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GRADUATED WORK INCENTIVES:
AN EXPERIMENT IN NEGATIVE TAXATION

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ABSTRACT

        For the most part, economists have had to be content
with data produced as a bi-product of private and public
administrative and regulatory activities--augmented more
recently by survey data. In both cases the range of the
data collected has had to be determined by the "accidental"
experiments of reality, and historical evidence simply
does not always provide direct enough evidence to answer
important questions. In such cases the possibility of
experimentation ought to be considered. This paper describes
an experiment which the Institute for Research on Poverty,
in cooperation with MATHEMATICA (a private research corpor-
ation in Princeton, New Jersey) is conducting in urban
New Jersey.
        The primary rationale of the experiment is to fill a
gap in our knowledge of the labor-supply function. Static
economic theory does provide some qualitative answers to
our questions about the effects of redistributive transfers
made to households whose present income is almost entirely
earned. But even within that framework, the "natural
evidence" is inadequate for making a responsible quan-
titative forecast of the impact of a negative income tax--
or family allowance, or guaranteed annual income--on work
effort. Other economic effects will also be investigated,
along with a host of non-economic ones--such as social and
political participation, alienation, time-preference, family
stability and so forth.
        The experimental sample is composed of intact families
with at least one working-age male, and is drawn from the
poverty areas of three SMSAs in urban New Jersey--Trenton,
Patterson-Passaic, and a third one as yet undecided.
        The first benefit payments were made in August 1968.
By Spring 1969 there will be almost a thousand families
receiving, or eligible to receive benefits; and there will
be in addition several hundred control families. The pay-
ments are now scheduled to continue for three years, and
the total cost of the experiment will be around five million
dollars.

GRADUATED WORK INCENTIVES: AN EXPERIMENT IN NEGATIVE TAXATION

By this time many people are aware---by rumour, magazine and news-
paper, if not from personal contact---of an experiment being carried
on in New Jersey on the subject of negative income taxation. This
paper will confirm these reports and give an account of the motiva-
tion behind this study, and of the contribution it is expected to
make.

The first benefit payments were made to an initial group of
sample families in August, 1968. By Spring 1969 there will be almost
a thousand households receiving, or eligible to receive, income-
conditioned benefits of a negative income-tax variety. There will be
in addition several hundred control families. The payments are now
scheduled to continue for at least three years, and the total cost of
the experiment (including both research and benefit payments) will be
around five million dollars. The experimental sample is being drawn
from the poverty areas of three SMSA's in urban New Jersey---the first
in Trenton, the second in Patterson-Passaic and the third still is to
be chosen.

Experiments of any sort---let alone ones that cost five million
dollars---are usually considered outside the realm of economic research
methodology. Therefore, for reasons of novelty if nothing else, there
should be some interest in a more specific description of this enter-
prise. There is, at least in the minds of its promulgators, a care-
fully-considered rationale supporting the need for such an experiment
and guiding its design.

In the discussion below I would like to present this rationale, describe the experiment briefly, and conclude with some observations about the role of experimentation in the further development of our discipline.

I must acknowledge at the outset that this is a joint venture involving personnel at the Institute for Research on Poverty, MATHEMATICA, and OEO. In serving as spokesman for the experiment I cannot take extraordinary credit for any merit or demerit you may see. I am, of course, fully responsible for providing an accurate and balanced description.

INTRODUCTION

Economics, as one among the social sciences, has a tradition of concern with the improvement of public policy. Its former name— political economy—can be interpreted as a direct reflection of this concern. But the very structuring of economic relationships and models reflects more substantively than the other disciplines the concern with finding answers to questions that begin "What would happen if we changed our policy to . . .?" We are continually developing, or adapting, our theoretical frameworks to make them relevant to current problems. And we are also seeking empirical evidence to test these frameworks and/or provide some quantitative precision for them.

For the most part, economists have had to be content with data produced as a bi-product of private and public administrative and regulatory activities. In recent years such sources have been

augmented by survey data of various kinds. But in both the former and
the latter case the data collected have their range determined by the
"accidental" experiments of reality. Sometimes this process provides
evidence adequate to answer the question at hand. Sometimes, indeed,
it is overabundant. But there seems to be no basis for assuming that
sufficient unto the task is the data therefor. There are indeed
cases where historical experience simply does not provide direct
enough evidence. In these cases, and perhaps only in such cases, the
possibility of experimentation ought to be considered.

The experiment in New Jersey--to come to our concrete example--
finds its primary rationale in filling a gap in our knowledge of the
labor-supply function. The revival of interest in direct income
redistribution, sparked by the War on Poverty, raises acutely import-
ant questions about the effect of redistributive transfers made to
households whose current unsatisfactory level of income is mostly
obtained through their own sweat.

Static theory does provide some qualitative answers to our ques-
tions--specifically that the beneficiaries of a redistribution will
tend to reduce their labor supply. But even within that framework the
"natural evidence" is inadequate for making a responsible quantitative
forecast of the impact of a negative income tax--or family allowance, or
guaranteed annual income. There are also questions about the basic
adequacy of the static income/leisure choice model that cannot be
satisfactorily tested with available data. Estimation of the labor--
supply response requires information on the behavior of households
with different earning capacities enjoying the same level of unearned

income, and also households with varying amounts of unearned income facing similar earning opportunities. The usual cross section turns up an insignificant amount of unearned income in the lower ranges of earning capacity, and what little there is tends to be either extremely transitory (gifts, prizes, inheritances), or property income that is the result of choices highly endogenous to the household's intertemporal allocation of its resources. For both of these reasons it would seem hazardous to project the apparent effects of such income to the case of a wholly exogenous and ostensibly permanent shift in the household's opportunity locus. Similarly, it is hard to find plausible analogues for the drastic change in the marginal trade-off rate between leisure and income that would be exogenously induced by the negative-income-tax schemes currently under discussion.

We have here then, I would argue, a case where important public-policy decisions may soon be made. Reliable information about both the cost and income-augmenting effect of specific redistribution plans is a sine qua non of any well-informed choice; and both these depend on the labor-supply response to particular features of these schemes that are not well approximated by any phenomena occurring "naturally" in our economic and social system. Given this basic situation, all our cleverness and wit will not draw what we need out of our usual sources of empirical raw materials--which leaves us with a very strong case for inducing experimentally the sorts of changes that will permit us to make the crucial inferences.

When one considers seriously the prospect of singling out a sample of households and giving them cash-transfer payments of various kinds

and amounts, one immediately has apprehensions about the validity of
their actions as an indicator of the response we should expect from the
full-scale application of such measures. Misleading artifacts of the
experimental situation, often referred to as Hawthorne effects, could
(and some are saying certainly will) make the evidence valueless.
There seem to be examples, such as the Western Electric experience, that
are constantly brought up as reasons for despairing of experimental
activities in largely uncontrolled environments. Such doomsayers
notwithstanding, social experimentation of this kind is so rare that
we really don't know much about these hazards. Nobody even knows
whether three years is a long or short time horizon for the people
we are interested in.

The experiment described below has been designed with the threat
of Hawthorne effects vividly in mind. Several features of the design
are explicitly aimed at minimizing these; and if we knew further
ways to reduce them we would make added adjustments. But there has
also been a stubborn (I suppose) refusal, given the depth of ignorance
about labor supply, to admit defeat before the fact.

DESCRIPTION

Stated broadly, economists and other social scientists ought to
be able to make scientifically-based statements about the social and
economic effects of a radical change such as the introduction of a
major redistributive program. On the economic side, we can say that
such a program will change the "opportunity locus" or "budget lines"

for individual choices between leisure and income, spending and saving, investing and consuming. Clearly, the outcomes of these choices are crucial to the performance of our largely decentralized economic system, as well as to the future development (or fortunes) of the choice makers.

The experiment has been designed primarily to investigate the leisure/work choice, largely because of the relative economic importance we place on it, but partly also because fears about work-effort implications loom so large in the mind of the public at large and the politician in particular. Other economic effects, however, will also be investigated along with a host of non-economic ones—such as social and political participation, alienation, time-preference, family stability and so forth.

Given our primary concern with labor-supply response, the experiment has been limited to households in urban industrial areas which include at least one working-age (18-58) male who is neither a full-time student nor permanently disabled, and at least one other family member. The eligible population is further limited to those whose "normal"* income places them in the poor or near-poor categories (less

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*As used above, "normal income" refers to an empirical approximation to a long-run income concept such as Friedman's Permanent Income. We have been developing regressions for describing the average "relation" between family income and a fairly eclectic set of household characteristics. This "relation" has been fitted so as to yield a good approximation at the low end of the income distribution. We propose to use an interpolation between (1) a household's predicted income by this regression and (2) its actual income over the most recent year as reported on the screening interview, as our "normal income." Turned around the other way, we are attempting to adjust the short-period income figure, that we measure directly, according to its consistency with other, non-income measures of the households' income potential.

than 1.5 times the official poverty thresholds). Experience so far suggests that as much as 80 per cent of the eligible households will have normal incomes above poverty--i.e., between the poverty threshold and 150 per cent of it.

The ethnic distribution of our eligible households has been something of a surprise. In Patterson-Passaic, 41 per cent of the eligible families are Negro and a disconcerting 44 per cent Puerto Rican, leaving only 13 per cent of available households to represent the non-Puerto Rican whites--a group which, according to most recent national survey data, still comprises a majority of the poor even in urban areas. This is a matter of some concern since we would like, at the very least to have a sufficient portion of the sample in the "native white" category to detect any ethnic differences in response there may be. The composition of eligibles in Patterson-Passaic is given in more detail in Table 1.

Table 1

Percentage Distribution of Eligible Households in Patterson-Passaic

	Poor	Near Poor	Total
Family Size			
Small (2-3)	3	17	20
Medium (4-5)	5	33	38
Large (6+)	10	32	42
Race			
Negro	6	35	41
Puerto Rican	9	35	44
Other White	3	10	13
Not Ascertained	0	2	2
Total	18	82	100

The group we have chosen as our primary target for experimentation--intact families with at least one working-age male--bulks large in the total poverty population and is also, in the main, outside the eligible categories of existing welfare programs. It is perhaps fair to say that this "working poor" group is excluded from such programs precisely <u>because</u> of fears about their labor-supply. From the experimental point of view, these are the households most likely to have observable changes in their labor supply, because these are the household heads with no income alternative up to now except full-time work if they can get it. Female and aged heads have historically contributed less to the labor market.

Homogeneity is another criterion that has influence our selection of the experimental group. Clearly one can carry homogenization to ridiculous extremes and end up with a group so narrow that the outcome is of no interest. But on the other side, an experiment of this sort is such a novel undertaking that it needs all the help it can get. There are no obviously outstanding economies in trying to cover the whole population at a single shot, so we have taken the prudent course of applying this largely untried methodology to a limited group. We have achieved important simplifications by excludikng certain groups. In some cases the excluded groups--the rural poor is a primary example--should be candidates for further experimentation (indeed plans are currently underway for such an experiment). In this sense the New Jersey experiment should be regarded as dealing with an important (and hopefully tractable) part of the problem, but requiring supplementation from further experiments. The main groups we have excluded are

unattached males, female heads, aged heads, and all rural poor. We are also giving only perfunctory treatment to the intractable (but important) self-employed.

Households meeting our criteria and selected for our sample are either assigned to a "treatment"—which is one of a range of nine linear transfer schemes—or to a control group—which, of course, is merely observed, being given a nominal payment for the time taken up by each interview. Any specific linear scheme can be described by two numbers—the tax rate r, and a scalar, g, which indicates the level of the maximum benefit or income guarantee. The dollar amount of the income guarantee for a specific family depends on household size and on the value of g for the plan assigned to that family. Table 2 shows the basic guarantee schedule for families of size 2 to 8+ (single individuals are not, of course, included in the sample). The actual guarantee, then, is the product $g \cdot G(n)$. The household's net benefit is calculated as the difference between the maximum benefit $g \cdot G(n)$ and the product of the negative tax rate and income. The net benefit diminishes and finally vanishes at the break-even point $\frac{g}{r} \cdot G(n)$.

Table 2

Basic Guarantee Schedule

Household size, n	Basic Guarantee, G(n)
2	$2000
3	2750
4	3300
5	3700
6	4050
7	4350
8 or more	4600

Table 3

Experimental Combinations of Tax Rate and Guarantee Level

	r	g	g/r
I	.3	.50	1.67
II	.5	.50	1.00
III	.3	.75	2.50
IV	.5	.75	1.50
V	.7	.75	1.07
VI	.5	1.00	2.00
VII	.7	1.00	1.43
VIII	.5	1.25	2.50
IX	.7	1.25	1.79

Current plans call for nine different combinations of g and r, specified in Table 3. In these plans the tax rate ranges from .3 to .7, and the guarantee level from 50 to 125 per cent of the basic schedule in Table 2. Table 3 also shows the break-even levels as fractions of the basic schedule. The basic guarantee schedule is scaled for four- and five-person families at the level of the official poverty-threshold levels, but it departs somewhat for larger and smaller families. Experimental combination IX is roughly comparable to the welfare rates applicable to families eligible for the new AFDC-UP program in New Jersey. Only the first seven plans are currently in operation in Trenton. We plan to add the last two plans in our second and third cities.

As with any tax system, definitions of the unit, and of the income of that unit, are important components of the program. Being important,

it could well be argued that some experimentation should be done to ascertain more precisely the effects of alternative definitions. Again, we have chosen not to complicate things further in this way, but to use our best judgment in specifying a single set of rules, given the precedent and experience of both the positive tax system and existing welfare programs.

In defining the eligible unit, the decision was made to include (besides the spouse) any child or stepchild or a descendant of any child or stepchild if such a person is either living with the head, or derives more than half his support from the head. In additon, any other person who both lives with, and derives more than half his support from, the head is included. This definition conforms fairly closely to the economic notion of a family decision-unit and is also close to the census-type family unit underlying the statistical tabulations we are all familiar with. It <u>does</u> differ from the positive income-tax definition in that our basic unit is the family, while in the positive income tax it is considered the individual (this has only been partly qualified by joint filing--which is optional).

Because of the peculiarities of an experiment, as compared with a full implementation of such a law, some additional features must be included to eliminate the possibility of uncontrolled additions to the experimental sample. Consequently, no new individuals who join the eligible household after initial eligibility has been established will be included for our purposes (except children born to an eligible female, and any minor child who joins the unit after an initial waiting period of six months). Initially eligible unit members who join the armed services, or leave the United States, or are institutionalized

(except those voluntarily institutionalized and who remain dependent upon the tax unit for support) will cease to be unit members while in such status. Arguments in favor of making persons marrying an eligible person also eligible can be made, but we have decided against this, because such a provision would produce (in our experimental setting) an effective dowry which could distort normal mating patterns purely as a result of the artificial setting.

It is impossible to simulate the family-composition incentives of a full-scale program in the experiment. The best we can do is to be as neutral as we know how, relative to the status quo. We may be able to get some general information about how sensitive such choices are, but nothing more.

Before moving on to the income definition, it should be explicitly mentioned that the definition of the unit bears on the level of benefit calculation in two ways: (1) through its implications for family unit size, and (2) through its implication for the level of income--because income received by all members of the unit must be included in the income calculation.

ITEMS ADDED to the definition of current taxable income for the experiment represent a substantial modification of the definition of income used in the positive tax system. Our concept will include:

a. any pension or annuity (including Social Security)

b. all prizes and awards

c. all life-insurance proceeds over $1000

d. gifts, support payments and inheritances over $100 from persons outside the family unit

e. all interest on governmental obligations

f. damages, insurance payments or workman's compensation due to injury or sickness including wage or income continuation

g. all dividends

h. fellowships or scholarships, including value of room and board supplied without charge, to the extent that such stipend exceeds the costs of tuition, fees and books

i. income from trusts and estates

j. gross rental value of owner-occupied housing or other quarters occupied rent free

k. capital gains counted in full as income, and losses to be deducted to the extent of gains received during the period of the experiment

l. payments received from

i) unemployment compensation

ii) strike or supplementary unemployment benefits

iii) Social Security Survivors benefits

iv) veterans disability benefits

v) training stipends

SPECIFIC EXCLUSIONS are the following benefits and others for which the size of the benefit is based on demonstrated need:

a. aid to the permanently and totally disabled

b. old age assistance

c. aid to the blind

d. aid to families with dependent children

e. general welfare and public assistance

An additional word should be said about (d) and (e). When our experiment was planned, New Jersey had no Unemployed Parent provision

in their welfare programs. We, therefore, did not have to worry about

welfare payments confusing our observations, since their only significant

welfare program was AFDC and we were not concerned with households headed

by females. Time caught up with us, however, and starting January 1,

1969, New Jersey instituted an AFDC-UP program—the benefit-level of

which is higher than all but our most generous plan. We have been

forced, therefore, to reconsider our previous cavalier decision simply

to ignore competing welfare payments. We have adopted the following rule:

> Any family in our sample who becomes eligible for AFDC-UP
> benefits will remain in our sample. During any month for
> which they elect to receive those benefits, they cannot
> receive any payment from us. They do remain eligible
> to return to our plan whenever (and as soon as) welfare
> payments stop. [As New Jersey's welfare laws are written,
> this provision does not affect a household's final dis-
> posable income—New Jersey would offset any amount we
> paid to welfare clients anyhow. It does, however, ensure
> that we are not simply reducing New Jersey's welfare outlay
> in return for observations of no use to us.]

DEDUCTIONS are limited to the following:

a. Businessmen and independent contractors may deduct costs
   of earning income except if the net income so derived is
   below the New Jersey minimum wage, that wage rate will
   be imputed to them.

b. Mortgage interest and real property taxes may be deducted
   in full in computing income from owner-occupied housing.

c. Payments of alimony to persons outside the tax unit may
   be deducted.

d. Finally, full credit (reimbursement) will be allowed for
   all federal income taxes paid or withheld, and reductions
   in benefits will be made to offset any tax refunds received.
   Thus the recipient's unit will be insulated from the regular
   tax system until its income reaches the level at which the
   unit will fare better under the positive tax system.

This definition aims at including most components of "total family

income" as measured for census purposes. This broadening seems to be

called for in a program aimed at relieving genuine need as compared to tax-exempt status. Some of these provisions will have little if any application in the experiment, but they do need to be spelled out to take care of the surprising exception.

It may be useful here to outline very briefly the sequence of events involved in a family's becoming enrolled and operating under one of the experimental plans. The first contact will be through a screening survey to identify a group of eligible households. Very short interviews are administered to a random sample of households in the poverty areas aimed at eliciting just enough information to determine the household's eligibility for inclusion in the experiment. Our experience in the poverty areas has been that 10 per cent or less of the screened households are eligible. A family that *is* eligible will be contacted again (if it is selected)---in a second-stage stratified sampling process--for enrollment in one of the experimental plans or the control group. It will first be interviewed at some length (the pre-enrollment interview) to provide base-line data (pre-treatment measurements).

Very soon after this interview the family will be visited by a member of the experiment staff who explains the general features of the experiment and its role as a part of research on methods of income maintenance. He then proceeds to explain in detail the plan being offered to the particular family he is speaking to. The family is then asked for its agreement to participate and is given an opportunity to confirm the legitimacy of the offer if it wishes to do so. If they do agree to join the experiment, they are given their first benefit check

immediately, and another one every second week. Every fourth week

they are required to report their income for the preceding 4-week period.

At the time of enrollment they are also given a schedule which shows in

some detail the size of the benefit they are entitled to at each level

of reported income.

So long as the household continues to report monthly, and send in

the relevant pay-stubs, they continue to receive benefits for a three-

year period--which come through the mail in the form of bi-weekly

checks, drawn upon the account of the Council for Grants to Families.*

Their continued eligibility does not require that they stay in New

Jersey. We are prepared to continue benefit payments wherever they may

go, within the continental U.S., provided only that they continue to

send in their income reports.

If their income rises above the break-even point, they are of

course entitled to no net benefits, but they will remain eligible

should their income subsequently decline. In an effort to maintain

contact with households receiving no benefits we do continue to

give a minimum payment for their trouble every time they send in an

income statement, and allow them to change to a quarterly income-

reporting period until such time as they are again eligible for benefits.

Since it is undesirable, and probably not even feasible, to get a

contractual agreement that limits their future choices, we are mostly

dependent upon making it financially attractive for them to continue

to participate. The related problem of competing welfare payments

has been dealt with above.

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* A wholly-owned subsidiary of MATHEMATICA.

Each household in the experiment, including control households, is interviewed every three months. These interviews last about an hour and will, except for the same short section on labor-force activity every time, vary from quarter to quarter over the 36-month period, depending on how often data have to be gathered on specific questions. (Children's performance in school, for instance, will hardly need to be checked every three months; and past biographical data need to be collected only once.) These interviews provide the main source of data for analyzing both economic and non-economic responses to the treatments. Along with the pre-enrollment interview and the monthly income and family-size reports, therefore, there will be a large quantity of longitudinal evidence on each family.

Co-operation with the quarterly interview is not a requirement for receiving benefits, and, of course, the control group does not receive benefits anyway. Consequently there are the usual problems of maintaining a panel of respondents over a three-year period. Cash payments will be made for each interview and additional efforts made to enable us to maintain contact with those who change residence.

There is an inevitable conflict between our desire to gather substantial amounts of information, and our intent to place no welfare-type restrictions on the use of benefit payments or, indeed, on the activities of the recipient households in general. The only requirement for benefits is prompt and accurate reporting of income and family composition. We are, therefore, making every effort to keep the administration of payments separate from the collection of data both in fact and in appearance. (There are indeed two nominally separate organizations

dealing with the households.) No doubt either the actual behavior or the response to the interview will still be distorted to some extent by this process, but we expect that such bias will become small over time as the sample becomes accustomed to the routine.

As currently operated, there is a local office in each experimental site. This office serves during the screening process as a headquarters for a large interviewing crew. It also provides a base for efforts to establish contacts and, with luck, a degree of rapport with local figures of authority. Such contacts can provide useful assurance to experimental households at the time of enrollment. After the experiment is underway these offices serve to answer questions, to receive complaints of non-receipt of benefits etc., to assist the experimental households with their income reports, and to investigate failures to report.

Supervision of operations is centered in Princeton, under the responsibility of MATHEMATICA, Inc. Housed there are the claim and disbursement activities, and the data coding, editing, and processing functions, along with the general management operations.

The operations at the Institute for Research on Poverty are limited to planning, design, questionnaire formulation, and analysis (all of which are carried on jointly with people at MATHEMATICA), plus the overall financial management and ultimate responsibility.

As mentioned above, households will be drawn from a stratified sample of eligible households and assigned to specific experimental variants, or to the control group. Defining and solving the problem of assigning households in an optimal manner is a significant study in its own right. We have stated the problem as one of minimizing the

informational output per dollar of cost. The "information" being

minimized is specified as increasing with the inverse of the expected

forecast error for a projection of total disbursements under a full-

blown implementation of a similar transfer program. Taking the static

or no-response estimates of such disbursements as given with no error

(or at least irreducible error), we are mainly concerned with the

incremental costs produced by induced changes in earnings and other

income sources.

It is possible, by using suitable restrictions, to specify the

marginal information value of an additional household with a given

"normal" income assigned to each of the experimental groups. The cost

of such as assignment depends on the household's "normal" income, its

size, the generosity of the particular transfer plan, and alas, the

very same labor-force response the experiment is intended to illuminate.

We have been able to use a range of guesses about response in order to

carry out the optimization. Our best judgment is that most of the

uncertainty lies in the level of these responses, rather than in their

relative magnitudes. If this turns out to be correct, our optimization

model will provide a good pattern of assignment. We can hedge our

limited budget by making a conservative guess about the costs, and then

extend the sample (and/or the time period) if actual responses are

enough like our a priori estimates.

With the several parametrically-defined variants of the treatment,

we shall be able to utilize regression techniques for exploring the

response to tax rates and guarantee levels. In an important sense the

estimates will be based as much upon differential response among the

experimental variants, as between each of these and the "unpaid" control
group. This provides an effective means of minimizing biases from
"Hawthorne effects"--if such effects are additive they do not affect
inter-group comparisons among the experimental groups.

By taking advantage of the longitudinal nature of our data, we can
also observe the time pattern of any adjustment induced by the experi-
mental treatments. This may give us some further clues about the
validity of the observed response as well as simply extend our know-
ledge of such processes. Our first approximation holds that the most
reliable indication of the "steady state" consequences of redistribution
will be produced in the middle of the experimental period--after the
novelty has worn off and the adjustments completed, and before the
anticipation of the end of the benefit period contaminates the results.

With the experiment described above in mind, it is worth considering
the distinction between an experiment and a "demonstration." In the
former there are well-specified hypothesis tests or estimation tasks to
be carried out from the experimental evidence. Building from available
knowledge, the object is to extend that knowledge in quite specific
ways. In the latter, the important questions have either been answered,
or not asked, and the object is to convince a wider public that a spe-
cific pre-selected program won't sour the milk. The emphasis is on
extending the recognition or acceptance of a policy or program and
not on extending the knowledge needed for selecting alternative policies
or programs.

One cannot readily carry this useful distinction to the point of
identifying pure types--many "demonstration" projects can and do

extend our knowledge. Similarly, an experiment such as the one des-
cribed above is producing experimental variation in some dimensions
and not in others--and in this latter category may be said to be pro-
viding a demonstration of feasibility. Many of the administrative
details such as monthly income reports, non-standard income definitions,
etc., will be "demonstrated" as workable, or abandoned. But despite
the fact that experiments and demonstrations usually occur in some sort
of mixture, they should not be treated as synonymous. Experimentation
has a great potential as a source of scientific evidence on social and
economic behavior. As such it merits consideration as an addition to
our research tool-box. The potential of "demonstrations" seems much
more doubtful.

CONCLUSION

At the present moment there is little to report of a substantive
nature beyond the fact that almost all of those who have been invited
to participate in the payments program have chosen to do so, and so far
most of them are reporting their incomes promptly--and moreover still
have an income to report. No immediately apparent outbreak of hives
or plague of locusts has been induced. From a scientific point of
view the most encouraging result is that with enough patience, under-
standing, and luck, it is feasible to initiate a social experiment that
substantially (1) alters the alternative choices of some households
and (2) introduces several graduated treatments as well as the null or
"control" treatment. At the outset, such an undertaking within troubled

center-city areas appeared formidable--and still does--but at present
we remain convinced that meaningful experimentation of this kind can
be carried out.

Without generalizing from a sample of one, I am encouraged to
report that this project--whether because of its importance, novelty,
or challenge--has succeeded beyond anything in my experience in bringing
a variety of academic scholars from diverse disciplines into concerted,
prolonged and fruitful contact. Interdisciplinary activity has a
rather bad name and may richly deserve it. But there are conditions
under which it can be successful. From the point of view of the social
scientist this example also provides a most welcome assurance that it
is possible in yet another way to make a substantial contribution
toward solving urgent social problems from our so-called ivory tower.