NIT 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 NIT encourages marital dissolution, there may will be direct educational effects from the removal of one parent.

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 an NIT leads to better nutrition, more effective expenditures on health, and to generally improved housing, the overall capacity for children to learn could be improved.¹³ Improving housing may also involve shopping for better schools. Consumption expenditures that cut down on the time 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 falls 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 SIME/DIME experiments

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 not 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 [1986].

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 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 when 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

¹⁵ Analyses of SIME/DIME 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.

from nonexperimental families. In higher grades and in nontest score measures of performance, no generally significant experimental effects were found, although there are some differences across the samples.¹⁷ Further, the experimental effects and consistency of the findings were greater in North Carolina (Maynard and Crawford [1976], Maynard [1977]) 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 explanation of direct experimental effects on scholastic performance emphasizes parental time effects. However, as noted above, this must come from fairly subtle factors since the direct testing of a relationship between labor supply of parents and achievement finds none. In the rural experiment, fairly elaborate attempts were made to decompose the experimental effects into underlying causes (through interactions of treatments with background factors). These attempts are, however, difficult to interpret, because it is difficult to pick out consistent results across the different samples. For the most part, simple treatment

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, compartment grades were also considered. Absences were significantly reduced and compartment 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.

indicator variables convey the central findings. These experimental coefficients will be differences in average growth in test performance between experimentals and controls, holding constant preexperimental family incomes and preexperimental characteristics of the families. They will, therefore, include any effects of differences of average income, family composition, school characteristics, and so forth that occur over the experiment.¹⁸

Generalizing from the Experiments

Each of the experiments presents an issue of geographical sampling. When prices and incomes vary across geographical areas, there is an issue about generalizing to a national experience. The housing analyses provide the simplest example. There appeared to be significant behavioral variation across the sites of the housing allowance experiments, particularly with respect to program participation. This could readily be explained by varying conditions in the housing markets (cf. Hanushek and Quigley [1981]), but this leaves some question about how generalizations in terms of expenditures, quality, adjustments, and so forth should be made. 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--simply through the normal adding up conditions.

These variations across sites also show up in the analysis of

¹⁸ These estimates necessarily do not capture any differences in experimental effects across families. Therefore, for example, variations in income transfers to experimental families would be ignored.

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 there is not a direct test of this. Venti and Wise [1983] find very different estimates of the college attendance induced by the experiment in Denver and Seattle. In particular, Seattle youth--who are more likely to attend college in the first place--react much more to the experimental treatment than do Denver youth.¹⁹ No attempt is made to explain this difference.

Variations in behavior across regions are not easy to explain by economic theory, but leave tremendous uncertainty about generalizations from the limited experimentation that has been conducted. Neither program costs nor participant behavior can be extrapolated easily. With the small number of experimental site observations 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.

All this is added to the previously discussed issues about limited duration experiments and time of adjustments. Certainly progress has been made on understanding aspects of the dynamics, particularly with the added features of the SIME/DIME variations in experimental length. But the uncertainty about results that arises from this source is difficult to eliminate.

¹⁹ It is not possible, however, to ascertain the statistical significance from the paper.

Conclusions

The issues of the NIT experiments considered here are less central to the overall policy deliberations than either the labor supply or the family composition issues. Moreover, understanding or predicting behavior in these areas is less dependent on experimental evidence. In virtually every area considered here, there are cheaper and more direct ways to construct data bases and do analyses than through an experiment.²⁰ Therefore, to the extent that there are major policy concerns about consumption or investment aspects of a NIT, one could design and conduct a supplementary research program that 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. Beside 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, there is 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 NIT to affect investment behavior is the area of human capital. Human capital investments operate to alter the

²⁰ The possible exceptions to this are understanding the effects of the direct training incentives or, in the case of the housing allowance experiments, of estimating price elasticities of housing demand.

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, but the former seems much more relevant for policy purposes.

There is no doubt that a NIT will lower the costs of continuing in schooling by lessening the impact of not being in the labor force. Further, the reduction in costs observed in the experiment will be the same as that from an on-going program. The uncertainty in evaluating the experiments and projecting to on-going programs arises in considering the potential effects on the returns to more schooling. A NIT 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, a NIT does appear to induce more schooling. In fact, for youth the reduction in labor supply brought about by the NIT 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.

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