

DISCUSSION

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Bawden and Orr present a potpourri of interesting information about three social experiments. The Orr paper also ventures out into the area of meta-science and lays down some criteria for social science experimentation. The three criteria he specifies are:

1. Relevance
2. Replicability
3. Adequacy of theory and measurement techniques

The third of these is directly out of the researcher's primer and nothing more needs to be said about it. The first two, however, warrant some comment.

Unfortunately, relevance is not operationally defined. Every time I attempted to push hard on his criteria it came out sounding like: That which is relevant is that which I think is relevant. What Orr would like to do is lay down some guidelines to select research projects which have the greatest efficacy for changing peoples lives vis-a-vis the political decision making process. Presumably the most relevant projects would be those which changed the lives of the greatest number of people in the shortest period.

At one point he argues "... an obvious and appropriate criterion in developing experimental research is the usefulness of the information to be obtained from the research as input to the policy decision-making process." He goes on to say that "this does not mean catering to political whims or pressures." But only a short distance on he cites the New Jersey experiment as an example of social research with the comment that "clearly the response of recipients' earned income will have a major impact on the cost of the negative income tax, as well as its political acceptability in terms of the dominant Puritan ethic." It seems to me that the selection of what to test for was quite sensitive to the political wind at the time the New Jersey experiment was being formulated.

The other criteria, replicability, seems to me to be a useful one for bureaucrats responsible for allocating research money and who have the potential intervention of the government into the society as a concern. There is no point in such people experimenting with what can be done for a small experimental group if there is little hope of replicating the effort for a much larger group, presumably national in scope.

Turning to the rural negative income tax experiment, I would first like to raise a few questions about measurement. It was mentioned that rental income would be imputed to home owners. In a panel study of income dynamics that has been going on at the Survey Research Center of the University of Michigan for the past two years it was found that between five and six percent of farm families get free housing in connection with their job. It

might be desirable then to estimate the value of such housing in computing family income for the negative income tax experiment.

In the same panel study we found that just under five percent of family heads had severe work limitations caused by mental or physical conditions. And another approximate eight percent had some limitation on the amount of work they could do--such things as being unable to lift heavy weights, periods of pain and need for frequent rest periods. The incidence of work limitation is likely to be much higher among the poor than for a cross-section of the population. And, if one is interested in measuring work response to a money incentive, some account should be taken of the people who are precluded from responding by medical conditions.

One final comment on measurement. All of us who have been involved in data gathering have given lip service to the desirability of doing validation work along with field studies, but we have been grossly guilty of not doing it. In the case of the negative income tax experiment it would seem that the accuracy of reported financial information is of direct substantive importance. First, underreporting of income will result in overestimating the cost of such a program and, secondly, I would predict that one of the more articulated concerns of political decision makers will be the opportunity for cheating that a negative tax scheme will afford casual workers, the marginally self-employed and those in a position to substitute income in kind for cash flows. It also seems that these opportunities may exist relatively more in rural areas than in urban ones. I would strongly urge some validation work, at least on income reporting.

Finally, I would like to raise what seems to me to be a couple of rather knotty ethical questions and ask what you are doing about them. Since some of the families you will be experimenting with will have been on welfare programs which provide them with income in kind and social services, they will presumably be giving up their nonmoney benefits in exchange for cash. Cash will increase the consumption options of the family head but might have little effect on those of a wife and children. It is entirely possible that the dietary and medical needs of dependents could suffer with the change to a cash flow. Has any thought been given to the possibility that the experimenters may wish to intervene directly to alter an internal family mal-distribution of income? Another problem which is much more likely to occur involves the acquisition of consumer debt by people who have been assigned higher benefit levels in the experiment. When the experiment ends these people will presumably end up with a much smaller income. Since the society has used them as experimental subjects, a good case can be made for not leaving them any worse off than they were before the experiment.