A Sociologist's View of the Income Maintenance Experiments

*Lee Rainwater**

Given how much money was spent on the negative income tax experiments, what can be learned about social, as opposed to economic, behavior seems remarkably skimpy. Nevertheless, something interesting can be learned from a consideration of why the experiments tell us so little about the people who were their subjects. I may have a vested interest in making this assertion, since when the experiments were being planned I argued that such would be the result. That is, we would not be able to understand in human terms what had happened because of the narrow way in which the data collection was to be done — and now I can say, "I told you so."

One might bring to bear a sociological view on the negative income tax experiments in one of two ways. The first is to ask what can be learned from the experiments that is useful in the development of substantive or theoretical sociological knowledge. The other is to ask what sociology has to contribute to policy lessons. Most of my comments are concerned with the latter, but from time to time I will take account of the reverse flow from "applied" to "basic" knowledge and puzzle over why it is so frustrating to try to learn something about social behavior from these experiments.

My remarks will offer ample evidence of a distinction often drawn between economics and sociology. Economists are interested in the choices people make; sociologists are interested in the fact that people have no choice. Our model is that constraints by institutions, culture, interaction dynamics and personality determine and overdetermine behavior in ways that often leave little room for rational choice. One of the important lessons for sociology as a field from the experiments has

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to do with understanding that choice still plays a role.[1]

I want to deal with three aspects of what I see as the failure of the experiments. The first concerns a failure of perspective in the initial conceptions of the negative income tax as a policy initiative: the experiments were done because of the widespread belief among policy experts that the negative income tax could bring major benefits to society. (I do not mean that only the other guys thought so; I supported the initiative also.) To properly see this failure we need to broaden our policy thinking in ways that have proved particularly difficult for American policy specialists.

The second lesson to be drawn has to do with a failure of method. Why, after the great expenditure of talent, time and money, have we learned so little? What are the lessons for social science methodology that we should take from a consideration of the quality and quantity of the experimental findings?

Finally, there is the failure of policy interpretation from the experiments. We should and could have learned much that we did not. There are reasonably solid findings in the area of labor supply and shaky but plausible findings in other areas, summarized in the three papers prepared for this conference by Burtless, Hanushek and Cain. The established, conventional wisdom concerning the experimental findings is that any negative income tax would be a very expensive and a very pernicious program. From a broader perspective, what other kinds of policy implications might be drawn?

Policy Paradigms That Informed the Experiments

It is easy to lose sight of the context of social and political concerns that led to an interest in the negative income tax as a policy innovation, but this context should inform our assessment of the lessons to be drawn. Remember that the idea of a negative income tax was put forward by a large group of economists of highly varied political persuasions as a centerpiece for a sensible war on poverty. It was also a distinctly American idea. Neither at the time nor since have other countries shown enthusiasm for a negative income tax.

The belief that something had to be done about poverty was clearly related to the revolution in race relations going on in the country. The growing realization of the plight of blacks outside the South was particularly important. In the early sixties the warnings of the Triple Revolutionists concerning automation and accompanying unemployment had captured a good deal of attention among intellectuals. Their proposals for a guaranteed annual income brought that idea to the fore. Though their arguments about the inevitability of rising unemployment were eventually defeated in the debate that followed (see Robert Lekachman's contemporary summary), the notion that job creation strategies for combating poverty were either too expensive or doomed to failure had taken hold by the end of the decade.[2] It seemed obvious that the lower class, and particularly blacks as the most disadvantaged victims of this process, would be hard to help with job creation programs.

Therefore, a prosperous society could meet its obligations to the poor more cheaply with transfer programs than with programs that required changes in the way labor market institutions operated. One heard that it was cheaper to give money to the poor than to make jobs for them. The negative income tax won out over the credit income tax, its main income maintenance competitor, with the argument that money could be sharply targeted on the poor, and thus the tax burden kept lower.

By the mid-seventies the national mood had changed, and it no longer seemed urgent to do something to change in any basic way the propensity of American institutions to generate a rather steady rate of relative poverty — probably around one-sixth to one-fifth of families since the 1950s.[3] During the civil rights revolution the establishment mood had combined sympathy and not a little fear. Now the fear was gone and a nasty mood of "We've given them too much already" took its place. In this context the experimental findings of work and marital stability disincentives provided welcome support, but it would be a mistake to think that they caused the turning away from an incipient national commitment to increase the income of the poor.

As a way of appreciating the distinctiveness of these policy paradigms, let me return to the earlier observation that the guaranteed annual income never excited much interest outside the United States. European welfare states have generally adopted programs earlier than we, and they spend more of their national income on the programs we have in common. Why did the negative income tax not excite interest in the same way that many American social policy ideas have—for example, community action?[4]

The dominant model that has guided the development of the European welfare state has been a social security model. It has assumed several things about the society and the economy. The most important was that full employment would be a central goal of the state, and that it would exist most of the time. For the most part it was expected that the income distribution generated in the economy was a reasonable one, and that the purpose of social protection programs was to maintain a reasonable approximation of a family's usual income when an untoward event reduced the capacity of breadwinners to work—sickness, injury, old age. Family allowances existed to compensate families who chose to rear children and thus made more equitable the distribution of income between parents and nonparents. But family allowances were not expensive programs. The allowances generally have not increased as fast as other benefits; in most countries they did not keep pace with the growth in real income.

The assumption that social programs replace adequate work income, rather than substitute for it, not only requires near full employment, but also a very small low-wage sector. In Sweden, which has no minimum wage, almost no one who is employed full-time will be poor because the lowest wage is above the poverty line. In most other advanced welfare states in Europe the lower half of the income distribution has much less variance than in the United States (France may be an exception). The combination of a small low-wage sector and near full employment until recently meant that there were few pre-transfer poor working-age families in Europe.[5]

We have fairly precise evidence concerning the proportion of poor families for which a negative income tax would have been designed in Sweden, Britain and Germany compared to the United States around 1970.[6] Among families with a head age 25 to 54 years, the pre-transfer poor ranged from 6 percent to 9 percent in the European countries and amounted to 14 percent in the United States. The post-transfer poor ranged from 1 percent to 3 percent in Europe and 11 percent in the United States. Germany and Sweden moved three-quarters or more of their pre-transfer poor out of poverty with transfers, Britain 58 percent, and the United States only 22 percent. Few Europeans required public assistance to move them from poverty, and thus very few would have been candidates for a negative income tax.

These differences between Europe and the United States persist to the 1980s. Data from the Luxembourg Income Study (using a different poverty line and equivalence scale than our 1970 study) show that European post-transfer poor families with heads age 25 to 54 years ranged from 5 to 7 percent, compared to 15 per cent in the United States.[7] Poverty among children is found to range from 5 percent to 11 percent in the European countries compared to a 24 percent U. S. rate.[8]

From the mid-1970s this standard assumption of European welfare strategy — that social programs could rely on economic growth, full employment, and a compressed wage distribution from the low end to the middle — has increasingly been called into question by economic events. One might have thought an interest in guaranteed income strategies might have arisen on the part of mainstream policymakers and intellectuals. But it has not. Instead, policy responses have continued to emphasize the employment link through extended unemployment benefits, emphasis on subsidies to industries to maintain employment, and extensive retraining programs. Sweden provides the most dramatic example — expenditures on employment-related social programs increased from 0.5 percent of GNP in 1965 to 2.6 percent in 1983. (Benefits from industrial subsidy programs should be added to this, but data on numbers of beneficiaries and level of benefits from these programs are not available.) During the 1970s Sweden also expanded considerably its housing allowance program, which works like a negative income tax, but by 1983 it still cost less than 0.4 percent of GNP. Child allowances and advanced maintenance payments amounted to 1 percent of GNP.

Thus, about 4 percent of Swedish GNP is directed to the kinds of families who might be candidates for a negative income tax, but the mechanisms are very different, reflecting the social citizenship orientation of the Swedish welfare regime in contrast to the public assistance orientation of the American regime. In Sweden, child allowances go to all families; employment programs are for all those without adequate jobs; industrial subsidy programs slow down the pace of rationalization; housing allowances have high break-even levels and are administered in such a way that no stigma is attached to their receipt. (In any case the average beneficiary is not heavily dependent on them.) These program aspects are a reflection of a broader set of policy choices which together constitute a particular country's welfare regime.

In short, the first lesson to be drawn is a political-sociological one. It is essential to see any policy research in the context of actual or potential policies involved, and the chosen policies, in turn, in the context of the overall welfare regimes of which they are a part. The choices nations make can only be seen if we look at national policies in a comparative context.

Methodological Disappointments

The point of research is to increase understanding and, if possible, control. The experimental method in the social sciences, at least when done without a rich context of substantive knowledge on its subject, ends up long on control and much too short on understanding. The experiment is a black box — we know what goes in, and we know what comes out, but we don't know what went on in between, nor do we know how what happened happened. If the control the experiment provides does not also yield understanding, then we probably will do a bad job of prediction.

I think the principal shortcoming of the policy studies called negative income tax experiments was to conceive of them as only experiments. They were necessarily much more, and should have been even more than they were. As the researchers quickly discovered, they were forced to invent a whole system of administration and control — the experiment was also a program, a mini-program. All of the problems of implementation were present.

If the experiments had been conceived as a study in which was imbedded an experiment, we would know more. Instead the black box was preferred, and therefore we cannot answer interesting questions about the findings. Why did the marriages that broke up break up? What was the nature of those marriages that broke up, among both controls and experimentals? Were the people who reduced their work effort conscious of doing so? Did they think they did so because of the experimental payments? What did they think their own purposes were? Why did they not take all the money they could get by combining working and payments? What was their reasoning? More broadly, what did they think was going on — how did they construe the experiments? Early on, how did they expect to be affected, and how did their conceptions change over time?

In short, what is missing from the experiments is description, the most primitive level of science. The more ambitious goals of science, we all learn in introductory texts, presume the less ambitious. Without description we have hard findings, but brittle ones. More description might have yielded tougher findings. We would know much more if more resources had been devoted to chronicling the operation of the experiment (administration, relations of staff to subjects, etc.) and to learning more about the life situations of both experimentals and controls over the time of the experiment.

To do this, however, would have challenged much that is central to the ideology of contemporary social science. Much of the data that were needed would have been qualitative, narrative, processual. But quantitative methods have captured the imagination of both economics and sociology. We want so badly to be respected as sciences, and we have fixed on the elegant manipulation of numbers as the way to gain that respect.

The economists who ran the experiments might not have been willing to sponsor methods that looked "soft" and "subjective." Given the centrality of revealed preferences to economic methodology, they might have been little interested in rich contextual analysis of what was going on in the heads of the families being studied. But they had little chance to make the choice. For a couple of decades before the experiments, social psychology, for example, had pursued the quantitative will o' the wisp into a dead end, so that it had very little to offer in the way of useful methods to measure some of the noneconomic matters of interest. Sociology has forgone the opportunity to develop flexible methodologies of open-ended interviewing and coding in favor of closed-ended questionnaire approaches that seemed to offer more scientific control (and were cheaper, too).

The result is that a great many questions were asked, many answers given, recorded, tabled, but we never get to know the people and their lives.

The Failure of Policy Interpretation

The narrowness of the policy conclusions drawn from the experiments follows from the two kinds of problems sketched above. Because our knowledge from the experiments is shallow, and because the experiments are conceived in the context of the American welfare regime, with its implicit assumption of high unemployment rates, inactive manpower policies and a means-tested bias in transfer programs, the ''logical'' conclusion that is drawn from the findings is that the negative income tax is not a useful policy innovation. After all, we do not want to encourage the breakup of families, and we do not want to discourage people from working, and we do not want to spend a great deal of money on the poor.

Because we do not know anything about the dynamics of the marital disruptions associated with the negative income tax, even if one accepts the finding it is hard to draw other than simple-minded policy implications. Suppose all the excess of disruptions involved battered wives, or wives of alcoholic men. Would the policy implication still be that the negative income tax has an unconstructive effect? Do we believe that the extra money caused people to be self-destructive in the sense that they ended unions in which they would have been better off remaining? What happened to consumer sovereignty? If we want to give couples the best chance to stay together and if more income will make things better, we have only to choose the more generous plan.

To decide what the policy implications — as opposed to cost implications, the two should not be confused — of reduced worktime are, we need to know more about when people reduced effort, and what they did with the time. If the unemployed were a little slower in finding a job, they must have done some other citizen who wanted a job a favor. Given our unemployment rates, recipients taking their time finding a job because they receive negative income tax payments isn't likely to create a labor shortage. The marginal reductions of working hours increase the cost of a negative income tax but they do not seem large enough to constitute a major change in low-income workers' attachment to the labor force.

Given the ambiguity of the findings, whether the negative income tax was a good idea for this country reduces mainly to a question of cost — or of choice. From a comparative perspective on social protection even the most generous plan described by Gary Burtless would be relatively cheap — just 1.5 percent of GNP. Almost all European countries spend far more than we on social protection programs, yet their societies have not fallen apart and, within their economic realities, their economies are in reasonably good shape. Most have seen the proportion of GNP devoted to such programs increase over the past decade rather than decrease as ours has done. If for reasons of our particular history we were not likely to go the European route of heavy investment in employment programs, modest child allowances for all families, and even more modest housing allowances, then a negative income tax could have been considered a sensible, pro-family addition to the nation's social protection programs.

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[1]It is also encouraging to see an economist like Thomas Schelling begin to deal with issues of "nonrational" personality constraints; I hope that other economists will expand this kind of interest to more social constraints. Cf. Thomas C. Schelling, "Self-Command in Practice, in Policy, and in Theory of Rational Choice," *American Economic Review*, 74, (May 1984), pp. 1-11.

[2]Robert Lekachman, *The Age of Keynes*, McGraw-Hill Book Company, 1966, pp. 226-245.

[3]The one-sixth figure comes from my unpublished study using survey data starting in 1953. The one-fifth figure comes from the census data analysis by Christine Ross, Sheldon Danziger and Eugene Smolensky, "The Level and Trend of Poverty, 1939-1979," IRP Discussion Papers: DP#790-85, December 1985.

[4]See Peter Maris, *Community Planning and Conceptions of Change*, Routledge and Kegan Paul, 1982.

[5]See Michael Bruno and Jeffrey D. Sachs, *Economics of Worldwide Stagflation*, Harvard University Press, 1985, for a discussion of some of the consequences of high wages under recent economic conditions.

[6]Lee Rainwater, Martin Rein and Joseph Schwartz, *Income Packaging in The Welfare State*, Oxford University Press, 1986, and unpublished German data.

[7]Peter Hedstrom and Stein Ringen, "Age and Income in Contemporary Society: A Comparative Study," L.I.S.-C.E.P.S. Working Paper Series # 4, CEPS, Walferdange, Luxembourg.

[8]Timothy Smeeding, Barbara B. Torrey and Martin Rein, "The Economic Status of the Young and the Old in Six Countries," paper presented at annual meeting of the American Association for the Advancement of Science, May 1986.

Discussion

Charles Murray*

Lee Rainwater's paper makes some points that I want to second without elaboration. Program assessments—and the negative income tax experiments were a demonstration program writ large—have been persistently myopic in all the ways Rainwater points out. They have ignored the policy context within which they must fit. They have shown a remarkable indifference to trying to penetrate the black box of causation. They have been downright hostile toward qualitative data.

I do wish to elaborate on Rainwater's last and quite important observation, that the negative income tax findings do not drive an answer to the question, "Is a negative income tax a good idea as national policy?" Rainwater generally refers to the ways in which its advocates on the left might be able to live with reduced labor supply and increased marital disruptions. But the same point could be made of its advocates on the right. A Milton Friedman, one of the earliest proponents of a negative income tax, might well read the evaluations of the experiments and nonetheless continue to support the concept—Friedman always expected that a negative income tax would produce work disincentives, and the results of the experiment do not negate (or say much about) the merits he originally saw in replacing a Rube Goldberg welfare system with a tidier one. Has Friedman changed his mind because of the experimental results? I do not know, but nothing in the findings would demand that he do so.

Let me add to Rainwater's list three other reasons why we still have little idea whether a negative income tax is good policy or, for that matter, whether a wide range of other antipoverty devices are good policy.

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First is the problem of deciding upon a baseline. By definition, a negative income tax must have one. For the negative income tax experiments as in most other means-tested programs, the poverty line served that function. Let me suggest that the poverty index (along with any conceivable refinement of it) is so inadequate—so completely meaningless—that it obscures both the interpretation of the negative income tax results and any inferences about policy to be drawn from them. When I say "meaningless" I am not referring to problems of valuing noncash benefits, or to problems of imputing unreported income, or to any other marginal technical problem. I mean "meaningless." To know that someone is below the poverty line in this country is to know extremely little about that person except for the most general inference of low income.

The dominant source of the meaninglessness of the poverty index is the difference between living a low-income life in the inner city and in a small town. The average poverty index for a family of four in 1985 was roughly $11,000. Any of you in this audience with a spouse and two children, given that amount of money and told to live on it in a small town in Missouri or Colorado, could make a decent life for yourself and your family. You could get a decent place to live—small and shabby perhaps, but one that could be kept clean, warm, and dry. You could eat nutritious food, send your children to a pretty good school, live in a neighborhood with stable families and employed fathers and well-brought-up children. You could be safe from criminals. If you got the same $11,000 and had to go live in the South Bronx, you could not make a decent life for yourself. You could not do it, I submit, if your income was twice or three times the poverty line.

This source of error in the poverty index is not going to be finessed by more sophisticated cost-of-living discriminations, because the source of the problem is not the difference in the cost of hamburger in the South Bronx and in a small town in Missouri. It is the difference between a civilized, functioning community and one that is lawless and foundering. The poverty index cannot be reconstructed to cope with this. There is no negative income tax that can establish a baseline income that will enable everyone to have a decent standard of living. Persisting in attempts to correct any baseline index will probably only make matters worse—the most likely result being the introduction of large dollar differences in the allowances for urban and nonurban areas, in effect bribing people to stay in places like the South Bronx without making it possible for them to control their environment.

Another reason why we have no basis for voting yea or nay on a national negative income tax on the evidence of the experiments is that no one (to my knowledge) has used much imagination in anticipating the policy choices that would arise if the experiment were implemented nationwide. Let me give just one example of many. If a national negative income tax were put in place, we can predict (on the basis of what we do know about the nature of the underclass) that at the end of the first month, very large numbers of people will be without money for food. Large numbers of people will not have paid their rents. For good reasons or bad, large numbers of persons who have enough money to live a decent existence will be living the same existence they live now. What shall we do? Install Son of Food Stamps? Create a Special Rental Assistance Program for persons unable to manage their money? The merits of a negative income tax that supplants the current system are one thing; the merits of a negative income tax that supplements the current system are quite another. Professor Rainwater has lamented that we failed to consider policy in the context of the welfare state in which the policy must be implemented; I am lamenting that we failed to consider it in the context of the welfare population who would be the major intended beneficiaries of the program.

Finally, let me suggest that the assessment of the negative income tax, in common with the assessment of every other social program, wears blinders when it comes to selecting dependent variables for measuring success. If the negative income tax had produced very small changes in labor supply and if it had not shown other deleterious effects such as marital disruption, it surely would have been interpreted as a great success. It would have accomplished the Great Good of recent social policy, bringing people above the poverty line. But just because income is one of the few outcomes we can measure reliably, it does not follow that effects on income can be segregated from all the other good things that we would like people to have in their lives. On the contrary, it seems more reasonable to assume going in (it is always the safest assumption) that economic and noneconomic rewards interact, rebound off each other, and that gains in one area may well come at the expense of other equally important objectives.

We wallow in great ignorance on such subjects. For example, when curmudgeonly conservatives like me raise the possibility that it is fundamentally important that a person *earn* his own income—that earning one's own keep and pulling one's own weight are the source of deep-seated rewards and satisfactions, especially for persons who do not enjoy the limelight in any other respect—the hypothesis is too often presumed to be adequately tested by running out and administering an eight-item self-esteem scale to recipients of income transfers and then, when they seem to score as high as anyone else, saying, "Well, we dealt with that."

I would argue that we can do better, but to do so we will have to develop a richer interchange of ideas, knowledge, and methods across disciplines. I am thinking of economists and sociologists and political scientists, but most especially of a group not represented here, psychologists. Fascinating literatures are accumulating on such topics as locus of control, on the role of "competence" and "self-determination," and on the complicated relationship between intrinsic and extrinsic rewards. These bodies of research can be pulled into our ken. They inform the things we want to accomplish with programs such as the negative income tax. In some cases, they tell us about ways to provide benefits that can be more fruitful than the ones we might intuitively choose. In other cases, they can at least permit a knowledgeable dialogue about the trade-offs between income and the other aspects of quality of life that we have not systematically considered in the past.

So we have a considerable way to go, in my view, before we know what to make of the results of the negative income tax experiments. We need a new baseline for defining where "negative" begins in "negative income tax." We need to think in more detail about how a national negative income tax would differ from an experimental one. And we need to employ broader and more sophisticated dependent variables in assessing what we are accomplishing. Lacking those, we are not much better able now than we were 15 years ago to tell whether the negative income tax would be good policy.