FIRST NEGATIVE INCOME TAX EXPERIMENT: A GIANT FORWARD STEP

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INTRODUCTION

We have two aims: 1) to describe briefly the New Jersey-Pennsylvania Negative Income Tax Experiment, including its rationale, context, and major findings; 2) to offer a critique of the study, including lessons learned and implications for social experimentation.

First we must note the perspectives of each author. Rossi was a post-experiment critic, having written an extended evlauation as part of the Russel Sage Foundation program (Rossi, Lyall 1975). It is easy to criticize and fault any piece of research. It is much more difficult to work as a *responsible* critic. Responsible criticism aims to learn from past errors, and offers suggestions which might improve future work. Sonia Wright (now Rosenbaum) was a member of the research team at the Institute for Research on Poverty, and thus represents an insider's viewpoint, with involvement in research operations covering the last year of the experiment, and in later analyses. We act as friendly, responsible critics, and do not wish to detract from the accomplishments of this study.

The New Jersey-Pennsylvania Negative Income Tax Experiment (hereafter called *NIT*) is singularly important both for its substance and as a precedent. NIT had a tremendous impact on the field of policy evlauation because it was a "first" as a randomized controlled field experiment on a social issue pertaining to public policy. Field experiements have since become almost commonplace. Large-scale field experiments are directed at income maintenance, housing, criminal justice, health insurance and supported work. The NIT experiment was a bold first try, first devised in 1967 and fielded in 1968.

In the early days of the War on Poverty and the Office of Economic Opportunity (OEO) academicians such as Rossi, Donald Campbell, and Julian Stanley tried unsuccessfully to convince OEO to start its progrms on the basis of randomized experiments, or at least, to build experimental components into such programs as the Job Corps and Head Start. The prevalent attitude then was that social programs were being designed to serve people, not social scientists. From bitter experience, it became recognized that *ex post facto* evaluations are inconclusive for social program impact assessment. Despite many evaluation efforts, we are still unable to get credible estimates for the effects of early *War on Poverty* programs. The NIT experiment paved the way for experimental evlauations because it showed that large-scale, randomized field experiments on social programs are feasible both in design and execution.

WELFARE SYSTEM DEFECTS

The United States system of public assistance has been the focus of dissatisfaction for more two decades, and welfare reform continues to be a central political concern. Nor have the criticisms changed over time. We will cite five primary defects in the welfare system (Danziger 1977; Lerman, Skidmore 1977).

Defect 1: The welfare non-system is chaotic and inefficient. It includes income transfers, such as Aid to Families with Dependent Children (AFDC), General Assistance, Social Security, Unemployment Insurance, and other in-kind benefits such as Housing Subsidies and Food Stamps. Some are means-tested, and income-conditioned, like AFDC, General Assistance, and Medicaid. Others are categorical, restricted to certain types, regardless of income, like Social Security, Medicare, and Unemployment Insurance. Each transfer and in-kind program has different operating rules, different eligibility requirements, and a very cumbersome, complex, and costly bureaucratic administrations. In the same household, one person might receive AFDC and food stamps; another SSI benefits and food stamps; another General Assistance, with each program having different rules, different accounting periods, and different filing unit definitions. Some programs are entirely funded by the Federal Government, administered and supplemented by the states. Others are primarily state and local programs. Administrative costs are staggering.

Defect 2: The system applies benefit levels inequitably across states in a way which cannot be justified by cost-of-living differentials

(Lehrman, Skidmore 1977). Hence, some believe that the present system encourages interstate migration, especially from the deep South to northern and western states.

Defect 3: Eligibility and assistance levels also vary by family structure. In the 26 states which in 1977 did not have an Unemployed Parent provision for AFDC, a family with two parents is eligible for assistance only if the father leaves the family; otherwise, the family is eligible only for food stamps. This was a perverse incentive for desertion and divorce.

Defect 4: The welfare system has perverse disincentives for work. For some, benefits are higher if a person does not work. If a family receives multiple benefits, going from part-time to a full-time job may result in a net loss of income and discontinued Medicaid benefits. The system discourages work on the part of those who are able to work (Danziger 1977).

Defect 5: The categorial nature of the welfare system systematically excludes the working poor – individuals and families with employed members whose earnings are so low that they fall below the poverty line. The system also subjects clients to an undignified means test.

THE NEGATIVE INCOME TAX (NIT)

A universal negative income tax was first proposed in the early 1960's to reform the welfare system and rectify defects (Friedman 1963). The negative income tax is defined by two parameters: 1) a guarantee level, and 2) a tax rate on earnings. The program guarantees a pre-established minimum income, dependent only on family size, which is received in the form of direct cash transfer payments. The payments are reduced according to a "tax", most often cited as 50 percent, on earnings which the family is able to generate. The payment program is determined by the formula:

$$P = G - t(E)$$

P is the transfer payment; G is the guarantee or minimum level below which income may not fall; t is the tax rate; E is earnings from work. The guarantee is adjusted for family size.

A household with no earnings (E = 0) is entitled to transfer payments equal to the quarantee level. Calculated on a yearly basis, the guarantee level may be set at the poverty level, such as $4000 for a family of four in 1970. But there is a built-in potential for work incentives since earnings are only partly reduced, allowing increased income above the guarantee level, in contrast to existing welfare programs which often disqualify households or persons with any earnings.

Under the negative income tax, total family income equals earnings plus the transfer payment. Under a NIT plan with a guarantee level of $4000 for a family of four with no earnings, the transfer payments total $4000. With $3000 of outside earnings, the $3000 is "taxed" 50 percent by reducing the transfer payment to $2500. The $2500 transfer payment plus the $3000 outside earnings yields an annual income of $5500. Those who earn more are better off in terms of total income, and the program of transfer payments serves as a protective cushion in case of severe loss of earnings. The program is in effect up to a break-even point, where negative income tax payments stop, and above which, the normal positive income taxes take effect. In the example given, the break-even point is:

$$(G / t) = ($4000 / .5) = $8000$$

A family of four would receive income support until it earns $8000. From a policy viewpoint, it is not easy to set guarantee and tax levels, since equity, generosity, and feasibility must be considered (Moynihan 1973).

A universal negative income tax program has several features intended to correct the defects of the existing disordered mix of welfare programs. It is designed to be universal, nonobtrusive, administratively efficient, and a help to the working poor as well as to those unable to work. In terms of the defects ennumerated above, an NIT program, in principle, offers administrative simplicity, especially when it can be tied to the existing tax system, thus providing a smooth transition into or out of the income support system. Inequities across state lines are eliminated if uniform standards are applied, although states with more resources could supplement guarantee levels. Its univeralist nature guards against demeaning eligibility requirements and means tests. Eligibility is based on earnings and family or household size, and not on family type. The stigma of welfare support is eliminated. An NIT program provides incentives to work, since every dollar earned increases the net income. It also assists the

working poor, a previously excluded group.

Such impressive virtues would obviously elicit support from political and academic liberals and conservatives. But there was no direct evidence that these virtues could be achieved in practice, since there were no previous attempts to institute NIT plans in the United States. Though the program was designed to produce incentives to work, classical economic theory, specifically the *work-leisure choice* hypothesis suggested that in the choice between work and leisure at the same level of income, people prefer leisure.

No direct test was possible, but there had been attempts to estimate the size of disincentives to work induced by income maintenance. Data from the 1967 Survey of Economic Opportunity (OEO) provided discrepant estimates of work disincentives of existing programs which could simluate an income maintenance program. Contradictory conclusions were drawn even from the same set of data (Cain, Watts 1973; Garfinkel 1974).

A second problem was that since no empirical test had taken place for a universal NIT, it simply was not known how to operate and administer such a program, nor what the costs would be. The particular combination of guarantee and tax levels, most desirable for creating work incentives and most feasible in cost, was unknown.

THE NIT EXPERIMENT

The OEO research staff began serious consideration of an NIT program as a substitute for the welfare system, and were receptive to suggestions from economists who proposed an experiment on the NIT, hoping to leave a heritage on which future public policy could be built (Kershaw, Fair 1976; Rossi, Lyall 1975; Moynihan 1973). The experiment was funded in 1968 at $3 million, with a final cost of $7.5 million, to run for three years as a randomized field experiment with about 1000 families. The research was designed and analyzed by a group mostly of economists, under Harold Watts, at the University of Wisconsin Institute for Research on Poverty. *Mathematica,* a private research firm in Princeton, New Jersey performed the field operation, administration, and data gathering.

A random sample of households was drawn from poverty Census tracts in Trenton, Paterson-Passaic, and Jersey City, New Jersey, an later from Scranton, Pennsylvania. Over 48,000 households were screened to enroll about 1300. The target population was defined by the following eligibility criteria: 1) a household had to include at least one adult male 18 to 58 years of age, eligible to work, and not a full time student nor permanently disabled; 2) there had to be at least one other family member; 3) household income could not exceed 150 percent of the current official poverty level, or $4950 for a family of four. Poverty lines according to family size are shown in Table 1. The proportion of blacks and hispanics in the New Jersey cities was so large as to include very few white (non-hispanic) families. Adding Scranton produced a more ethnically balanced sample. To compensate control group, attrition, new controls were added from Trenton and Paterson–Passaic, after the first year. The total sample equals 1357. Without the new controls it was 1216, with 725 in the experimental group and 491 in the control group. Subsets in the sample of 1216 were the most fully analyzed.

Random assignment was by the Conlisk–Watts (1969) optimum allocation model. The experimental plans represent eight levels of negative income tax, as shown in Table 2.

TABLE 1: POVERTY LINES, FIRST YEAR

Family Size	Poverty Line
2	$2000
3	2750
4	3300
5	3700
6	4050
7	4350
8+	4600

TABLE 2: GUARANTEES & TAX RATES

Guarantee Levels	Tax Rates		
	30%	50%	70%
50%	X	X	
75%	X	X	X
100%		X	X
125%		X	

There were four guarantee levels expressed in percent of the poverty line, and three tax rates. Of the 12 possible cells in the factorial design, four were omitted, leaving those believed to include the NIT plans of practical

policy interest. The plan with 50 percent guarantee and a 70 percent tax rate would have been too ungenerous to be worth studying, and the plan with 125 percent guarantee and a 30 percent tax rate was too costly. The experimenters, along with OEO essentially guessed about what would be acceptable NIT plans for the future. Setting the guarantee levels and tax rates were political judgments on what would constitute acceptable limits to the United States Congress after 1968.

The target population consisted of the working poor and those eligible for work but unemployed and poor by definition. The primary consideration in the experiment design was to evaluate the effects fo the NIT program, and its guarantee and tax levels, on work behavior. Thus, what was known least was the eventual labor supply response of poor workers under an NIT program. The more likely place to locate such a population is in the urban industrial centers.

The group who designed the experiment considered a number of strategies. A national sample was rejected due to cost and difficulty of administering over a large number of widely dispersed sites. Doing the experiment in a large population area such as New York City was rejected on the ground that it is atypical. The decision was to carry out the experiment in sites conveninet to Princeton; hence the choice of nearby cities in New Jersey and Pennsylvania.

It turned out that at the time the experiment was being considered, New Jersey did not have an Unemployed Parent provision for AFDC, and therefore intact families were not eligible for welfare if an unemployed father was with the family. Hence the negative income tax program would not compete with the welfare system. Unpredictably, soon after the experiment began, in 1969, New Jersey laws were changed to include an Unemployed Parent provision. To make matters worse for the experiment, the welfare plan became one of the nation's most generous in payment levels. The 125 percent poverty line plan was added to the experimental program to compete successfully with the new welfare system, which was more generous than most of the experimental plans. About a year later, New Jersey reduced the welfare outlays, and became less generous. The rules of the experiment allowed households to collect welfare, provided that they relinquished experimental NIT payments, and vice versa. Administering this provision led to serious controversy with local authorities, and to charges of fraud by NIT families (Kershaw 1972).

The payment system appeared easy to administer, but the NIT program actually turned out to be not very different from the administration of the ordinary welfare system. In some ways the NIT experiment was more intrusive. The major issue concerns the accounting period chosen as a basis for calculating payments. In the NIT experiment, eligibility was determined twice monthly. Any income support scheme must be sensitive to changes in earnings: 1) if households have losses, the support system must adjust, to be effective; 2) if the household's earnings increase substantially without entering into the payments computation, the household might be obligated but unable to refund overpayments to the system. Unusual earnings changes should also figure into the calculations without biasing later transfers.

The NIT experiment households filed income report forms with paycheck stubs twice monthly, and transfer payments were calculated on a moving average of the previous three months' earnings plus transfer payments. The payment system was sensitive to changes in income, but sluggish in replenishing income losses. If a family had zero income for a month, it was not reflected in payments for 45 days. And a means test had to be instituted. Eventually, earnings audits were also carried out to investigate false reporting.

NIT payments were relatively generous. The average annual payments for a family of four were about $1200, with considerable variation, depending on which experimental plan was applied. Considering the low earnings level of this population, the payments constituted a rise in income from 10 to 50 percent.

In addition to the pre-enrolment interview, which obtained baseline data for all households, both experimental and control households were interviewed each quarter for 12 quarterly interviews. Six monthe after the experiment ended, another interview was administered to determine current employment status, and to get qualitative judgments on the participants' experience during the study. One

other interview during the last year of the experiment was designed for households which had left the experiment. By heroic efforts, about half of this group were found.

Most of the evaluation analysis was based on the 12 quarterly interviews, with relevant controls introduced from the pre-enrolment interview. The principal dependent variables for the research, concerned labor supply response. They were measured for the head of household and three members aged 12 and over, including labor force participation, employment status, number of hours worked per week, earnings per week, and other income. These variables were specified for the preceding week at the time of interview. As the study progressed, these key labor supply variables were also measured for the four weeks preceding the time of interview. Only the last week of the period could be used, to compare with the early interviews.

The rest of the interviews contained questions on a wide range of variables which were expected to influence work behavior, or which might be affected by the experimental treatment, such as consumption patterns, personal and family health and health practices, family interaction patterns, neighborhood participation, schooling, and a large group of social psychological variables like alienation, self esteem, attitudes to work, and time orientation. These were measured yearly. In addition to quarterly interviews and twice-monthly income reports for the experimental group, a yearly interview was made around April, with an audit of the participants' income tax returns, to verify monthly and quarterly income reports. Social Security aggregate data were also obtained to verify earnings.

FINDINGS

There was a 3 to 5 percent decline in work effort. But this decline is credited mainly to withdrawal of secondary earners from the labor market. Wives tended to leave the labor market, and adolescents were not dropping out of school. There were some peculiar results. Blacks on the payment plan *increased* their work effort, and whites *decreased* their work effort compared to the controls, while hispanics fell between (Watts, Rees 1977; 1978). The differences were not great, and the overall effect on work behavior was not large. Contrary to economists' expectations, there were no consistent differences associated with generosity of payment. The eight treatment levels with combinations of guarantee levels and tax rates did not produce consistent effects. Work disincentives were not higher for higher tax rates (Watts, Rees 1977; 1978).

These results for labor supply responses to the experiment are based on a subset of 690 intact, two-parent families participating continuously during the three years. They were interviewed at the 12th quarter and missed no more than two contiguous interviews. Their work was summarized by averaging the available quarterly data for each of the three years. However, they constitute only 57 percent of the original sample of 1216, excluding "new controls." Such serious attrition losses jeopardize the internal validity of the results.

Additional labor supply analyses have been made on a slightly larger subset of 750 intact families who meet the same criteria (Nicholson, Wright 1977). The findings are essentially the same. Finally, Wright (1975) analyzed the sample of male heads of household, who completed at least one quarterly interview in each of the second and third years of the experiment, regardless of family status or continuity criteria. These subsets constituted 82 percent (n = 1119) and 73 percent (n = 993) of the full sample of 1357 households. *No work disincentives are apparent for these males in the experimental treatments, nor are there significant ethnicity differences.* But these estimates may be less stable and more subject to seasonal fluctuations than those based on a larger number of interviews and more continuous quarterly data.

Detailed analyses showed few significant non-labor supply effects. There was accelerated buying of homes and major appliances. Experimental group households bought homes at a faster rate than the control group households. These were mainly older homes in the inner city, and mobile homes. There was an interesting effect on job turnover. Young people on the program turned over their jobs much more rapidly than older persons, and more rapidly than persons in the control group. They thereby obtained better jobs. For young families, the income guarantee apparently provided a cushion enabling them to undertake a more thorough job search,

even though there might be a short-term loss of earnings. This result may account for some of the apparent reduction in overall work effort, since there was also some indication that the experimental group reached higher wage rates. In contrast, older workers remained in the same jobs. The experimental payments possibly served as a cushion for older people on poor jobs who felt they could not get far in trying to get better employment. Young adolescents were more likely to remain in school longer. There were no effects on health, medical practices, fertility, participation in organizations, family interaction patterns or any social psychological variables.

CRITIQUE

1. Perhaps the most vulnerable aspect of the negative income tax experiment was the choice of sites. How can we generalize to the nation as a whole on the basis of a few neighboring cities in New Jersey and one in Pennsylvania? Moreover, the specific sites may be especially important because of the special features of the labor market in those sites. The potential effects of a negative income tax on poor people, who are primarily on local labor markets, must be assessed in reference to specific labor market characteristics. There was a confounding between the sites and ethnicity for the experiment. Most of the whites were in Scranton; most of the blacks were in Trenton and Jersey City; most Puerto Ricans were in Paterson-Passaic. Some of the ethnic differences may reflect site differences in labor market conditions.

2. Exactly what was the nature of the treatment? Initially it was thought to be the plan as defined by the eight combinations of guarantee level and tax rate. It was assumed to be the transfer payments, as calculated by the formula. But the treatment must be conceived not only in terms of what was intended to be delivered, but also the way of delivering it. Thus, the administration of the experiment also becomes part of the treatment. The treatment, in comparison to the existing welfare system required more contacts between administrators and recipients than is the case for ordinary public welfare. The careful monitoring and extensive contact between the staff of the experiment and the participants will not typify a national program – another obstacle to generalize beyond the experiment.

3. A serious question relates to the complexity of the plans and the low level of recipient awareness of the the plans' critical features. Analysis of the effects of participants' knowledge and understanding of the treatment and the rules of operation shows that participants' knowledge level was low (Nicholson, Wright 1977). This low level of knowledge was itself correlated with experiences in the experiment and treatment parameters. To include the variable of recipient understanding in the estimate of labor supply response produces changes in some of the estimates of labor disincentives. Though these data were obtained six months after the experiment ended, and are subject to recall bias, they do suggest that the treatment should be conceptualized and measured more precisely.

4. In New Jersey, the NIT experiment was confounded by changes in site welfare laws, and the fact that the experiment had to compete with welfare. Households eligible for welfare could enter and leave the experiment if they preferred welfare assistance. Less generous plans were dominated by more lucative welfare income, when the full mix of welfare benefits is recognized. Thus, the effects of the NIT experiment should be calibrated against the available alternatives in New Jersey and Pennsylvania.

5. A serious criticism is a measurement problem which might have been avoided, had the research staff included more experienced survey researchers. Though the major interest in the experiment was on work effort, work variables were very badly measured. At first, the referent period was just the one week prior to the interview, later expanded to the preceding four weeks. The questions used were those of the Census Bureau, valid for population surveys, but insufficient and imprecise for measuring individual household income and work effort, especially for the poor. Many low-paying jobs are paid on a piecework rate, not hourly rates. This method of payment is not accurately determined by the standard questions. The difference between gross and net earnings was not clearly explained. There is evidence that some respondents reported net rather than gross earnings at the beginning of the study, and later began reporting gross earnings with

more proficiency in answering interviewers. Wage rates were not asked directly, but were constructed by dividing earnings by hours of work. The complexity of accounting for more than one job, overtime work and piecework rates was not anticipated by the researchers. Some defects were met by revision of later questionnaires, but the detailed and more precise data obtained near the study's end were not comparable to those obtained at first.

6. Detailed pre-experiment work histories were not obtained until the end of the study, nor is anything known about the work histories of the households during the two months between quarterly interviews. Recognizing the potential measurement error, the results of 3 to 5 percent disincentives may underestimate or overestimate actual effects.

7. A most serious limitation of the NIT experiment is its high attrition rate. Losses were experienced despite substantial incentive payments. Experimental households received $10 for filing each biweekly income report form; control households received $8 for filing current addresses each month; each household received $5 for responding in each quarterly review. Thus, households in the experimental group who faithfully reported and were interviewed received annual bonuses totaling $260 for filing reports, and $20 for interviews. Even so, 20 percent of the sample had dropped out by the end of the experiment, including a 25 percent loss in the control group and a 15 percent loss in the experimental group. Besides households dropping out, some missed one or more quarterly interviews each quarter. When analysis is restricted to households continuously reporting, the sample size may be reduced as much as 43 percent.

8. Some critical outcome variables other than work effort were poorly measured. The social psychological variables appear to have been thrown together by borrowing from existing instruments without regard to transfer of understanding from middle class student populations to economically poor adults in the experimental group. There was no adequate pretesting, and no a priori theoretical expectation that such variables would be sensitive to the experimental treatment.

9. As in other exploratory research, only a small fraction fo the data collected appear to be useful. And an even smaller fraction were ever analyzed. This high ratio of usable information to usable variables is typical of exploratory research. Few of the principal investigators were experienced survey researchers. Whatever the ultimate explanation, the outcome for the study was to place a research burden on respondents which was only partially compensated, as suggested by the high attrition rates.

CONCLUSIONS

The lessons of the New Jersey-Pennsylvania negative income tax experiment derive partly from its substantive findings and partly from technical methodological experiences. Both types of implications make this first venture into field experimentation a major event in the history of social science research.

On the substantive side, one can be confident that there are no massive work disincentive effects to be expected from a negative income tax plan. Within the range of guarantees and tax rates tested in NIT, very few of the working poor will leave the labor force to subsist entirely on the guaranteed payments. Due to measurement difficulties and site choices, it is not clear whether the small work disincentive effects found here were underestimates or overestimates of the true effects. Later NIT experiments in Seattle, Denver, Gary, and Winnepeg, Canada help put reasonable bounds on these estimates.

The experiment has provided very useful information on the administration of NIT plans. Despite the hopes of early advocates, NIT plans may be only slightly easier than AFDC to administer. Means tests apparently cannot be avoided if payments are conditioned on income. Checks cannot be issued by computer according to a set of simple algorithms. Social workers as professionals, under an NIT plan may become more clerical than social.

The work disincentive issue is not the only sticking point impeding broad acceptance of NIT. The experiences of NIT researchers testifying before the United States Congress in Family Assistance Plan hearings were rather disappointing. After testimony about the small work disincentive effects, legislators showed as much interest in recipient fraud and general labor market effects. Conservative opposition to NIT simply shifted ground to other issues.

CONTRIBUTIONS

1. The most important contribution was to advance considerably the acceptability of field experiments on social programs. Randomized field experiments are clearly now removed from the realm of dreams to workaday reality, and are widely accepted as the correct approach to test prospective social policy.

2. We have learned that field experiments are difficult and expensive. Particular attention must be given the problem of attrition where experiments are extended in time. Longitudinal studies are vulnerable to data overload, and to acute problems with missing data. These problems have sparked considerable work on the knotty problems of handling such data bases and analysis models.

3. We have learned that treatment in a field experiment becomes complicated by its administration. This finding has resulted in later experiments using administrative arrangements as an experimental variable. This move promises considerable knowledge about how to design social interventions.

4. We have learned that the politics of evaluation are as important as the purity of experimental design. It is not possible for researchers to proceed with field experiments on social programs without policy maker input into the parmeters of the program to be tested. One of the failures of the NIT experiment was that it was conceived isolation from the policy making process. While recognizing the difficulty of bringing politics into field experimentation, failure to do so may lead to making it simply another form of basic research performed under the guise of policy relevance.

In sum, the NIT experiment represents both a giant step forward in applying social science research methods to policy isuses, and at the same time, a flawed venture. We profit from both. On the basis of its success, we have been able to establish field experiments as the top of the state of the art in evaluating social policies. Building on its failure, we are learning how properly to conduct future field experiemnts.

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