An Experimental Study

of the

Negative Income Tax

by

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ABSTRACT

This thesis analyzes the design and early months of operation of an actual field study of the negative income tax funded by the U. S. Office of Economic Opportunity and carried out in several New Jersey and Pennsylvania cities. Chapter I presents a background discussion of various alternative income maintenance strategies. Chapter II discusses design decisions reached during a preliminary planning stage of the experimental study. Chapter III considers questions of practical importance for the field operation of the study. In Chapter IV, design modifications based on the first year's experience with the experimental program are examined. Chapter V analyzes several mathematical models for the allocation of sample families over negative tax treatments. Finally, Chapter VI concludes with a summary of the major design issues in the experiment and a preliminary evaluation of the decisions reached on each.

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Finally, I would like to thank Mrs. Rita de Clercq Zubli for excellent typing of the final manuscript.
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INTRODUCTION

In June of 1967, the U. S. Office of Economic Opportunity awarded a grant to the Institute for Research on Poverty at the University of Wisconsin for a Graduated Work Incentive Experiment. Shortly thereafter, the Institute signed a contract with MATHEMATICA, a Princeton, New Jersey research firm, and the experiment, a scientific field study of the negative income tax, began to take shape in New Jersey.

Only the broadest outline of this unprecedented research effort had then been set. Major questions of designing, operating, and evaluating the pilot program remained to be worked out, offering a unique opportunity for contribution to both science and policy. My analysis as a participant in the design and operation phases of the study is the substance of this thesis. In following chapters, I will discuss the development of the experiment from its early planning stage in the fall of 1967 through the spring of 1970, when the program was fully operational and its main features essentially fixed for the duration of the study.

Chapter I is a background paper on the need for a new national income maintenance strategy, and the need for a pilot project to help fashion such a strategy. Chapter II discusses design decisions reached during the preliminary planning stage of the New Jersey experiment, before the move to field operations. Chapter III considers field work decisions of major importance to the experiment, particularly the operation of interview and payment mechanisms. Chapter IV examines the first year's operation and the several significant design adjustments which it produced. Chapter V present several mathematical models for the allocation of sample families over experimental
treatments and discusses the actual allocation adopted for the experiment. Chapter VI concludes with a summary of the main experimental design issues and a preliminary evaluation of the decisions made on each of them.

In each of the chapters, I will draw on working papers and memoranda written by me at the time. This approach conveys a chronological sense of the experiment which is important to understanding the features of the program as they finally emerged, and provides a useful ordering of the multitude of topics on which coordinated decisions had to be reached. It also follows my own view of the experiment's progress, making this a sort of professional record tracing my involvement from early advocacy of income maintenance experimentation, to preparation of proposals for studying the negative income tax, on through three years of work on the New Jersey experiment.
Chapter 1

BACKGROUND: MAY 1967

In November 1966, nearly one out of every ten workers in poverty areas of thirteen major U.S. cities was unemployed. In addition, nearly one out of four workers in these areas was either employed part-time though he wanted full-time work, earning less than $60 a week at a full-time job, or discouraged from entering the labor force at all. Thus 34% of the potential working population of these urban slums was termed subemployed by the Labor Department.2

Right now, national estimates of this subemployment run as high as 13 million persons. But at the same time, increased efforts are being made to put these valuable human resources to better and fuller use. The recent upward trend in public expenditures for education, training, and rehabilitation reflects a growing awareness of the magnitude and urgency of our manpower problem. Estimated Federal outlays for a broad range of employment and opportunity programs in fiscal 1968 total $4,764 million — an increase of two-thirds over the actual fiscal, 1966 expenditures for such programs. Old programs are being improved, expanded, and redirected toward the disadvantaged, while tailor-made new programs, such as the Labor Department's Concentrated Employment Program, are being introduced and built up to strength. These accelerated

attacks on unemployment and poverty reaffirm a policy which has concerned this country since the Depression, when one-third of its citizens were ill-fed, ill-housed, and ill-clothed. Today the poverty population, albeit by updated measures, had fallen to one fifth, and the nation has made an official commitment to expedite its further decline.

Why in these circumstances, with fewer and fewer people left in need every year, should policy-makers branch out from strategies which have already proven successful in eating away at poverty? An answer can be seen in the estimate that, at an absolute maximum, 1,400,000 people can be served by Federal manpower programs in fiscal 1968. Some of these slots will not be filled. Some of the people who do enroll will not finish training for one reason or another. Some will not be able to earn a non-poverty wage even if they do. And some may not be able to find a job at all — recent figures for the District of Columbia show that three quarters of its Job Corps returnees were unemployed, even though some had spent a full two years in the program. Thus only a small, if meaningful, dent will be made in the block of 13 million sub-employed.

Undoubtedly, the future will bring even further expansion of manpower programs. This is not only desirable, it is indispensable to a lasting solution to poverty. Yet it is also time-consuming, as judged by present budget limitations, shortages of trained manpower personnel, and lack of placement success. And in the interim, the problem of the hard core unemployed is becoming more intractable. Indeed, sometimes it is not altogether clear but what the problem is outdistancing the solution.

The worsening plight of poor families is especially evident in the cities, where a grim selection process is systematically isolating them from the economic and social mainstream. From 1960 to 1965, while the national unemployment rate dropped two full percentage points, unemployment in major urban slums remained roughly constant. Economic growth brought diminishing returns to slum dwellers as other groups, notably white women and teenagers, entered the labor force to capture employment gains from Negro male adults. This result stemmed in part from the latter’s reliance on work as laborers and
operatives, occupations which were, and are, expanding much less rapidly than total employment. Furthermore, most of this modest expansion is taking place in the suburbs, usually a long, circuitous, and expensive trip from the central-city homes of the poor. And prices for public transportation continue to be one of the fastest rising items in the Consumer Price Index.

While job availability is dwindling, qualifications for employment are increasing. Many employers are beginning to require high school diplomas for even the lowest-skilled work. This is often an effective method of discriminating by race. Further discrimination takes place by police record, causing low income and crime to feed on each other in a vicious circle.

All in all, one of the few measures by which poverty is diminishing is the sheer numbers of people involved. From 1959 to 1964, the national poverty population fell by five million. Yet in this same time, the median income of all families rose by nearly $1,000, and the Consumer Price Index for city dwellers rose by over 8%. As a result, a number of families left in poverty in 1964 were worse off relative to other families than they had been in 1959, and worse off relative to their own purchasing power position in 1959. Furthermore, in those five years, the Negro teenage population in urban poverty areas increased by a phenomenal 50%, and the proportion of female household heads rose 10%, to one-third. Clearly, today's slums are becoming greater and greater concentrations of the least economically secure, for whom jobs are the least relevant. Finally, poverty remained a fact of life for millions of people for another five years.

Five years of new children entered upon the poverty cycle. Five years of frustrated aspirations alienated adults and youths, and hardened counterproductive attitudes and life-styles. Five years of unsuccessful job-seeking discouraged the dwindling percentage of adult male slum workers, undercutting job search and training activities, and producing labor force nonparticipation rates well over half again the national average.

It is in this context that recent calls for new Federal income maintenance programs should be placed. These proposals indicate a growing conviction that the U. S. must soon take steps to meet the immediate financial need of poor
families. As the elimination of poverty becomes more feasible, the failure to take such steps becomes less defensible. Furthermore, the returns coming in from six years of unprecedented growth and four years of accelerated opportunity and manpower programs, confirm that poverty will not yield readily to these strategies. In these circumstances, it makes little sense to condemn generations of Americans to uselessness and want, while policy makers devise ways of perfecting the labor market.

Income maintenance advocates, with few exceptions, are not offering money as a panacea, but as a contribution to complement and enhance the operation of other programs. Money can permit a long-term unemployed man to remain with his family, rather than leaving them so that they can draw welfare. Money can help support him while he trains for another job, and can help pay his transport costs to suburban employment. Money, given without residence requirements, can provide him with the security he needs to consider moving to areas of greater work opportunity, and it can encourage him to take other economic risks. It can allow his children to live in better housing, receive better health care, and stay in school longer, so that they will not suffer from the same educational and physical handicaps which presently make him unemployable. And it can accord him the right and the responsibility to choose among these competing uses in ways which are most satisfactory to him and his family.

Money given to individuals can greatly assist the community. It can revitalize the economy of urban slums, creating new employment opportunities in the process. Under a system of national standards, it can help cities pursue progressive welfare and manpower policies without being inundated by new arrivals. Properly disbursed, it can relieve state and local governments of their spiraling burden of welfare costs. And with any luck it can begin to undercut the crime, delinquency, and summer unrest which threaten almost every major U. S. city.

However, there remains a concern that any transfer program which paid out money regardless of employment status would conflict with other programs by drastically reducing the incentive to work. On this issue, there is a wealth
of opinion, but very little fact. A lot of the worry about disincentives stems from the suspicion that many current welfare recipients could get a job but prefer not to. Actually, the Federally-aided Public Assistance categories are set up precisely for those persons — the aged, blind, disabled, and female heads of families with small children — who are expected to be unable to hold a job, and recent statistics released by Presidential Assistant Joseph Califano show that only 50,000 persons, or fewer than one percent of the caseload, are capable of earning enough to get off welfare. Under the non-Federal General Assistance Program, recipients may be able-bodied men of working age, but the most thorough recent study of this program,3 by Professor Hirschel Kasper of Oberlin College, indicates that labor market conditions and not levels of assistance payments are the most important determinants of its caseload.

To be sure, welfare is characterized by some severe disincentive problems, but these stem mostly from the way the program is run, not from the availability of money without work, and operate primarily to reinforce the dependency of families already receiving assistance, not to attract new people to the rolls. These disincentives exist in the stiff means test, which taxes earnings at 100%4 and forces the liquidation of all assets which recipients have strained to accumulate, and upon which they might have built a future of self-support. They exist in the administrative vagaries and delays, especially lengthy waits for reinstatement, which make attempts at independence in an uncertain labor market highly risky. But these are exactly the procedural drawbacks which new income maintenance programs are being designed to avoid.

Consider first the negative income tax, a program to improve the symmetry of the personal income tax by extending its redistributive advantages to the

4 The 1967 Amendments to the Social Security Act were later to reduce the tax rate on earnings to 66 2/3% on earnings above $30/month.
one-fifth of all Americans who need them most. As commonly conceived, it would set up a schedule of allowances (refunds) based on family size, to be paid to families in the event that they had no other income. It would also specify a tax rate, at which these basic allowances would be reduced as income from other sources rose. Families would declare their eligibility by filing income affidavits periodically during the year. This extended tax treatment would be neither demeaning nor administratively cumbersome. It would reserve to all families the right to make their own spending decisions and their own work-leisure choices, while preserving the financial incentive to choose work. In short, it would provide money promptly and efficiently, with minimum inroads on private market operation and personal dignity and initiative.

A number of variants of this central theme have been suggested. Two of the better known are those of Professors Robert Lampman and James Tobin. Lampman's plan ⁵ (one of several he has discussed) would establish basic allowances equal to one half the sum of family exemptions and minimum deduction under the personal income tax ($3,000 for a family of four), and would reduce these allowances one dollar for every two dollars the family realized elsewhere. Tobin's ⁶ plan would establish basic allowances equal to $400 times the number of family members, and would tax other family income at 33-1/3%, until allowances were reduced to zero and positive tax liabilities reached the amount of tax due under the regular personal income tax. The authors have estimated total annual payments under these plans at $10 billion and $12 billion, respectively, fairly modest outlays when contrasted with the projected trillion-dollar GNP by 1975 and the vast potential returns from full-scale investment in human resources.

The Lampman and Tobin proposals are illustrative of a range of negative tax plans which would merely append themselves to the current tax system without

⁵ Christopher Green and Robert Lampman, "Schemes for Transferring Income to the Poor," Industrial Relations, February 1967, pp. 121-137.
making any significant changes in it. This add-on strategy raises certain problems of coordinating programs based on divergent tax and welfare concepts, but none that cannot be solved. For example, the definition of income usually suggested for negative income taxation includes a number of items — unemployment compensation, veterans' pensions, interest on municipal bonds, etc. — which are not now taxable. However, experience with the personal income tax shows all too well that we possess sufficient ingenuity in taxing different income sources differently to cope with this.

Yet, on grounds of equity and simplicity, there is a strong case for modifying the personal income tax to achieve long-needed reforms and to bind it with the negative tax into a single rational whole. Professor Earl Rolph has developed a credit income tax which serves these ends admirably. Its components are a flat per-capita credit for which all U. S. residents would eligible, and a proportional income tax levied on a comprehensive definition of income with no exemptions. Filing units whose tax was less than their credit would be refunded the unused portion of the credit. Removal of progressive rates would greatly reduce both the complexity of the total tax structure and the amount of valuable resources devoted each year to minimizing taxes under it. Yet progression would be systematically maintained by the existence of the basic credit. Professor Rolph has estimated that a 25% tax on comprehensive income could finance a $400 per-capita credit and still produce the tax yield of 10% of personal income which the federal income tax has been realizing in recent years. This low tax rate diminishes considerably the work disincentive problem which has troubled other transfer structures.

The drawback of this approach is that it would tie income maintenance directly to an even more controversial issue, tax reform. It would bring out the joint opposition of all the powerful interest groups which presently enjoy special treatment under the personal income tax. Adding these groups to the list of

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negative tax opponents would very likely defeat the program. The credit income tax sets an excellent standard for tax simplicity and equity, but it must probably await an evolutionary process before emerging in anything near its ideal form.

In addition to variations of the negative income tax, a wide range of other income maintenance plans has been suggested. One proposal is to increase and extend social insurance payments. Benefit levels under Social Security could be raised and all persons over 65 blanketed in. All workers could be made eligible for unemployment compensation with higher benefit payments and longer benefit periods. A generous program of this sort could eliminate poverty for up to seven million people, but estimates show that almost four times as much money would be paid out as would go to the poor. Many of the neediest poor would be excluded from even this expanded system, and any effort to incorporate them would endanger the popular insurance image of both Social Security and Unemployment Compensation.

Another approach would be to build upon the present welfare system. Uniform standards for eligibility, for size of payments, and for administrative techniques could be set. The means test could be relaxed to allow families to retain some part of their meager assets, and to realize some net gain from work. Families could be allowed to declare their own resources by affidavit, subject to audit as under the income tax, and social workers could turn from investigation to provision of services. In other words, public assistance could be modified along the lines of a negative income tax, as urged by the Advisory Council on Public Welfare. Indeed, if the Council's recommendation that need be established as the sole criterion for welfare eligibility were followed, public assistance would become virtually indistinguishable from a universal negative tax, except in terms of administering agency.

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8 Green and Lampman, op. cit., p. 135
However, many critics of welfare would prefer to introduce reforms without altering the present categorical approach. They cling to the belief that public policy can and should discriminate on the basis of employability. Many of them see no reason to give income assistance to poor people who are, by some rule of thumb, decreed able to work.

Others envisage a special new program of earned income supplements for the working poor. This program would presumably make low transfer payments and tax earnings at a fairly low rate, as opposed to welfare which would make higher payments and retain its high tax on earnings. But experience with welfare, where large numbers of AFDC mothers are working or have expressed a strong desire to work, has exposed the difficulty of making categorical judgments about individual employability. And even if public officials could, at any one point in time, determine who was able and who unable to work, it would be poor policy to declare the latter wards of the state and enroll them in programs which perpetuated that status. Rather than building a patchwork of programs which reinforce the differences on which they are predicated, public policy should aim at a transfer system which does not discriminate cruelly by source of poverty and does leave ample room for individual initiative and choice.

A third set of proposals which have been widely discussed are those designed to raise the incomes of families with children. The focus on children has a number of explanations — the large proportion of the poverty population (60% of all family members) under 18, the investment aspects of creating a better home life for children, the satisfactory operation of child-related transfer programs in most Western industrialized nations, the concomitance of poverty and large numbers of children in a labor market where wages are unrelated to family size, and the feeling that children are the most innocent victims of poverty. Alvin Schorr has estimated that a children's allowance of $50 per child per

10 Green and Lampman, op. cit.
month would bring three out of four poor children out of poverty. He emphasizes that the money would be paid to all families with children, thus avoiding the poor law image which he feels would afflict an income-related program. But he is also quick to note that spill-over of benefits to the non-poor could be largely avoided by careful coordination with the income tax. This is precisely the "income-relatedness" which characterizes the negative income tax. Under the negative tax, all families are eligible for some uniform structure of payments subject to some uniform schedule of tax rates. The fact that the rates succeed in taxing back amounts equal to or greater than the payments for most filing units does not make the negative income tax any less universal. Just as Schorr's children's allowance is the taxable equivalent of a categorical income grant paid to certain households as a matter of right, so the negative income tax is the taxable equivalent of the universal income grant paid to all households as a matter of right. The real issue is who should have what rights. In this regard, a strong case can be made that public policy ought to promote the right of all citizens to live decently, not the right of certain individuals to exercise special claim on the public purse by virtue of their age, health, residence, or family structure.

A final, challenging alternative for raising poverty incomes is a guaranteed job program, with the government, in keeping with its status as the nation's largest employer, spearheading a major expansion of work opportunities for the poor. Most advocates of this strategy envisage a program which would operate very much like unemployment compensation, where people who are judged able to work and available for work are required to take a suitable job or lose their income benefits. There are several difficulties with this approach, stemming primarily from the definition of suitable work. Under unemployment compensation, suitable work is any job entailing skills and wages commensurate with those involved in previous employment. This is a passable definition for a program whose participants have all been recently employed at at least the minimum wage. But for the 13 million sub-employed, it is a highly inappropriate standard.
A quick glance at the help-wanted ads will verify that there are consistently a number of very low-pay, low-status jobs for whom no takers can be found. By unemployment compensation standards, these are suitable jobs for a sizeable proportion of the poor. A guaranteed job program could deny income assistance to families with earners who refused to take such jobs. This is precisely the sort of compulsory work program which welfare recipients have successfully fought in several parts of the country. It places the federal government in the position of endorsing and enforcing low income status which has resulted in large part from government's own failure to provide adequate public services and to curb racial discrimination.

Yet the idea of a job-related income maintenance program is an attractive one. Its work orientation would enhance its public acceptability and reaffirm the government's long-standing obligation to maintain full-employment. Such a program might be developed along the following lines.

- (1) The government would announce its responsibility to find, create, or provide incentives for others to create, suitable jobs for all adults who declare themselves available for full-time work. A suitable job would be defined as one which paid at least $3,300 a year ($1.65/hour) did not involve extreme public opprobrium or physical danger, and offered some opportunity of advancement.

- (2) All households with an adult or adults who had declared themselves available for full-time work but for whom suitable jobs had not been found would be eligible for a transfer payment according to the following schedule.

<table>
<thead>
<tr>
<th>Household size</th>
<th>Transfer payment</th>
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<tr>
<td>1</td>
<td>$750</td>
</tr>
<tr>
<td>2</td>
<td>1,000</td>
</tr>
<tr>
<td>3</td>
<td>1,250</td>
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<tr>
<td>4 or more</td>
<td>1,500</td>
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</tbody>
</table>

- (3) Earners in households receiving transfers would be allowed to continue in their previous jobs or to take new jobs while awaiting training for, or placement in, a suitable job — in which case transfer payments would be reduced fifty
cents for each dollar earned. This would be the equivalent of a Lampman negative income tax filling half the poverty gap for all households of up to size four and at least a third of the gap for larger households. In all cases, transfers would be reduced to zero before other family income reached $3,000, so that no holder of a "guaranteed job" would receive these payments. Refusal to take a suitable job would result in the cutoff of all transfers under this program.

(4) Over time, the transfers payable to households with available earners for whom suitable jobs had not been found would be gradually increased. For example, the schedule of payments in Item 2 could be increased by ten percent each year for families of four or fewer, and by slightly more (up to at most 20%) for larger families. At the end of ten years, the combination of work and transfers under the guaranteed job program would have succeeded in closing the poverty gap for all participants. The proportion of participants employed would depend on the success of the government in opening up job opportunities, and in training people to take advantage of them.

A program of this sort has a number of attractive features. It balances the highly-touted obligation of the individual to contribute his labor services to society with the little-emphasized obligation of the society to maintain full employment at a decent standard of living. And it does this at a time when there is growing evidence that society is short-changing the individual, not vice versa. Furthermore, it makes the government directly responsible for the numbers of people receiving transfers, and raises the cost of each succeeding year's failure to secure them suitable jobs.

But there are also intricate problems involved. The pressure to get able-bodied workers off the transfer rolls and into some sort of employment would be great. The guaranteed job law would have to be carefully written and enforced to make sure that this was done only by the acceptable route of placing workers in a "suitable" job, with suitable strictly defined along the lines suggested above. This would require as much legal and administrative ingenuity in protecting individuals from exploitation as now goes into protecting the state from welfare fraud. A determination to set and maintain high standards might well cut into the political acceptability of the program.
Other problems would arise from the inevitable excess of demand over supply of suitable jobs. At the outset, some sort of rationing device would be required — perhaps a stipulation that primary earners be trained and placed first. This opens up the whole question of how best to coordinate jobs which are held by individual earners and transfer payments which are attached to entire family units. Families could maximize their income by splitting off their first guaranteed job holder, leaving other members eligible for transfer payments as long as some other adult in the family declared himself or herself available for full-time work. This new family head would be accorded priority in the job program as a primary earner. Against these financial incentives to family fragmentation, which can be minimized by thoughtful structuring of the program, must be offset the costs of setting up separate households and the non-pecuniary returns to family living. On the whole, there is no reason to expect that a guaranteed job program would increase the instability of low income families, and every reason to hope that it might decrease it.

Coordination with other programs would require special thought. One attractive possibility would be to limit the guaranteed job program to the non-aged, and blanket all individuals over 65 into Social Security. After a preliminary period of operation, the job program could itself extend to take on families headed by teenagers or having no member available for full-time work. The latter could be included by introducing an option allowing certain families to take part in the transfer end of the program without being bound by the employment requirement. Public policy has decided who these people are already — the blind, disabled, women with young children — so no new difficulties would be encountered in determining who could take advantage of the option. The important point is that it would be an option, and those eligible for it could elect instead to prepare for a suitable job. Finally, training programs would have to be coordinated with the transfer structure and tied very closely to the work opportunities being opened up by job search and creation.

Discussion of a guaranteed job program raises a number of questions to which there are presently few answers. How fast can useful jobs at adequate pay
be identified and structured in this economy? How fast can the sub-employed be trained to fill them? What are the costs of these job creation and training activities? To what extent must new jobs be concentrated in the public sector? How many new entrants will be attracted into the labor force by the job guarantee, and what difficulties will this entail for placing the disadvantaged?

Perhaps, the ten-year timetable mentioned in Item 4 is too optimistic. Perhaps we can go neither that fast nor that far. The whole incentive question of how transfers and taxes can be made before producing significant reductions in work effort is still an open one. The impact of income maintenance on individual market decisions, on family structure, and on local poverty pockets remains to be investigated. These are all difficult questions — but questions of fact which can be answered by careful research and experimentation. The time has come to stop speculating about possible outcomes of new policies and start conducting thoughtful, sustained research, including small-scale field tests of promising programs. Government Commissions and Task Forces are reporting regularly without benefit of the vital empirical evidence which such studies could yield. An excellent example is the National Commission on Income Guarantees, which President Johnson has announced will report in two years. If it were to round up every thread of presently available information, this Commission could still not begin to resolve the controversial issues underlying guaranteed jobs and income. Its failure to do so will severely damage the chances of these programs receiving the policy consideration they deserve. Yet special pilot projects in the field of income maintenance — projects which would produce the first hard fact on which to judge these programs — are entirely feasible. This is exactly the kind of research which the government must undertake if it is to make wise policy choices in the hard core area where poverty and unemployment meet.
Chapter II

PLANNING STAGE: SEPTEMBER 1967 - FEBRUARY 1968

The New Jersey negative income tax study began with a preliminary six-month period during which MATHEMATICA and the Institute for Research on Poverty developed detailed plans for the conduct of the experiment. Discussions at this time centered on six major issues:

1. Values for negative tax parameters.
2. Eligibility criteria for participation.
3. Definition of tax unit.
4. Definition of taxable income.
5. Coordination with other taxes.
6. Accounting and payment procedures.

At MATHEMATICA, memoranda on these topics were prepared and circulated among the project staff. This chapter records my memoranda of that time, updated with notes and comments on measures which were ultimately adopted for the experiment.

1. Values for Negative Tax Parameters

From the beginning, the negative tax project was conceived as an experimental rather than a demonstration program — that is, as an effort to study the effects of a general approach to income maintenance, rather than an attempt to prove the workability or desirability of a particular program. This was only reasonable given the lack of public consensus on the merits of the negative tax concept, and the lack of any kind of consensus on a single superior negative tax variant to test.

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Thus the basic design question was determination of the number, structure, and range of variation of negative tax plans to be incorporated in the experiment. This soon came to be known as the policy space question, where policy space was defined as the area of interesting combinations of income guarantees and negative tax rates \(^1\) about which the experiment wished to draw conclusions. This space could be pictured as a polygon of some shape, and the question of measuring work reaction to negative tax plans in the space could then be seen as a matter of determining the contours of a response surface over the space.

\(^1\) At a very early stage, nonlinear tax rates, specifically regressive schedules featuring two or more marginal rate brackets, were considered but ruled out as introducing too great a complexity into the work analysis.
TO: Members of Graduated Work Incentive Study
FROM: Heather Ross
SUBJECT: Policy Space
DATE: January 24, 1968

I. Description

Attached are:

(1) Chart A — a diagram of a proposed policy space shown for
    the standard example case of a family of size four.
    and

(2) Table A — a corresponding schedule of income guarantees by
    family size for various guarantee levels.

(1) The chart depicts the proposed policy space in the usual way, with
    levels of income guarantee (the amount of negative tax payment families are
    assured if they have no other income) measured along the y-axis and rates of
    negative tax (the rate at which negative tax payments are reduced as other
    family income rises) measured along the x-axis. Points on a ray from the
    origin represent combinations of guarantees and rates having the same break-
    even level (the level of other income at which negative tax payments are re-
    duced to zero).

    The hexagon delineated by the solid line in the chart seems to me to be
    the maximum symmetric space of interest to us. Its upper boundary closely
    approximates the current Social Security Administration non-farm poverty
    line. Although controversial in itself and related only to the barest measure
    of subsistence, this line (and others for other family sizes) is the most widely
    known and used standard of need, and the basis for nearly all of the published
    statistics on the poverty population, the poverty income gap, etc. Though it
    has probably achieved a credibility somewhat beyond its due, it appears to be
    the most accepted standard on which to base income guarantees.

    Yet as a standard, it need not be an upper limit — we could adopt plans
    with guarantees of 110%, 125%, 150% of the poverty line. However, OEO has
made very clear its reluctance to spend substantial sums of money on testing plans with above-poverty level guarantees. Anything but a token effort to incorporate these plans would be quite expensive given the relatively high cost of each observation. Thus it appears that in the absence of a particularly strong scientific case for working with high-guarantee plans, OEO's practical concerns must remain binding, at least for the moment. Early field results will allow us to weigh the wisdom of our present choices in time to make necessary adjustments for the body of the program.

The lower boundary of the proposed policy space is 40% of the Social Security Administration poverty line. This guarantee level is discussed by Christopher Green in his book *Negative Taxes and the Poverty Problem*. It is the lowest such level I have seen seriously considered anywhere.

The right and left hand boundaries — the maximum and minimum negative tax rates — are set at 70% and 30%, respectively. The latter is a low compromise between small guarantees and high break-even levels. It is slightly below the tax rate of 1/3 featured in a well known negative tax plan proposed by Professor James Tobin. The latter is a device to combine relatively high guarantees with relatively low break-even levels — an effort to achieve a large degree of redistribution for any amount of transfer payments. The 70% rate is presently the highest marginal tax rate on earned income in the personal income tax and in many state Aid to Families with Dependent Children programs.

As far as alleviating poverty, the two rates are intended to bracket a realistic range of tradeoffs between incentive and efficiency. The proposed range is quite substantial, as variations in economic phenomena go, and should be wide enough to elicit differences in work behavior if any of relevance exist.

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The hexagonal shape is achieved by lopping off the north-west and south-east corners of what would otherwise be a rectangle. This is done to eliminate from consideration guarantee-rate combinations which produce what appear to be unrealistic break-even levels. Plans with break-even levels in excess of two and one half times the poverty level have been effectively ruled out by OEO as being too ambitious. Plans with break-even levels below two-thirds of the poverty level are too meagre to warrant the costs of setting up a sophisticated apparatus to test them.

(2) As mentioned in (1), the upper boundary of the policy space is approximately the Social Security Administration poverty line. As can be seen in Table A, the approximation consists in rounding off the line to the nearest hundred dollars, except for two household sizes. Single individuals receive considerably less than their poverty line and families of three receive somewhat more.

The lower amount for single individuals allows for the concern expressed by people on the project staff and elsewhere that large differentials between payments attributable to first and second family members might induce household heads to split off from the rest of their families. The amount proposed here is a compromise between a purely economies-of-scale calculation of need ($1700 for the first member, $400 for the second according to the Social Security Administration) and an even splitting of payments for a family of size two between the first two family members ($1050 for each person). It is closer to the latter than the former — a reflection of society's apparently grave concern with public policy that might conceivably foster family instability.

The payment for a family of size three is raised slightly above the SSA poverty line to avoid an anomalous situation wherein the fourth family member is assumed to entail greater marginal expense for the family than the third. This reverse economy-of-scale would be the outcome if the poverty line for a family of size three were adopted without adjustment.

Guarantees by family size for all other tax plans within the policy space are calculated as percentages of these modified SSA poverty line standards.
Two particular percentage levels are proposed for inclusion and are shown in Table A.

II. Reservations

A principal feature of the proposed policy space is its near-rectangularity — that is, the low correlation between guarantees and rates for admissible tax plans. This is a valuable property when it comes to estimating the independent effects of income guarantees and negative tax rates on work behavior. Orthogonality or near-orthogonality among key economic variables is a relationship rarely found by economists in nature, and I believe that the opportunity to arrange such a relationship in an experimental setting should not be dismissed lightly. This is especially true in the present case where distinguishing the effects of guarantees and rates — that is, the income and substitution effects of negative taxation — is a matter of major interest, and one on which current theory offers little practical guidance.

However, there is considerable doubt in my mind that the appropriate policy space includes some of the extreme values on the hexagon, in particular the points constituting the lower right hand corner. I would prefer that the policy space lie everywhere above the $3300 break-even ray — that is, that all poor families be eligible for some payment, however small. In the first place, I think this is a particularly defensible lower cut off. In the second place, I believe it may greatly contribute to success in our research objectives.

I am uneasy about attempting to enroll families in programs which, at their initial income levels, yield them very little or nothing, especially since we are counting on the financial attraction of negative tax payments to keep a hold on our experimental group over three years. Slightly over half of all poor families with male head and two children in the U. S. had an annual income above $2200 in 1966. 3 Confining the calculation to such families in

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northern urban areas would raise the median income well above this level. If 70% of families in our sample are poor and if this stratification is maintained in all treatment groups, fewer than 1/3 of the families assigned to the $2200 break-even treatment will be eligible for any payments at their initial level of income. This may lead to very high rejection rates in this category.

These high rejection rates may be particularly damaging, since we are interested in both the height and the contours of the response surface

\[ Z = f(X, Y) \]

over the XY policy space, where \( Z = \) the level or change in earnings and/or hours worked, \( X = \) the negative tax rate, and \( Y = \) the basic family negative tax payment. Being interested in the height, we will want to anchor the surface firmly at at least one point. A logical point to do this is at the least generous treatment, since it will be the least expensive to group families here and it will require fewer families here to get a good fix on the height, the variance of the response likely being relatively small at this point.

However, high rejection rates plus high attrition rates could make observations here difficult, and their generality suspect. This plus the fact that high negative tax rates could elicit such marked work disincentive

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4 This assumes a limited number of standard tax treatments, not a multitude of treatments constructed at random from all admissible rate-level combinations, a point which has yet to be decided but seems likely to be resolved in the former direction.

5 We plan to adopt an incentive payment scheme for insuring interview response, but are not confident of our ability to retain families receiving only these fees. Raising their remuneration will cut our losses, but will shortly begin to contaminate our control. Also, significant numbers of families ineligible for negative taxes because their incomes are too high (and do not drop) seems an inefficient use of our experimental group.
responses that the least-cost point\textsuperscript{6} would move toward the left (a direction in which the least variance point is likely to be found anyway) suggests to me that we don't wish to focus on the lower-right hand corner of the hexagon.

It would be easy to establish some compromise solution between the hexagon and the $3300 break-even level cutoff. However, I believe that complete acceptance of the cutoff is the best solution, even though it entails considerable correlation between rates and levels of tax. This is a realistic outcome, reflecting the practical tradeoffs between negative tax parameters — i.e., reflecting the asymmetry of the true policy space.

All these considerations lead me to propose seven tax plans for inclusion in the experiment. These seven plans are at the edges and in the middle of the suggested policy space and are marked by asterisks on Chart A. They bound the space and allow for observation of any unusual curvature of response in the middle.

NOTES:

The final policy space featured tax plans with guarantee levels equal to 50\%, 75\%, and 100\% of a further modified poverty standard.

The further modification in the standard resulted from:

(1) a proposal by Professor Albert Rees to adjust the SSA poverty lines, which are based solely on a nutritional standard, to allow for more marked economies-of-scale in urban living, chiefly in housing expenditures. This proposal adopted the SSA standard for a family of size four ($3300), but reduced the marginal payments for additional family members, producing progressively smaller guarantees than before for larger family sizes.

\textsuperscript{6} For any given guarantee level, payments to eligible families with a particular initial income will be higher the lower is the negative tax rate, provided that families continue to earn their initial income. However, families subject to high tax rates could reduce their earnings so much relative to earnings of families with lower tax rates that the high tax rate treatment would actually cost more after work adjustment than treatments with lower rates.
Table A

Schedules of Income Guarantees by Family Size

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Tax Treatment</th>
<th>Schedule E (40%)</th>
<th>Schedule B (70%)</th>
<th>Schedule A (100%)</th>
<th>SSA Poverty Line March 1967 (male head)</th>
<th>SSA Poverty Line March 1967 (non-farm)</th>
<th>Schedule A Discrepancy from Poverty Line</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>max. per person</td>
<td>max. total</td>
<td>max. per person</td>
<td>max. total</td>
<td>max. per person</td>
<td>max. total</td>
<td>max. per person</td>
</tr>
<tr>
<td>1st Member</td>
<td>480</td>
<td>480</td>
<td>840</td>
<td>840</td>
<td>1200</td>
<td>1200</td>
<td></td>
</tr>
<tr>
<td>2nd Member</td>
<td>360</td>
<td>840</td>
<td>630</td>
<td>1470</td>
<td>900</td>
<td>2100</td>
<td></td>
</tr>
<tr>
<td>3rd Member</td>
<td>240</td>
<td>1080</td>
<td>420</td>
<td>1890</td>
<td>600</td>
<td>2700</td>
<td></td>
</tr>
<tr>
<td>4th Member</td>
<td>240</td>
<td>1320</td>
<td>420</td>
<td>2310</td>
<td>600</td>
<td>3300</td>
<td></td>
</tr>
<tr>
<td>5th Member</td>
<td>240</td>
<td>1560</td>
<td>420</td>
<td>2730</td>
<td>600</td>
<td>3900</td>
<td></td>
</tr>
<tr>
<td>6th Member</td>
<td>200</td>
<td>1760</td>
<td>350</td>
<td>3080</td>
<td>500</td>
<td>4400</td>
<td></td>
</tr>
<tr>
<td>7th Member</td>
<td>200</td>
<td>1960</td>
<td>350</td>
<td>3430</td>
<td>500</td>
<td>4900</td>
<td></td>
</tr>
<tr>
<td>8th Member</td>
<td>200</td>
<td>2160</td>
<td>350</td>
<td>3780</td>
<td>500</td>
<td>5400</td>
<td></td>
</tr>
<tr>
<td>9th Member</td>
<td>0</td>
<td>2160</td>
<td>0</td>
<td>3780</td>
<td>0</td>
<td>5400</td>
<td></td>
</tr>
</tbody>
</table>

1 Social Security Administration, Research and Statistics, Note 23-1967.
(2) a desire by the project staff, particularly those at the Institute to avoid payment structures which might contribute to family instability. Guarantees for two member families were reduced and allocated equally between the two members. Thus, household heads would be eligible for exactly the same guarantee whether they stayed with their families or not. Concerning the second modification, I am not convinced that guaranteeing single individuals half the amount for a family of size two is the best procedure. A proper schedule of guarantees is designed to give equal treatment (support) to families of different sizes — that is, to play a neutral role in families' decisions on additions or separations. By definition, an accurate accounting of economies-of-scale yields this proper schedule. To overturn such an accounting, one must either not accept his own economies-of-scale (living costs for one person are certainly more than half as much as living costs for two people) or must anticipate widespread ignorance of true living costs or widespread fraud through the reporting of fictitious family splits. My proposal made substantial allowance for such contingencies, in large part because of the evident concern here on the part of other staff members. But the final schedule adopted penalizes single individuals even further in an effort to promote family stability. (Note that the only way a single individual can enter the program is by splitting off from a larger unit, since the smallest household we will accept initially is of size two.) This non-neutrality is enhanced by other decisions on the treatment of fragmented families which will be discussed later under definition of family unit.

The decision to set the minimum guarantee at 50% of the poverty standard reflected the belief that this was the lowest plan of policy interest. It is approximately the guarantee level proposed by Professors Friedman and Lampman in separate discussions of the negative income tax. This was a particularly wise decision in view of the higher than expected incomes which we later found in our city sites.

The policy space was covered by seven tax plans located around the edges of the space and in the middle as previously proposed. These seven
plans are shown in the chart on the next page and their respective guarantee levels by family size are shown in the table below.

Table B
Income Guarantees by Family Size

<table>
<thead>
<tr>
<th>Family Size</th>
<th>% of Modified Poverty Standard</th>
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<tbody>
<tr>
<td></td>
<td>50%</td>
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<tr>
<td>1</td>
<td>500</td>
</tr>
<tr>
<td>2</td>
<td>1000</td>
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<td>3</td>
<td>1375</td>
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<tr>
<td>4</td>
<td>1650</td>
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<td>1850</td>
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<td>6</td>
<td>2025</td>
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<td>7</td>
<td>2175</td>
</tr>
<tr>
<td>8</td>
<td>2300</td>
</tr>
<tr>
<td>9</td>
<td>2300</td>
</tr>
</tbody>
</table>
Chart B: Negative Tax Policy Space
(Family Size Four)

Rays denote breakeven levels

Negative tax rate
2. Eligibility Criteria

A second major decision concerned the segment of the population on which the negative tax was to be tested. The original conception of the experiment determined this in several important ways. The work incentive focus of the research dictated that households of interest contain at least one member who was presumed able to make the work-leisure choice at issue. An urban setting had long since been agreed upon by all parties, reflecting concern for the mounting problems of the cities and the belief that it would be simpler to conduct this first test with a compact group of predominantly wage-earning families with no substantial assets. Plans were discussed for a companion rural study to be conducted in one or more counties in the south and midwest. The Institute has now received preliminary funds from the Ford Foundation for such a study, which will be getting underway this June in Iowa and North Carolina.

Finally an understanding had been reached, and budgeted, that the total sample would be comprised of approximately 1000 families. This was the sample size in my original draft proposal, and one which Glen Cain had found to be adequate in an early analysis. Given this limited sample size, it was imperative that experimentation be confined to a fairly homogeneous group of people so that underlying variations in income patterns over time did not swamp differentials in response to the seven different tax plans.

Thus the task of the project staff was to decide upon the inclusion of a particular homogeneous group of urban households with employment capability, and to establish a specific definition of eligibility upon which survey instruments could be developed to identify these households in the larger population.

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TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Population to be Sampled in Graduated Work Incentive Experiment
DATE: November 27, 1967

We propose to select participants for the experiment at random from the following population:

All families

— with a male head between the ages of 18 and 60 who has worked at least 20 days in the past year, and is not currently disabled.
— with a woman living in the home as the head's wife, whether they are legally married or not.
— with total family income for 1967 as defined for experimental negative tax purposes, at or below 125% of the Social Security Administration's most recent poverty level for a family of its size (presently $3,335 for a family of four), and net wealth under $25,000.
— living in quarters, in poverty tracts, as defined by the U. S. Census Bureau, in the following SMSA's:
    Trenton, New Jersey
    Jersey City, New Jersey
    Paterson, Clifton, Passaic, New Jersey.

These criteria are chosen in an effort to focus the experiment on a particularly interesting segment of low income households in an easily accessible, non-atypical location. The need for a narrower focus than the entire low-income population, given the limited sample allowed by available funds, is made clear in papers by Guy Orcutt\(^1\) and Glen Cain.\(^2\) The nature of the focus is dictated by the prime research concern with the work behavior of negative tax recipients.

2 Cain, Glen, *op. cit.*
The households described above exhibit a considerable degree of homogeneity as to a number of potentially important determinants of income, and therefore, presumably, as to patterns of income change, without being so specialized as to lose much of their experimental interest. Of 11.89 million poor households (families and unrelated individuals) in 1964, 6.15 were headed by a person with some work experience in that year. Of these latter households with a head who exhibited work capability, 3.54 million or over 57% were families headed by a male. Approximately 89% of these male-headed families had a head under 60, and approximately 95% of them had the head's wife living in the home. Over 80% of them had a non-farm residence. Thus, about 40% of poor households with obvious work potential are represented by the proposed sample. Given an average family size of 4.4 persons for poor, male-headed families in 1964, these eligible households contained approximately 46% of the entire 1964 poverty population of 34 million people.

These estimates are surely off the mark for a number of reasons. The data are themselves estimates based on a sample survey of households in 1964. The limitation of sampling to urban poverty-tracts — i.e., tracts in SMSA's of 250,000 or more which rate in the lowest quartile of the nation's census tracts on 5 poverty criteria — is certainly restricting. The inclusion of some near poor families will make the experimental population more representative of the total population in the relevant income range, since intact, non-aged families with some labor force attachment form a larger proportion of near-poor families than of poor families. All in all, the population defined above does seem representative enough of low income households with work potential to be of real policy interest.

Unfortunately, there do not seem to be data on the homogeneity gains from thus restricting the population. Without such data, the tradeoff between

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statistical significance of results and representativeness of sample, given sample size, is obscure, making determination of the optimal experimental population difficult.

But there are other considerations which recommend the population specified above. One of these is the difficulty of applying negative tax allowances to households which enlarge during the course of the experiment. It is certainly to our interest to have payments by family size uniform except for controlled variations introduced by us. This could be achieved by raising basic negative tax allowances appropriately for each person added to the family as initially enrolled. But, such a financial incentive to enlarge participating families would be unrealistic in the context of a national program and probably very costly to the experiment. Failing to raise basic allowances as new family members appear permits differentials in economic standing to arise between families in the same before-tax situation and subject ostensibly to the same tax treatment. Whether the experimental design calls for a few discrete values for tax parameters, or continuous variation of tax parameters, a systematic discrepancy will occur in that families with particular preferences will, in exercising those preferences, effectively modify the tax treatment to which they have been randomly assigned. The problems presented by this prospect do not seem critical, but they can be minimized by enrolling only those families which are, in some sense, complete at the beginning of the experiment. A look at statistics for the U. S. shows intact families to be the predominate household form even at low income levels.

Single individuals over 18 and families with female heads are likely to become part of an intact family at some time during the experiment causing comparability problems, and perhaps leaving few individuals or female-headed families left to study. Furthermore, such transformations are more likely to alter the economic role of the original sample members, than are additions of new dependents to intact families. This can be accommodated by designating the new breadwinner, if any, as the household head, but such uncontrolled entry into a carefully selected sample may be undesirable.
Finally, there is the problem of drawing a representative sample of the population of interest. U. S. Census experience suggests that it may be difficult to come up with a truly random sample of unrelated individuals in poverty areas, especially Negro males. Also, the relatively high levels of assistance in New Jersey under AFDC (average payment = $2,700 per year in July 1967 for a family of size four) indicate that a substantial proportion of female-headed families meeting the income requirements of our program will be eligible to receive public assistance. If they are actually receiving assistance, they are unlikely to agree to participate in certain low-benefit variants of the negative tax, even though they would probably be made no worse off financially thereby. If they do agree to participate in those variants, they will be subject to welfare's 100% tax regardless of how we choose to treat welfare payments as income. If they are eligible on an income basis to receive welfare, but are not — (New York City estimates that it has as many people in this category as are already on the welfare rolls) — a selection process has already taken place, differentiating them by such things as length of residence and attitude toward transfer benefits. It may be true that female-headed families with income somewhat above cut-off levels for assistance are also making a conscious choice in favor of work over welfare "leisure" at slightly lower income. Thus, over broad range of low income, female-headed families not on welfare may have already expressed a work-leisure choice of the sort we plan to present to families in the experiment. Given the different nature of public assistance and negative income taxation, it is probably not legitimate to say they have already previewed their negative tax behavior. But the basic similarities between the two programs suggest that the least expensive way to experiment with new transfer structures for female-headed families would be to alter the present welfare structure in controlled ways. Amendments to the Social Security Act presently before Congress, and statements by the Welfare Commissioner in New York City indicate that such experimental changes in welfare may soon be underway, yielding data to complement the findings of our program.
The requirement for labor force attachment is intended to be fairly loose in order not to exclude households with a head who is able to work but, for any of a number of reasons, has not had extensive recent employment experience. It is designed to rule out only the disabled and those very marginal workers who can in no sense be said to rely on earnings as a primary source of income.

The income ceiling is an arbitrary one, designed to focus the experiment on the lower tail of the income distribution. It is at these levels that the most pronounced response to negative income taxation may be expected. There is, of course, great interest in the negative tax behavior of households with income considerably above this limit, primarily because the number of such households is so large that even a fairly minor income adjustment by each of them would be highly significant in the aggregate for national output and the cost of the negative tax program. It is hoped that the proposed limit will allow enough income variation to observe differential response at different income levels and permit some extrapolation of these results to higher income levels. Including these higher income households at the outset would load the sample with potentially low response units and greatly raise sampling and interview costs by making concentration on poverty tracts untenable.

The wealth ceiling is incorporated to exclude the rare case of poverty area families with low current income but high assets, but is not expected to be binding.

It seems reasonable, on grounds of administrative economy and the homogeneity requirements of the sample, to concentrate this first, small effort in a fairly restricted geographic area. The State of New Jersey has been advanced as a desirable setting because, while it is not atypical of the rest of the nation in its socio-economic makeup, it does have a number of features which may be conducive to a successful experiment. First of all, it is one of few heavily populated, industrial states which do not conduct an Unemployed Parent Program under Aid to Families with Dependent Children. Thus the negative tax program, in confining itself to male-headed families, will not be
indirect competition with another federally-financed, need-based transfer program. Secondly, New Jersey has a unique set of state agencies which are eager to assist the negative tax program in anyway possible, including acting as fiscal agent for negative tax payments.

Within the State, sampling will be confined to urban areas in order to avoid problems of severe seasonality of income and non-comparability of farm and non-farm receipts. We will almost certainly want to conduct the main body of the experiment in what may be called the Northern New Jersey industrial complex. This area contains the second and third largest cities in the State (Jersey City and Paterson) with the second and third largest concentrations of poor families. By far the largest city with the largest poor population, Newark, has been passed over because of its special social and political climate at the request of OEO. Newark, and especially its ghetto areas, has been saturated with surveys recently and is not receptive to further study. The neighborhoods are unusually well organized making group response the most likely reaction to our program, rather than the individual work-leisure choice we seek. Trenton, New Jersey is proposed as the site of an early test of the experimental apparatus. It has been selected because of its modest size, its relative community stability, and its nearness to research and administrative operations.

The limitation to poverty tracts in SMSA's containing these three cities is proposed purely on grounds of efficiency. Estimates from 1960 Census data suggest that these compact areas will yield ample numbers of eligible families. There are 35 contiguous poverty tracts in the Jersey City SMSA — 25 in Jersey City itself and 10 in Hoboken. In the Paterson, Clifton, Passaic SMSA are 21 such tracts — 16 in Paterson and 5 in Passaic. (The Paterson and Passaic tracts are separated by four non-poverty tracts, mostly in Clifton, which themselves have relatively low median incomes.) Trenton has 10 poverty tracts in the center of the city, which held a total of 13,507 households in 1964. Of these, approximately 2,200 or 1 in 6 appeared to meet our eligibility requirements. Given the Census Bureau's estimate of roughly
eight dollars per screening interview, it would have cost $48,000 to obtain 1,000 eligible families from these tracts. Moving beyond poverty tracts will greatly increase the costs of identifying any given number of such families. The focus on poor neighborhoods in central cities does not seem unacceptably narrow.

NOTES:

All four of these proposed criteria were changed in some respect. The upper limit on the age of the head was lowered to 58 to avoid projected overlap with early retirement under Social Security. To extend the sample's representativeness, the wife-present requirement was dropped entirely and replaced by the broader stipulation that eligible households consist of at least two related persons of different generations or of two persons living together as man and wife. The income ceiling was raised to 150% of the Social Security poverty line to include representatives of a more heavily populated income stratum and to explore directly the work behavior of families at or slightly above their breakeven lines. The focus on official poverty tracts was rejected by OEO as biasing the sample toward ghetto-type surroundings and minority group participants. Their statistics showed this bias to be very great indeed, and the physical scope of the study was quickly broadened to include non-poverty tracts as well.\(^4\)

Debate on these criteria was limited, and was directed almost entirely to the question of how best to implement a conception of the "right" participants which almost everyone shared. This consensus is ironic in the face of later public doubts about our eligibility criteria. Certainly the single most frequently heard criticism of the study to date has been directed at its focus on male-headed families. This criticism comes in part from people who feel

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\(^4\) The scope of the study was also eventually broadened to include an additional city, Scranton Pennsylvania, about which more will be said later.
the limitations are merely discriminatory, but also from people who feel that the major poverty and income maintenance problems, even in their work incentive aspects, reside with population groups, notably female-headed families, who are not included in the program.

At the outset, I did not have major qualms about the exclusion of female-headed families, although my opinion was to change over time. My chief reservation concerned the income distribution of sample families, as indicated in the following memorandum.
TO: Members of the Negative Tax Study
FROM: Heather Ross
SUBJECT: Income Stratification and Sampling in Trenton
DATE: April 19, 1968

I. Some time ago, the principal objective of the Wisconsin-MATHEMATICA project was stated as the estimation of costs of a national negative income tax program. Subsequent design work has made it clear that the project must focus on particular components of those costs, and to that end, a fairly narrow definition of family eligibility has been established. Thus, although the negative tax is heralded as a universal program serving all those in need, we have, of necessity, confined our sample to families representative of only about 15% of poor households nationally. The chief reason for excluding the others was the diversity of their labor force opportunities and behavior.

While limiting the participation of poor persons on labor force grounds, we have extended eligibility to persons substantially beyond official poverty levels. Yet, there is ample evidence that the labor market in which these latter people operate is significantly different from that facing the poor.
Sixty-two percent of poor male family heads under 55 who worked during 1963 worked the full year, whereas the corresponding figure for all male family heads under 55 was 85%. Also, 7% of all job holders in urban slums in November 1966 were working part-time although they wanted full-time work, as opposed to 2.3% nationally.

Several reasons have been advanced for including these two significantly different sets of workers in one constrained experiment.

(1) Lower middle class workers are an important segment of potential negative tax recipients whose participation in the experiment is essential if any firm policy conclusions are to be drawn. These people exist in large numbers and make a large aggregate contribution to national income. Virtually nothing is known about their likely response to income maintenance – even the rudimentary insights obtainable from public assistance are lacking.
(2) A low ceiling on eligible income is likely to bias the sample towards families with earnings which are temporarily low, making correct interpretation of any earned income rise in the presence of negative taxes difficult.

(3) The public image of the program may suffer needlessly if eligibility is limited to very low income levels. Wider participation may permit any disincentive outcomes to be seen as rational economic decision making, not merely laziness among the poor.

II. On these bases, a decision was reached to extend eligibility to families with annual income up to and including 150% of the modified poverty standard adopted for the experiment. The following two tables show the resulting ceilings on eligible income by family size, and the estimated percentage distribution of the sample by income and family size using these cutoffs. The second table is an adjustment of Rees' table in his memorandum of March 8, reflecting the new, lower poverty standards adopted for households of size one, two, and three at the suggestion of Wisconsin.

Table 1

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Poverty Standard</th>
<th>Maximum Initial Income for Eligibility</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1000</td>
<td>-</td>
</tr>
<tr>
<td>2</td>
<td>2000</td>
<td>3000</td>
</tr>
<tr>
<td>3</td>
<td>2750</td>
<td>4125</td>
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<tr>
<td>4</td>
<td>3300</td>
<td>4950</td>
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<td>5</td>
<td>3700</td>
<td>5550</td>
</tr>
<tr>
<td>6</td>
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<td>6075</td>
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<tr>
<td>7</td>
<td>4350</td>
<td>6525</td>
</tr>
<tr>
<td>8 or more</td>
<td>4600</td>
<td>6900</td>
</tr>
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Table 2
Estimated Percentage Distribution of Families by Income and Family Size Using Proposed Income Cutoffs

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Poverty Status</th>
<th>Income (thousands of dollars)</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Under 1</td>
<td>1-2</td>
<td>2-3</td>
<td>3-4</td>
<td>4-5</td>
<td>5-6</td>
<td>6-7</td>
</tr>
<tr>
<td>2</td>
<td>Poor</td>
<td>5.1</td>
<td>9.9</td>
<td>10.0</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>Poor</td>
<td>2.8</td>
<td>3.6</td>
<td>3.4</td>
<td>1.8</td>
<td>6.7</td>
<td>0.9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>Poor</td>
<td>1.6</td>
<td>2.0</td>
<td>3.2</td>
<td>1.3</td>
<td>3.8</td>
<td>7.6</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>5</td>
<td>Poor</td>
<td>1.0</td>
<td>1.3</td>
<td>1.9</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6</td>
<td>Poor</td>
<td>0.5</td>
<td>0.7</td>
<td>1.2</td>
<td>1.6</td>
<td>2.3</td>
<td>3.2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>Poor</td>
<td>0.6</td>
<td>1.0</td>
<td>1.5</td>
<td>1.8</td>
<td>1.1</td>
<td>2.5</td>
<td>2.1</td>
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<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>Poor</td>
<td>11.6</td>
<td>18.5</td>
<td>11.2</td>
<td>6.3</td>
<td>1.1</td>
<td>2.1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nonpoor</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
</tbody>
</table>

Table 2, which Rees developed from Census data on the 1959 incomes of all urban families, suggests that over half of a sample drawn subject to only these income limits would be above our poverty standard. This is an underestimate for several reasons.

(1) Family incomes have risen considerably since 1959. Wisconsin estimates that the number of persons found below 125% of the poverty level in 1960 would today be found below 150% of the current (higher) poverty level.

(2) The proportion of poor families who are aged and thus excluded from the sample has risen from 1 in 6 in 1959 to 1 in 5 in 1966 — or approximately 17%.

(3) Incomes of families with male heads are systematically higher than incomes of families with female heads. In 1966, this disparity caused the median income of male-headed families to exceed the median income of all families by 5%.

On the other hand, Trenton is below the national urban average in income, in part because its population is more heavily Negro than the average. Nonetheless, it is apparent that a random sample drawn widely throughout the city along the above lines would yield considerably more non-poor than poor eligible families. This seems undesirable for a number of reasons. It may present public relations problems for OEO. It may load the sample with potentially low-response families whose incomes are too high for their assigned tax variant to make any difference to them. These families may decline to participate or may drop out at some point if the small dollar returns from the program are not worth the effort and perhaps anxiety of filing monthly income reports and submitting to quarterly interviews. Among poor families, where incomes are most unstable to begin with and variations in work response likely to be high, there may be too few observations to get a good fix on experimental effects.

III. Several steps might be taken to weight the sample more heavily with poor families. Target areas for sampling could be limited to certain very
low income census tracts. This has a number of unacceptable biases and would probably not achieve the desired end anyway. Alternatively, the upper limits on income eligibility could be reduced to, say, 125% of the experimental poverty standard, and families could be drawn at random from this smaller population. This seems to me to be an arbitrary but reasonable way of proceeding. It grants the need for non-poor in the sample, but fewer of them and at lower income levels.

The question of which, if any, modest negative tax plan the nation will first enact depends primarily on the behavior of the poor. We would do well to get a firm idea of that behavior, no mean task given the large and erratic income fluctuations which already exist in these income strata. The question of how the non-poor respond seems logically second to me. If the poor show marked work disincentives, the policy will likely be ruled out for the time being, regardless of what the non-poor might do. If the poor do not show such disincentives, then the non-poor become a matter of real interest, although their response will certainly have been previewed to some extent by the poor. This is not to suggest that poverty results can be simply extrapolated upwards, but rather to point out that seldom are people observed to willingly exchange places on the relative income scale. Of course, this still leaves room for relatively small marginal adjustments in work effort among the non-poor-adjustments which could, in the aggregate, be very expensive in terms of direct costs and losses in national income. What this says is that the question of non-poor response will probably require further study — one of many areas for additional work which the experiment has already pinpointed in its early stages.

IV. Another way of achieving approximately the same poor/non-poor balance as would arise from setting a ceiling on initial income at 125% would be to retain the proposed 150% ceiling and require that 70% of the sample fall in the 0 - 100% range, and 30% in the 100 - 150% range. This would produce a sample in Trenton of the following makeup:
<table>
<thead>
<tr>
<th></th>
<th>Experimental Group</th>
<th>Control Group</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Poor</td>
<td>112</td>
<td>56</td>
<td>168</td>
</tr>
<tr>
<td>Non-poor</td>
<td>48</td>
<td>24</td>
<td>72</td>
</tr>
<tr>
<td>Total</td>
<td>160</td>
<td>80</td>
<td>240</td>
</tr>
</tbody>
</table>

Assuming a total non-participation rate of one-third among the poor — i.e., that as many as one-third of eligible poor families identified during the screening will either be lost sometime between screening and enrollment, prove ineligible at the time of enrollment or enrollment interview, or decline to participate in the enrollment interview, enrollment, or negative tax program — the Trenton sample should be drawn in such a way as to yield at least 252 eligible families with incomes below our full support standard. Given the income distribution of families in Trenton, as elsewhere in the U. S., it is inevitable that the process of identifying 252 poor families will yield many more than the required number of non-poor families — i.e., more than the 144 non-poor families required assuming a total non-participation rate among these people of 1/2.

In effect, the screening operation will be used to sample at random, but at different frequencies, two mutually exclusive populations. This in no way damages the statistical properties of the final sample, but does add a new complexity to the already intricate job of interpreting the results.

V. Census of data for Trenton yield the following proportions:

Total City — 23 tracts (excluding 2 tracts occupied solely by institutions)

\[
\frac{\text{Eligible Poor Households}}{\text{Total Households}} = \frac{2,904}{31,964} = .091
\]

15 Census tracts ranked highest by proportion of families with income under $3,000 in 1959.
\[
\frac{\text{Eligible Poor Households}}{\text{Total Households}} = \frac{2,216}{22,554} = .098
\]

Census-designated poverty area — 10 tracts.

\[
\frac{\text{Eligible Poor Households}}{\text{Total Households}} = \frac{1,402}{13,507} = .104
\]

These are very rough estimates — poor means income below $3,000/yr., etc. — but they suggest that the proportion of potentially eligible families does not vary drastically across the city. In part, this stems from the fact that in tracts where the number of poor families tended to be high, the proportion of households headed by a married couple tended to be low, and vice versa. It is useful to note that the 10 poverty tracts are all contained in the 15 tract area, but are not the ten highest ranking tracts in that larger area. This reflects the more comprehensive definition of poverty used by Census in defining official poverty tracts (including percent of children not living with both parents, etc.) and also their efforts to update poverty tract designations by taking account of large population shifts, notably those caused by urban renewal, which have taken place since 1960.

I suggest that we adopt the 15 tract area as our experimental site in Trenton. This area includes all of the official poverty tracts plus almost half of the other residential tracts in the city — tracts which are contiguous and relatively compact. It is impossible to judge their racial makeup from 1960 Census data which show only three predominately non-white tracts in the whole city. This has definitely changed. The proposed tracts are listed at the end of this memo.

I also suggest that we accept our field organization's proposed sampling method of interviewing every \( \frac{1}{n} \)th household in the experimental area, with an intensive screening operation in at least one segment of the area to check on biases in the broad screen. The field organization has indicated that the cost of doing this does not differ greatly from the cost of thoroughly listing and
screening certain blocks or segments only. The gains to us from lack of clustering of the sample, both in terms of independence of observations and avoidance of claims of selection bias between neighborhoods, seem considerable.

Again using rough estimates from 1960 Census data, it appears that conducting the screening interview with every ninth household in the 15 designated tracts will yield approximately the number of eligible families required. This is, of 22,554 total households in these tracts. 2506 will be screened, of which approximately 250 will be found eligible and below our 100% support level. If early sampling at this proportion yields too few eligible poor families, the proportion can (if desired) be raised.

VI. The question of income ceilings need not be resolved before the screening results in Trenton are in. Families which are not terminated before the income questions will have their completed questionnaires turned in to us regardless of the incomes which they report. I propose that we decide income cutoffs when we know what the income distribution in Trenton actually is. If it is evident that the eligible poverty population by our standard is very small, we may want to relax the requirement that 70% of the sample be poor. If the income distribution is such that large numbers of families are just above our poverty standard, we may want to stratify by income in order to bring families significantly above poverty into the sample without outweighing the poor. Otherwise, we may be willing to accept that income cutoff level which yields approximately 70% poor and 30% non-poor families (or some similar proportion).

This should not present any additional problems for our experimental design, as I assume that tax treatments will be assigned randomly to sample families, however, selected. It will, however, present another hurdle to be passed at a very rushed and critical time.

NOTES:

The striking thing about this memo is its gross misestimate of the eligibility rate in Trenton. As future memoranda will note, we took into the
<table>
<thead>
<tr>
<th>Tract number</th>
<th>Racial pattern</th>
<th>Total households</th>
<th>Married couple households</th>
<th>Families with 1959 income &lt; $3000</th>
<th>Rank by %</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Number</td>
<td>% of all families</td>
</tr>
<tr>
<td>1</td>
<td>W</td>
<td>925</td>
<td>634</td>
<td>96</td>
<td>12</td>
</tr>
<tr>
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<td>W</td>
<td>1219</td>
<td>893</td>
<td>112</td>
<td>10</td>
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<td>W</td>
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<td>154</td>
<td>12</td>
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<td>W</td>
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<td>13</td>
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<td>W</td>
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<td>403</td>
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<td>1233</td>
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<td>14</td>
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<td>1928</td>
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<td>228</td>
<td>14</td>
</tr>
<tr>
<td>22</td>
<td>W</td>
<td>1998</td>
<td>1440</td>
<td>203</td>
<td>12</td>
</tr>
</tbody>
</table>

W — White
N — Non-white
* — Significantly mixed

33594 Grand total
(22554) Total — 15 low-income tracts
4614 Grand total
(3656) Total — 15 low-income tracts
sample every eligible family we found in Trenton with income below 150% of the Social Security poverty line, and the eligibility rate for this over-all group, both poor and non-poor, was less than half of the projected rate for the poor alone. The ratio of eligible poor families to total households contacted turned out to be less than two percent. And this was to be the highest eligibility rate of any city in the study.

There was pressure at the time to raise rather than lower the income ceiling, but the 150% level was affirmed, and the question of income stratification was deferred to become part of a comprehensive design model for the experiment, which will be discussed later.
3. Definition of the Tax Unit

This topic differs from the previous two, which were principally questions of experimental design, in that the major consideration is how a national negative tax program might be expected to work. The task of the experiment is to anticipate this operation (viewed optimistically, to devise the best possible procedure) and to simulate it as closely as possible.

Because the definition of the tax unit is an important feature of any national negative tax plan, efforts to develop a proper definition had already been made by economists and lawyers, and were available for our use. However, in addition to the basic definition, we had to develop some purely experimental clauses, reflecting the fact that the program was not universal nor even available to all families meeting eligibility criteria. I wrote the following memo after early discussions with other staff members at Mathematica.

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TO: Members of GWIE Staff  
FROM: Heather Ross  
SUBJECT: Tax Unit for Graduated Work Incentive Experiment  
DATE: December 20, 1967

We propose the following rules for delineating tax units for graduated work incentive purposes.

RULE 1. All persons 18 and over are eligible to file for GWI payments unless they receive over 1/2 of their before (negative) tax support from another person 18 or older living in the same household.

RULE 2. Filers may claim as dependents all persons for whom they provide over 1/2 before-tax support, unless those persons are over 18 and do not consent to being so claimed. Such non-consenting adults may themselves file if they meet the criteria in Rule 1.

RULE 3. An eligible filer who is the parent or legal guardian of persons under 18 living in the same household with him (her), may claim those persons as dependents, regardless of who is supporting them. In all other circumstances, dependency for negative tax purposes will be determined entirely by the support test.

RULE 4. No person may be claimed as a member of more than one negative tax unit. For purposes of the experiment, no restriction against persons filing for both positive and negative taxes will be made.

These criteria are suggested as a compromise between welfare standards and standards embodied in the present positive tax. Strict adherence to welfare standards would require all persons pooling their income to meet major living expenses to file together and receive allowances adjusted (at least)
for economies-of-scale in group size. Disregarding the problems of delineating such units in legal terms and insuring proper allocation of negative tax payments within them, we observe that such a system would, depending on the structure of its payments, provide greater or lesser incentives to real or apparent fragmentation of expenditure units.

It is true that a schedule of allowances which just reflects economies-of-scale should be exactly neutral as to group size—prompting neither splitting nor consolidation of initial units. But this neutral result will not occur if the schedule is not accurate or if recipients do not perceive it to be. Also, families will clearly gain (if not discovered) by claiming to be many units when they qualify as only one. There is no evidence that such tax fraud would be more extensive than the limited amount of analogous fraud presently found under the positive tax, but it does not seem good policy to build in large money returns to easily-made misstatements, especially where the marginal utility of money is expected to be relatively high.

For all these reasons, it is desirable to move away from an expenditure or household definition of tax unit. We could as far as the Tobin-Pechman-Mieszkowski proposal to allow all persons over 21 to file separately. In their plan, economies-of-scale are reflected in negative tax payments only where children are present. We propose to go only part way in this direction and

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1 It is well to distinguish two kinds of family splits—those which occur when a family divides solely to maximize its financial gain, and those which result when people are enabled to exercise preferred living patterns which were formerly closed to them. The latter sort, a function of the general level of payments, is likely to be wide-spread under a generous negative tax plan (insofar as allowed by housing conditions), but it is the former type which is at issue here.

2 To make proper provision for economies-of-scale nationwide with a single schedule seems ambitious. Also, the urge to find a financial deterrent to childbirth seems likely to produce a program which grants payments to only the first eight or so household members, a limitation which, whatever its effect on the birth rate, has no economies-of-scale basis.
allow all self-supporting adults and adult household-heads, whether self-supporting or not, to file. Each of our rules is discussed in turn below.

RULE 1.

We realistically acknowledge persons 18 and over to be adults. This avoids assuming (imposing) a dependence between parent and child which, in large part, does not exist at these age levels, especially at low income.\(^3\) Where such dependence does exist, the support test of Rule 1 will prevent young adults from filing.\(^4\)

The gist of Rule 1 is that adults dependent upon other adults may not file unless those other adults live in other households. This is intended to group people, at least partially, into household spending units without forcing separate households to file jointly. As will be seen under Rule 2, these households may file jointly if they wish, but need not.

RULE 2.

Rule 2 allocates dependents to filers according to the support test of the positive income tax. However, filers supporting adults may not claim those adults as dependents unless the latter consent. Adults who do not agree to be claimed by other adults who support them and live in the same household with them, cannot be claimed by anyone. Adults who do not agree to be claimed by other adults who support them but live in different households, may themselves file. Thus, a mother with children who is receiving over one-half of

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3 It also avoids the financial incentives to early marriage which exist in the Tobin-Pechman-Mieszkowski plan.

4 Persons 18 and over studying full-time for their first college degree will be considered to be living in the household of their parents or legal guardians, regardless of their school-time place of residence, unless they are the source of over one-half of their own before-tax support. That is, if they are self-supporting, they may file for negative tax, if not, they may not.
her before-tax support from a father who has left the home may decide whether she wishes to be claimed as a dependent by the father, or file herself. If she and the children are claimed by the father, all support payments are intra-family transfers. If she files separately, she may claim the children as dependents (see Rule 3), and all support payments are taxable income to her, and deductible from income for the father. 5

If the family is knowledgeable about the negative tax program and wants to maximize its total negative tax payments, separated husbands and wives will file separately unless the payments are on a per-capita basis and the supporting parent earns so little that, after deducting for support payments, his income is still so low as to render him eligible for a negative tax payment, in which case the family will be equally well off financially whether they file separately or jointly. We expect that the negative tax program will be such that split families will prefer to file separately, and we do not wish to force joint filing in these cases.

**RULE 3.**

Rule 3 states that dependency for negative tax purposes will be established entirely on the basis of support, except in the case of an eligible filer who lives with children of whom he is the parent or legal guardian. Such a filer may claim those children as dependents whether he supports them or not. Thus, the mother filing separately in the example above may claim as

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5 This follows the positive tax procedure for such payments that whatever is taxable income to the wife is deductible from income for the husband, and what is non-deductible to the husband is non-taxable to the wife. However, under the positive tax, alimony is treated in the first way, and child support payments in the second. We propose that, for negative tax purposes, all transfers between separated spouses be treated as taxable income to the recipient and deductible from income for the donor, since this method most closely accords with the welfare standards of the negative tax.
dependents those of her children living with her. If she declines to claim them, the father may. 6

RULE 4.

Rule 4 insures that no person will appear in more than one negative tax claim. Certain other restrictions as to a person's appearance in both positive and negative tax returns would be necessary under a national negative tax program, but must be waived for the experiment.

- Application of rules to experiment.

We have proposed to confine the experiment at the outset to husband-wife families of some presumed permanence. The family unit to be enrolled will consist of the husband, wife, and all eligible dependents, as determined by statements of the family itself during screening interviews. Self-supporting adults living in the same household with eligible families will be considered separate tax units and will not be eligible to participate in the experiment unless they too meet the sampling criteria. All eligible family units identified in the screening process will be compiled in a list from which actual participants will be selected at random.

This separation of self-supporting adults, other than head and spouse, from the family tax unit will generally operate to the benefit of the household, in terms of total negative tax payments received. Such adults will likely be

6 Consider the case of a husband and wife living with the wife's sister and her children. The sister is self-supporting but the couple (husband) is supporting the children. Either the sister or the husband may claim the children, though the sister's claim takes precedence. Where there is no household conflict, the children may be expected to be apportioned in the way which maximizes total household negative tax payments. Where conflicting claims arise, the sister's has priority. The possibilities for gain by shifting children among eligible adults in this fashion do not seem disturbing. Note that when the children reach 18 their consent is the binding consideration on which adult eligible to claim them may do so.
earning more than the (marginal) basic allowance attributable to them divided by the negative tax rate, so that their income, if pooled, would cut back their basic allowance to zero and begin to reduce the allowances of other members. This observation is, of course, not true if the preferred behavior of the entire household in the presence of the negative tax is to stop work substantially or altogether.

Separation of self-supporting adults will also allow us to observe more closely the work-leisure decisions of individual family heads, rather than the group decisions of several earners who find their total income jointly determined through the negative tax. The former response seems likely to be of primary interest in our income analysis.

All members of the original family tax unit will remain eligible for negative tax payments according to the schedule of basic allowances by family size unless they are imprisoned, placed in a mental institution, join the Armed Forces, or leave the country. They will be ineligible only so long as they remain in one or another of these situations. This flexibility is adopted in order to avoid locking families into their initial position as a precondition for receiving continued payments.

We do expect a number of changes in family composition to occur. Children may leave the home to set up separate households. If they are under 18, they may still be claimed by parents who support them. If they are self-supporting or supported by some other adult, they are ineligible for experimental tax payments. When they become 18, they may themselves file for and receive negative taxes as single individuals, if they are self-supporting.

Children who reach 18 and are self-supporting may file as single individuals even though they do not leave the home. They may claim as dependents any one of the original tax unit for whom they are currently providing over one-half support. Similarly, any dependent adult members of the original family tax unit who become self-supporting during the course of the experiment (this includes becoming aged and eligible for Social Security) may file regardless of their residence and claim as dependents any members of the original tax unit whom they support.
Families where husband and wife separate may file jointly if one spouse continues to support the other. Otherwise they must file separately, claiming dependents as noted above. If dependents in the original family tax unit at any point in the experiment become dependents of adults not in the original sample of eligible families, they will be ineligible for negative tax payments as long as they remain in that status.

The purpose of the above procedure is to present families receiving experimental negative tax payments with as realistic a simulation of a (likely) national program as possible. To achieve this, we propose to allow maximum flexibility of behavior to persons initially selected as part of the sample, while prohibiting the entry of any new persons into the sample of eligible recipients during the course of the experiment. The only exception to the latter prohibition is children born to, or adopted by, women in the original sample. Not including persons who enter family units after the start of the program as eligible negative tax recipients avoids unrealistic and expensive experimental incentives to family enlargement. The exception for own children and adoptions is made because we are particularly interested in analyzing, if possible, the effect of negative income taxation of family enlargement of this sort.

• **A final observation.**

With the preceding definition of tax unit and an allowance schedule which reflects economies-of-scale there is clearly an incentive for families to maximize their number of earner-filers, either truly of fraudulently, at any given level of total income. This may be done by placing new members in the labor force and adjusting individual hours worked to achieve the same total income as before. It may also be done by attributing part of one person's

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7 We hope to be able to make some adjustment in our analysis for the differential dilution of negative tax payments caused by different degrees of unreimbursed family enlargement.
earnings to another. This will mean underreporting of the first person's income which we will be watching for anyway. Depending on the schedule of basic allowances and tax rates adopted, some families might also be able to increase their negative tax payments while increasing their total before-tax income by putting new family members to work. These sorts of incentives presented by the proposed definition of tax unit do not seem unacceptable.

NOTES:

After considerable debate, the major thrust of this memorandum, the use of a support test to determine dependency and dependency to determine family unit dimensions, was rejected in favor of an income-pooling criterion, wherein all persons residing in a given dwelling unit were part of the family tax unit unless it was clear that they did not make their income available to other household members, except perhaps through fixed amounts such as rent. No person residing outside the household could be claimed as part of the family unit, although people temporarily away from home — e.g., at camp, at school — could be so claimed. For families supporting persons outside the home at the start of the program or by court order, a standard deduction per person supported could be claimed.

Somewhat to our surprise, we did not encounter many of the jumbled household types which have come to be associated with low income living. This is not because they didn't exist, but because they were ineligible (usually, no adult male present) and were screened out in our initial survey. The final sample turned out to be predominately intact nuclear families, and thus the support and residence criteria would have achieved almost the same result.

The residence approach is easier to administer, since unit members must all live together and the burden of proof is on the unit to show that an adult living with them is not a rightful member. It is also closer in concept to the idea of an economic unit which underlies much negative tax theory. The difficulty with it may be seen as one of striking a balance. At one extreme is compulsory joint filing for all individuals sharing dwelling unit,
thereby forcing people out of the household if they wish to file separately or prevent their income from being applied against the unit's benefits. In the early days of the experiment, as we were trying to determine how lenient our rules for separate filing units within one residence should be, we found this kind of compulsion damaging, especially for young secondary earners who kept most of their income for their own use but wished to continue living at home. In most instances, we recognize such persons as separate filers and count as income to the main unit only the money which they actually contribute to the household, usually some fixed amount.

The other extreme is opportunity for declaration of separate filing units to the point of abuse, as for instance if a family was able to maintain that its head and chief wage earner was actually a boarder paying only small sums toward the upkeep of other family members. One could require that certain persons file jointly — many negative tax proposals stipulate this for spouses. This particular requirement discriminates against married couples, and its analog already exists in current welfare legislation to no good end.

Our approach to achieving the necessary balance has been to decide each family case, as it comes up according to a set of guidelines, and to abide by the precedents set. Our job in this regard has been made immeasurably easier in the first year of operation by the almost total lack of family effort to manipulate the system fraudulently. Requests for recognition of separate filing units within one household have been few, and focussed for the most part on teenagers leaving school and beginning to earn steady money for the first time.

The exact tax unit definition being used in the study appears in Appendix X, "Rules of Operation for the Negative Income Tax Experiment," which was prepared in major part by Project Director David N. Kershaw and myself.
4. Definition of Income

Like the definition of the tax unit, the definition of income for negative tax purposes is more a policy than an experimental design issue, and as a result, has already been dealt with at some length in the literature. The definition adopted by the New Jersey experiment conforms very closely with the general thrust of this work, and so I shall give only a brief description of the nature and intent of the definition here. The specific wording of the statute appears in Appendix X. Everyone of the research staffs at Mathematica and Wisconsin contributed to this effort, but special recognition should go to Professor Bernard Wolfman of the School of Law of the University of Pennsylvania who meticulously criticized early drafts and drew together the pieces of the final statute.

The income definition is intended to provide a comprehensive measure of families' financial resources. It includes all cash receipts from all sources accruing to all family members, except welfare payments and moneys required by the terms of their receipt to be used for specific purposes other than meeting family living expenses. In addition to these cash receipts, it provides for imputed income to home-owners and residents of subsidized housing. Deductions are allowed for alimony and support payments made to persons outside the tax unit, business expenses for self-employed persons, and housing expenditures for units with imputed income from housing.

The definition is designed almost entirely as a prototype for a national program, but a few features can be traced to the special circumstances of the

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4 See, for example: Green, op. cit., pp.
experiment. For example, we have very limited deductions, reflecting our limited administrative ability to police such items — we have a small staff with no official investigative officers and few of the powers of sanctions available to government agencies. Excluded are such things as medical expenses and casualty losses which would be hard to administer but which are nonetheless of major importance to our families.5 Perhaps deductions for these items will find their way into a national program.

For another example, the treatment of welfare benefits and positive income taxes were in large part determined for the experiment since welfare regulations and positive tax liabilities could not be altered for participants. Thus, welfare benefits are excluded from income to avoid an endless and purposeless iterative procedure whereby welfare taxes negative tax benefits and the negative tax program taxes welfare benefits. Also, coordination with the personal income tax is achieved by rebating personal taxes paid by negative tax recipients at the end of the year. A national negative tax program might operate in these ways, but only after choosing among options for more thorough integration which are not open to the experiment.

A final example is the absence of any asset test for eligibility or any imputed income from assets (other than homes) in the definition of income. A national program would almost certainly have some such feature, and early drafts of experimental rules contained various proposals. Attention to this matter lagged, however, when it was clear that no reasonable provision would have any impact for the kind of people we were finding in our surveys.

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5 Disease and temporary or partial disability are among the chief causes and effects of low income status in the sample, and there is no Medicaid program in New Jersey except for welfare recipients. Fire takes a continuous toll — eight of our families were burned out of their tenement building during the first week of operation in Paterson. Theft of large sums of money from participants on the street or from hiding places in their homes is not uncommon. Families hit by such major losses are bewildered by our refusal to recognize them.
In closing, let me point out that this experimental definition of income has none of the special provisions common to social welfare legislation. It has no exclusion for earnings of children or for income set aside for future educational needs of children. It has no flat deduction for "costs of employment" or for day care expenses. Some of these provisions are more defensible than others, and some, such as child care costs, appear as deductions under the personal income tax. I am not at all sure that it is advisable to introduce them into the experiment at this point, but there is great interest in including those that now appear in the proposed Family Assistance Plan, and I expect that some movement in this direction will be made in the near future.

5. Coordination with Other Taxes

This is one topic where, as mentioned above, the options of the experiment were limited in that it could only consider ways of fitting in with existing programs whose operations were given. However, within this limitation, a major range of choice still existed as discussed in the following memo.
TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Tax Rate for Graduated Work Incentive Experiment
DATE: November 20, 1967

Professor James Tobin has suggested a negative income tax plan under which family units are subject to negative taxation until their resulting tax liability equals their liability under the positive income tax, at which point they enter smoothly upon the positive tax schedule. This is an excellent method of coordinating positive and negative taxation in the (realistic) circumstances where definitions of income and filing unit are different under the two tax components. However, it means that throughout some transitional income range, families will pay less personal income tax than that required under the present code, a suspension of positive tax liability that we cannot duplicate in the work incentive experiment. Unless we wish to restrict negative tax payments to families with incomes below positive-tax levels, an arbitrary and undesirable limitation, we must devise our own method of coordinating positive and negative taxes.

Consider the outcome, in terms of effective negative tax rates, of the following four cases in which proportional negative tax plans are superimposed on the present positive tax setup.

**CASE 1.** Negative tax allowances are taxable as personal income and the income base for negative tax purposes is gross of personal income tax paid.

**CASE 2.** Negative tax allowances are taxable as personal income and the income base for negative tax purposes is net of personal income tax paid.
CASE 3. Negative tax allowances are not taxable as personal income and
the income base for negative tax purposes is gross of personal in-
come tax paid.

CASE 4. Negative tax allowances are not taxable as personal income and
the income base for negative tax purposes is net of personal in-
come tax paid.

These four situations are shown in Appendix, Charts 1 - 4 for the exam-
ple case of a family of size four with initial income = $4,000/year and as-
signed experimental tax parameters:

\[ \alpha = 2,500 \quad \text{(basic negative tax allowance)} \]
\[ \beta = 50\% \quad \text{(negative tax rate)} \]

As can be seen from the charts, the effective negative tax rate in all
these cases rises above the parameter value of 50%. There are two ways of
dealing with this:

(1) acknowledging it and raising the value of the tax variable used in the
income analysis accordingly, and

(2) adjusting the experimental negative tax rate to allow for the exist-
ence of other taxes on income.

Little can be said in favor of the first approach — setting negative tax
parameters to suit the experiment and accepting whatever cumulative outcome
arises. For example, where proportional tax rates were designated by the
experiment, this method would produce progressive effective rates. The sec-
ond, superior, approach allows us to achieve any desired effective negative
tax rate.

But we must first decide on which adjustment should be made, i.e.,
which of the preceding four cases, when adjusted, is the preferable one.
CASE 3. Negative tax allowances are not taxable as personal income and the income base for negative tax purposes is gross of personal income tax paid.

CASE 4. Negative tax allowances are not taxable as personal income and the income base for negative tax purposes is net of personal income tax paid.

These four situations are shown in Appendix, Charts 1 - 4 for the example case of a family of size four with initial income = $4,000/year and assigned experimental tax parameters:

\[ \alpha = 2,500 \quad \text{(basic negative tax allowance)} \]
\[ \beta = 50\% \quad \text{(negative tax rate)} \]

As can be seen from the charts, the effective negative tax rate in all these cases rises above the parameter value of 50%. There are two ways of dealing with this:

1. Acknowledging it and raising the value of the tax variable used in the income analysis accordingly, and

2. Adjusting the experimental negative tax rate to allow for the existence of other taxes on income.

Little can be said in favor of the first approach — setting negative tax parameters to suit the experiment and accepting whatever cumulative outcome arises. For example, where proportional tax rates were designated by the experiment, this method would produce progressive effective rates. The second, superior, approach allows us to achieve any desired effective negative tax rate.

But we must first decide on which adjustment should be made, i.e., which of the preceding four cases, when adjusted, is the preferable one.
Consider the situation where we wish to establish an effective 50% rate until the resulting liability equals the regular positive tax liability (analogous to Tobin). The cheapest way to make this adjustment is to work from Case 3 or Case 4 where the shaded area of payments lost to the federal government is smaller. These cases also avoid the complexities of having taxable negative tax payments.¹ These complexities do not appear to have any offsetting gains, unless one feels that the public will be particularly unhappy about tax-free payments. Taxable status does open up the possibility of new legal problems for participants² and accordingly, adverse publicity for the experiment.

Case 4 may be easier to administer since it is net, or take home pay that participants will undoubtedly report. However, it certainly is feasible to gross-up all reported earnings to pre-income tax levels, and it is more appealing to count as income those taxes which we are giving participants negative-tax credit for having paid. Thus, we propose that negative taxation be carried out along the lines of Case 3, i.e., negative tax payments non-taxable under the personal income tax, and an income base for negative tax purposes gross of personal income tax.

As a final observation, it seems reasonable to disregard the existence of payroll taxes when setting negative tax rates. In a national negative tax economy, payroll taxes and negative taxes are likely to exist side by side, with earners bearing their cumulative effect. Where Social Security payments

¹ We may easily arrange to have our payments ruled taxable as the income of independent contractors (like doctors' and lawyers' fees) but it is doubtful that we can get a wage or salary ruling. Thus, we will probably not be able to withhold taxes on our payments and the resulting year-end adjustments may cause problems for recipients, depending on the income period we adopt for payment purposes.

² Members of the IRS are required to report immediately all instances of tax fraud which come to their attention. If, as seems useful, we enlist IRS cooperation in checking income statements made to us by participants, we will open these families to special tax scrutiny. The resulting discoveries of fraud will likely be fewer if our payments are non-taxable, since fewer families will then be liable for positive tax.
are included in the negative tax base, as in our program, there is good reason not to tax contributions also. Thus, we propose to exclude payroll taxes from our negative tax definition of income. However, we may want to take account of payroll taxation in our analysis of work behavior.

- Description of charts.

The foregoing charts show the range of outcomes, in terms of effective tax rates, which will result from the imposition of a proportional negative tax of 50%, depending on whether negative tax payments are taxable and whether the income base for negative taxation is net or gross of positive taxes.

These outcomes are summarized below:

<table>
<thead>
<tr>
<th>Pre-tax income level</th>
<th>Case 1</th>
<th>Case 2</th>
<th>Case 3</th>
<th>Case 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 – 1,000</td>
<td>.50</td>
<td>.50</td>
<td>.50</td>
<td>.50</td>
</tr>
<tr>
<td>1 – 2,000</td>
<td>.57</td>
<td>.57</td>
<td>.50</td>
<td>.50</td>
</tr>
<tr>
<td>2 – 3,000</td>
<td>.57</td>
<td>.57</td>
<td>.50</td>
<td>.50</td>
</tr>
<tr>
<td>3 – 4,000</td>
<td>.575</td>
<td>.5155</td>
<td>.64</td>
<td>.57</td>
</tr>
<tr>
<td>4 – 5,000</td>
<td>.575</td>
<td>.5112</td>
<td>.65</td>
<td>.575</td>
</tr>
<tr>
<td>5 – 6,000</td>
<td>.16</td>
<td>.5128, .16</td>
<td>.16</td>
<td>.58, .15</td>
</tr>
</tbody>
</table>

If we may somehow be permitted to pass over the most obvious anomaly in these schedules, the rate drop in the $5,000 – $6,000 income range, we see that some modification of experimental negative tax rates to adjust for the existence of positive rates is desirable. Following Tobin's technique for Case 3, with a 50% rate, we get the following experimental tax rates:
Chart 1

Intact Family of Four Assigned Negative Tax Parameters

\( a = \$2,500 \quad \phi = 50\% \)

CASE 1. NIT payments taxable and
NIT income base gross of positive tax

After negative

tax income

\[
\begin{align*}
\text{positive taxes due} & \\
\text{positive tax schedule} & \\
\text{negative tax payments} & \\
\text{total gross income} & = $4,000 \\
\text{total net income} & = \text{initial effective tax rate} = 57.5\% \\
\text{(disregarding payroll taxes)} &
\end{align*}
\]

\begin{align*}
\text{Negatively taxable income} & = \begin{array}{cccccc}
1,000 & 2,000 & 3,000 & 4,000 & 5,000 & 6,000 \\
\end{array}
\end{align*}
Chart 2

Intact Family of Four: Assigned Negative Tax Parameters

- \( \alpha = \$2,500 \)
- \( \beta = 50\% \)

CASE 2: NIT payments taxable and NIT income base net of positive tax

After negative tax income

- $6,000
- $4,000
- $2,000

Positive taxes due

Positive tax schedule

With initial income = $4,000, this family's initial effective tax rate = 51.55%

Negative tax payments

Total gross income

Total net income

(disregarding payroll taxes)
Intact Family of Four Assigned Negative Tax Parameters:

$\alpha = 2,500 \quad \beta = 50\%$

CASE 3. NIT payments non-taxable and NIT income base gross of positive tax.
Intact Family of Four Assigned Negative Tax Parameters

\[ \alpha = \$2,500 \quad \beta = 50\% \]

CASE 4: NIT payments non-taxable and
NIT income base net of positive taxes

After negative tax income

- Positive taxes due
- Positive tax schedule

With initial income = \$4,000, this family's initial effective tax rate = 57%.

- Total gross income
- Total net income (disregarding payroll taxes)

Negatively taxable income
<table>
<thead>
<tr>
<th>Pre-tax income level</th>
<th>Negative tax rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - 1,000</td>
<td>50%</td>
</tr>
<tr>
<td>1 - 2,000</td>
<td>50%</td>
</tr>
<tr>
<td>2 - 3,000</td>
<td>50%</td>
</tr>
<tr>
<td>3 - 4,000</td>
<td>36%</td>
</tr>
<tr>
<td>4 - 5,000</td>
<td>35%</td>
</tr>
<tr>
<td>5 - 6,000</td>
<td>34% ; 0%</td>
</tr>
</tbody>
</table>

This new schedule of rates can be subsumed in a total tax table for experimental purposes. As suggested above, Case 3 is preferred to Cases 1 and 2 as the basis for adjustment because it is less expensive and avoids the problems of bringing new families and a new form of unwithheld income into the positive tax system. Case 3 is preferred to Case 4 on economic grounds — if families are to be given credit for positive taxes paid, then those taxes ought not to be deductions from income.

Note that different income definitions for positive and negative taxation will not cause (further) disruptive rate changes here — families will pay an effective 50% tax on comprehensive income until the resulting liability equals their positive tax liability, at which point gross negative tax payments to families will have been reduced to zero. However, differences in tax unit definitions could cause such discontinuities.

- **Practical considerations for tax coordination.**

If we grant the need for coordinating positive and negative taxes, and agree to do this by applying positive taxes to negative tax liabilities, a question arises as to the proper timing of this offset. Two possibilities are considered below.
METHOD 1.

We can make a year-end adjustment which will compensate each family for the net positive tax payments it has made throughout the previous year. This can be done by raising the family's regular negative tax allowance for April (or May) by the full amount of positive taxes paid in the previous calendar year, or by spreading the additional allowance over several months — as, for instance, in the case where positive taxes paid may be used only to offset negative taxes due (i.e., monthly allowances may rise no higher than 1/12 of basic annual allowance levels) but the excess of positive tax payments over negative tax liabilities may be carried over to later time periods until exhausted.

For the family shown in the previous charts — four members with a total annual income of $4,000 and negative tax parameters $\alpha = 3,500$ and $\beta = 50\%$ — the positive tax liability for one year (assuming all income was taxable) would be $140. Assuming one worker earned $80/week for 50 weeks, the family's regular monthly negative tax payment would be approximately $48, which, with a single-month lump sum adjustment, would be raised to about $188. The carry-over modification would not change this family's situation since it would be regularly paying a monthly negative tax of $1/2 (4)($80) = $160 > $140.$

The great advantage of this approach is that it permits the experiment to wait and observe families' net positive tax payments before making any adjustment. This is particularly simplifying where large numbers of families are expected to be withheld and file for year-end refunds. Yet, even if those families themselves expect to be withheld, an outcome into which often much more than their own private decision-making enters, they may still operate on a current-account measure of income. In that event, to disregard positive taxes paid throughout the year is to ignore an aspect of the very work-income decision we are investigating. Similarly, reimbursing positive taxpayers in a single lump sum for taxes paid throughout the year may be inappropriate.
METHOD 2.

We can limit lump-sum payments and keep the experiment on a more nearly current-account basis by crediting positive taxes as they are withheld. Where positive taxes withheld in any month are greater than negative tax liabilities (an unlikely circumstance) the excess can be carried over until exhausted. If persons receive a refund at the end of the year, that refund can be considered the equivalent of a negative tax payment from us. Where the refund is larger than the monthly negative tax payment, the excess can be carried over and offset against the following month's payment. The generally low level of refunds at eligible income levels is unlikely to produce any long term suspension or reduction of negative tax payments under this arrangement.

• Conclusion

If we wish to credit positive taxes against negative tax liabilities, Method 2, which applies this credit on a current account basis, seems preferable for our purposes. In the cases where persons' withholdings are equal to or less than their total positive tax liabilities, this technique spreads our adjustment smoothly across the year, reducing or eliminating year-end, lump-sum offsets. Where taxes are overwithheld, and here the amounts involved are likely to be small, the technique allows for the fact that tax transactions have taken place, even though they result in a wash at year-end. The complaint has been made that under Method 2 people may underclaim their withholding exemptions in order to get an interest-free loan from us. The implication is that they will gain thereby, but this will be true only in some psychological sense — if they prefer to pay more during the year to the U. S. government and less to the experiment. Financially, they are in the same position — namely, subject to an X% tax on comprehensive income — no matter what the mix of taxing agents, a mix which, of course, is adjusted to some standard value at year end anyway.
Reservation

However, the complexities of actually coordinating positive and negative taxes by either of the methods suggested above — obtaining accurate information about positive taxes due, paid, etc. — plus the relatively small dollar values involved makes one wonder whether it is worth it. If the experiment operated instead on the basis of Case 4 above (negative tax allowance non-taxable and negative tax base net of personal income tax), the effective tax rate changes introduced by the positive tax would perhaps be small enough to require no experimental adjustment, but merely numerical acknowledgement in the income analysis. It seems likely that no participant would at any time in the experiment be subject to marginal positive tax rates in excess of 19%. (This is the marginal rate for intact non-aged families of four with $7,000 - $11,000 income and for non-aged single individuals with $2,900 - $4,900 income.) Netting positive taxes from the comprehensive income base means that the negative tax would effectively make up a percentage of all these positive taxes — a percentage equal to the negative tax rate itself. Thus, with a 50% negative rate, half of all positive taxes paid would be reimbursed by the negative tax, and total effective tax rates on income would probably not rise as high as 60%.

We might be willing to accept this 10% maximum discrepancy (and the slight progression which went along with it). However, lower negative tax rates would produce greater maximum discrepancies and a greater degree of progression. Thus, varying the proportional negative tax rate would produce entirely different effective tax structures, though the dollar amounts involved would still be small. All in all, the more thorough technique (Case 3, Method 2) discussed above seems preferable unless it proves very difficult administratively.

NOTES:

In retrospect, this memorandum, one of the earliest, seems somewhat off the mark. Although its principal recommendation — that negative tax
benefits be non-taxable under the personal income tax and that income for
negative tax purposes be gross of income taxes paid — was adopted, the re-
commendation was made equivocally and some major arguments in support
of it glancingly or not at all.

Probably the chief consideration in preferring tax-exempt status for
negative tax benefits was the belief that this would be the realistic arrange-
ment for a national program. It would be in keeping with the consistent ruling
of the Internal Revenue Service that payments based solely on the need of re-
cipients do not constitute taxable income. This position was presented in a
request to the Internal Revenue Service in March 1968 for a ruling that expe-
rimental negative tax payments not constitute gross income to recipients un-
der Paragraph 61 of the Internal Revenue Codes. The ruling request, which
was prepared by Professor Wolfman, received favorable action two months
later.

The use of gross income for negative tax purposes was dictated by the
wish to fix the over-all effective tax rates facing recipients to certain fixed
proportions. Since recipients' personal tax liabilities could not be modified
or suspended for the purposes of the program, it was necessary to treat such
personal tax payments as payments in discharge of negative tax liabilities.
After granting this tax credit, it would be bad tax policy and bad experimental
design to allow a further deduction from income for these payments. Note
that the credit applies to all income taxes paid, although only Federal tax has
been involved to date, since neither New Jersey nor Pennsylvania nor any ex-
perimental locality within these states presently has an income tax.

Two recommendations in the memo were not adopted. No deduction for
payroll taxes was granted, largely because the administrative costs seemed
out of all proportion to potential benefits. Crediting of personal income taxes
as withheld (Method 2) was rejected in favor of year-end, lump-sum reim-
bursement of taxes (Method 1), again largely for administrative reasons —
obtaining consistent withholding information from families proved to be trou-
blesome and most withholding resulted in a wash transaction at year-end. By
far the most common personal tax treatment of families turned out to be appreciable withholding during the year followed by a complete refund at yearend.
6. Accounting and Payment Procedures

The accounting convention adopted for negative tax purposes is far more than just an administrative matter. By setting the timing of the tax bite on current income, it can have a major effect on work behavior and thereby program costs. By setting the sensitivity with which benefits respond to current income (the other side of the coin), it can determine the ability of the program to meet changing family needs, and thereby fix the role of the negative tax in a galaxy of income maintenance programs.

At the time of our design efforts, very little attention had been given in the literature to the question of accounting periods. While there was a consensus among the research staffs that different accounting systems could have very different impacts on family response, there was some difference of opinion about whether to conduct a formal test of such differences within the experiment. The following memorandum was written as a part of that discussion.
TO: GWIE Staff
FROM: Heather Ross

SUBJECT: Accounting Period and the Role of the Negative Income Tax

DATE: January 15, 1968

The Statute Group at the Institute for Research on Poverty has recommended that two distinct accounting periods be incorporated in the graduated work incentive experiment:

METHOD 1. Payment in each quarter based on average income for the previous 4 quarters.

METHOD 2. Payment in each quarter based on income estimate for that quarter made by recipient during previous quarter, with (calendar) year-end adjustment.

The group notes that such a difference in accounting period is likely to produce marked differences in incentive impact, and concludes that, this being the case, we would be remiss not to investigate those differences.

It would be hard to take issue with the first observation — if earners are at all sensitive to negative tax rates, the lag structure of Method 1 must surely elicit a different work incentive response from that forthcoming under the current basis of Method 2. However, it does not follow that we ought to investigate both procedures. One can conceive of many accounting techniques which may be expected to have distinctive incentive effects. Further arguments are needed before their inclusion in the experiment is justified.

In fact, the very pervasiveness of likely incentive differentials with different accounting set-ups suggests that we may be unable to accommodate more than one such set-up in a 1,000 — family experiment without undue loss of our statistical power to distinguish among experimental treatments. It is clear that the same nominal rate would, when administered by both Method 1 and Method
yield two entirely different effective rates on current income, to standardize on this measure, except in the special case of completely stable quarterly income. But it is doubtful that the timing variation is equivalent to merely establishing a new set of rates on current income. Workers' responses to lagged taxes will almost certainly depend on such things as time horizons and time preferences which are likely to be significantly related to age, education, initial income, family responsibility and other explanatory variables in our work behavior analysis. That is, the timing variable is likely to interact so completely with other independent variables that we will have, in effect, produced two separate parallel experiments whose data cannot reasonably be pooled. Papers by Glen Cain and Guy Orcutt indicate that 500-family experiments would not themselves have enough discriminatory power over treatment effects to warrant undertaking them.

The experiment could be expanded to make room for two accounting periods, but there are several reasons for questioning this move. In the first place, it will be difficult in three years to gain conclusive evidence about the impact of a tax program which operates with a full-year averaging provision. During the first year of the experiment, negative tax payments under Method 1 will depend predominately on pre-experiment income (probably calendar year 1967 income) reported retrospectively by recipients. This previous year income will differ importantly from later income with which it will be averaged.

1. It will not be known as accurately, either to recipients or to us. This is especially true for the quarterly figures which strict adherence to Method 1 will require.

2. It will be reported to us in different circumstances. Families will indicate their initial income level during the screening interview, without knowledge of the tax use to which this information will be put. Such knowledge might produce entirely different income estimates, either through fraud, correction of conscious overstatement, or just more careful thought.
(3) It will have been realized without regard to the tax variables being tested. In light of these non-comparabilities, it seems unwise to give initial income experimental tax leverage for as much as a full year.

Similarly, under the (unweighted) averaging provision of Method 1, an average of 62.5% of third-year income will escape negative tax entirely. Our hope of observing more permanent effects of negative taxes with passing time will be jeopardized enough as families react to approaching program termination, without permitting this growing irrelevance for current behavior of negative tax variables themselves.

In sum, testing a negative tax plan of the Method 1 type does not seem a reasonable objective for a three-year project. Only 1/3 of the experimental period (the 4 quarters of year 2) will truly simulate the analogous national program.

Again, the experiment could be expanded in time to permit a more substantial test of Method 1. But this does not appear to be a high priority use of our funds, since it is not clear that the 4-quarter averaging approach of Method 1 commands enough policy interest to warrant testing it. It is true that the negative tax is better suited to meeting the average income needs of poor families than their fluctuating day-to-day needs. A thorough income maintenance system will very likely require ancillary non-federal programs to meet emergencies, etc. But it does not follow that the negative tax ought to be as basic as Method 1 — i.e., highly insensitive to changes in current income. This is particularly true in light of the evident income instability of the poor. The rapid turnover of AFDC-UP and general assistance caseloads is, in large part, testimony to this continuing instability. Our own inquiries have shown the standard deviation of year-to-year income changes for households with (1st year) income below $3,000 to lie in the $500 - $1,000 range.

In these circumstances, it seems questionable to create an income maintenance program which fails completely to respond to current income change for as much as three months (and then responds only partially) and which virtually ignores widely prevalent and often severe seasonal variations. That
approach leaves to welfare much more than the emergency case,\(^1\) and raises the prospect of continual large-scale entry and exit from welfare rolls. It is precisely these movements — the opening and closing of cases with attendant investigations — which are so burdensome on both staff and clients and so time consuming.

However, such oscillations are, in fact, unlikely to occur at any near time, since transforming 50 state welfare programs into appropriate supplements to the negative tax will be a monumental task. In the meantime, many families whose needs are not met by the tax will have no recourse. But even if welfare is speedily transformed — i.e., made non-categorical, with no asset liquidation requirement, self-administered declarations of eligibility, no fallacious income imputations, less than 100% tax on other income, adequate payment levels (uniform standard adjusted for living costs), etc. — it will only have been turned into another negative tax — at best a duplication of effort, at worst a running conflict (it will presumably negatively tax negative taxes). Such an outcome can be largely avoided in the beginning by making the negative tax as finely-tuned an income maintenance device as possible within a tax context, and reserving to welfare the provision of social services and truly emergency assistance.\(^2\)

It has been argued that Method 1 will relieve the negative tax of the need to make frequent welfare-type investigations. The implication here is

\(^1\) In the event of income drop through loss of earnings in covered employment, unemployment compensation could fill in while negative taxes were catching up. However, our investigation of the New Jersey Unemployment Insurance Law indicates that a large proportion of our proposed low-income sample will likely be ineligible for these benefits. This poor coverage of low-income earners is typical of unemployment insurance laws generally. Any such reliance on unemployment compensation as a short-run negative tax supplement would probably require a rethinking of our income definition to render these benefits non-taxable.

\(^2\) Welfare might also, be paying extra benefits, carry out the preferences of any state which wished to maintain its residents’ incomes above the federal standard.
that shorter accounting periods will necessarily entail greater incursions into taxpayer privacy. This surely need not be true. The period over which income is averaged for (semi-monthly) payment purposes need not have any bearing on the types of amounts of taxpayer information which are to be verified in any auditing procedure. A year-end adjustment mechanism can be superimposed on any short-period accounting plan (as in Wisconsin Method 2) so that knowledge of short-term (weekly, monthly, etc.) income is not a special requirement for auditing such plans. Knowledge of short-term income is then required only to the extent that it is needed to generate an accurate figure for annual income — exactly the extent to which it is needed under Method 1.

Finally, income accounting à la Method 1 is no assurance that families will not be closely scrutinized. Perhaps the best evidence of this appears on page 15 of Wisconsin's report, where it is stipulated that Method 1 will combine an annual accounting of income with as much as a daily accounting of family composition.

On the whole, it is reasonable to expect that any negative tax system will seek to avoid welfare-type investigations, and that success in this endeavor will not hinge on the income accounting period adopted.

It has been argued that Method 1 will reduce the risk of loss from error or fraud, and will withhold payments to families which occasionally dip below the poverty line. Both these statements are true, by virtue of the fact that Method 1 responds slowly to any change in reported income, be it erroneous, fraudulent, or in the vicinity of the poverty line. The first point seems fairly unimportant, and the second largely undesirable. Welfare studies suggest that the crucial moment for giving aid is when families begin to drop into situations of need — small amounts invested at this time can often prevent larger more permanent payments later to families which have exhausted their resources and become truly dependent.  

3 This argument is, of course, much less compelling for negative tax programs with break-even lines well above poverty levels.
It is also true that Method 1 will likely dilute the work disincentive effects of the negative tax by delaying full taxation of current income. Even longer accounting periods would probably blunt the impact of tax rates on current earning decisions even further. But at some point, the income maintenance goal of negative taxation must become binding, and previous arguments have attempted to show that this point is reached before income averaging is extended to a full year.

A. Counterproposal

MATHEMATICA has proposed that a single accounting period — a three-month moving average with year-end adjustment — be used in the experiment.4 This could be implemented in the following manner:

On the first day of each month, families mail to a central accounting office a card stating their income for the previous month and their size at the middle of that month. On the fifteenth of the month, checks are mailed to families in an amount equal to 1/24 of the annual negative tax payment for which they would have been eligible if their annual income had been equal to four times their average income for the previous three months, and their family size equal to their average family size for that period.5

On the last day of the month, checks are mailed to families in the same amount as that paid out earlier in the month. At the end of the year, families' annual negative tax entitlements based on their full year incomes are calculated, and any discrepancies between these amounts and the sums of actual payments made throughout the year noted. All underpayments are made up in the May checks of the following year, and all overpayments deducted from the

4 See "The Definition of Income for the Graduated Work Incentive Experiment" by Michael Taussig, undated Mathematica memorandum.
5 Each month, families will be credited for the full month with the family size reported at the middle of the month.
May checks, subject to the condition that deductions from each check may not exceed one-half of the semi-monthly payment which the family would receive if it had zero income. Where deduction of overpayments due involves reducing current payments below zero, the excess is a liability to families to be met by cash repayment. Defaults on repayments are deductible from subsequent checks, subject to the above condition.

B. Rationale

(1) The three-month accounting period responds to current income sensitively enough to reflect families' changing short-run needs, but not so sensitively as to produce substantial payments to persons, such as vacationers, who voluntarily reduce their incomes for short periods. It can be thought of as analogous to Wisconsin's Method 2, where the estimate of income in any month is made by averaging income for the previous three months. This method of estimation reduces the problems of

(i) overpayments throughout the year based on continual income underestimates, and

(ii) large lump-sum adjustments at year-end (or alternatively, belated partial adjustments dragging through much of the next year), which one may expect to encounter with Method 2.

(2) The semi-monthly payment of benefits coordinates with our monthly basis for income reporting and averaging, and has proven satisfactory in a wide range of welfare programs. As to the experience of the New York City Welfare Department (see Wisconsin Report, page 14), former Commissioner Ginsberg encourages us to adopt their semi-monthly payment period.

(3) The year-end adjustment promotes horizontal equity among house- holds with identical annual earnings. Without it, annual negative tax receipts would depend crucially on the time pattern of income over the year. One might argue that a final annual accounting is merely an arbitrary holdover from the positive tax which, like definitions of income and tax unit, should be
modified to achieve income maintenance goals. Eliminating year-end adjustments avoids the nuisance of modifying previously paid benefits for every filer whose income undergoes a net rise or fall over the year. It also rewards income increases and penalizes income declines, a treatment which may enhance work incentives. In order to prevent net payments to persons whose incomes fluctuate greatly around a high average, the year-end adjustment can be imposed on those who, as a result, are shown to be ineligible for any payments, although this method produces discontinuities in tax liabilities which can be substantial in some cases.

NOTES:

The arguments of this memo appear somewhat overdrawn to me now. In particular, I think it would be an extreme case if two accounting systems could not be analyzed within a single experimental framework utilizing regression techniques on pooled data. Also, I must acknowledge that long-period accounting has emerged as a matter of definite policy interest in some quarters. For example, it is reportedly being considered now for Nixon's Family Assistance Plan.

The Wisconsin group responded to this memo, making three main points:

(1) The problem of the shortness of the experiment must be faced no matter what the accounting period. Since start-up and close-down biases "must be dealt with anyway, their presumed magnitude is of less importance ... It seems valid that the lagged structure would have some disadvantages, but it would be only marginally worse than the unlagged scheme." 6

(2) The lagged structure cannot be dismissed as a live policy alternative. It has a number of attractive features — e.g., it eliminates the problems of over-and-under-payments and resulting adjustment mechanisms and experimental results may show it to have valuable incentive properties.

6 Harold Watts, memo to Mathematica GWI Group, January 31, 1968.
The experiment would be remiss and its utility undermined if it did not provide some basis for estimating the effect of such an obvious program variation.

The memo closed by suggesting that both accounting provisions be carried along in our plans until further work was done on sample design which would indicate whether this additional variation could be accommodated in an experiment of the projected size (cost). It noted that an even sample split between the two structures was not the only possibility, and that fractional replication of all or some negative tax variants using the lagged scheme could be considered and would be better than nothing.

Discussion of a lagged accounting system lapsed for the most part after this exchange, and the issue remained dormant at Mathematica for well over a year. Then in June of 1969, Wisconsin announced that it had received approval and supplementary funds from OEO to add a group of lagged-accounting-period families to the study. These families were to receive checks on the same schedule as other families, but the two checks paid in each period were to be based on average income over the previous year. Wisconsin proposed that the group consist of 65 families allocated over four tax plans:

- 75% guarantee/30% tax rate
- 75% guarantee/50% tax rate
- 75% guarantee/70% tax rate
- 100% guarantee/70% tax rate.

The intent of this allocation was to provide observations across the full range of tax rates at a single guarantee level so as to test the interaction between timing and rates of tax, and to focus observations upon high tax plans where the lag structure might be expected to have greatest impact. Mathematica concurred in the addition of the group and in its allocation, and families subject to the lagged accounting provision were enrolled in the two remaining cities, Jersey City and Scranton.
This was, I think, a good move. By operating and comparing two accounting systems, even in a limited way, we should be able to make a useful contribution to the continuing policy debate about the proper role of negative taxation in an income maintenance system. There are those who believe that a tax oriented structure is not an appropriate or efficient mechanism for meeting changing current needs of poor families, that it is more suited to treating chronic poverty by placing a stable floor under income. Fluctuating needs can then be met by other programs — notably unemployment compensation, since job loss is a major source of income instability, and state welfare programs, which are expected to continue operation as supplements to any basic federal program. However, the experiment will not provide a good test of such a system, because a substantial proportion of our families are not presently eligible for these other programs. This is one of the drawbacks of experimenting with a lagged structure at the present time; and one of the problems with the system as a whole — its hard to get the parts to fit together and provide full coverage.

My own preference a priori is for our short, (basic) accounting system. I have a general preference for single comprehensive programs to accomplish broad social objectives or sets of objectives, falling back on a battery of more narrow programs only when a single program is shown to be unworkable, inadequate or unacceptably inefficient. If a negative tax structure cannot meet standard changing family needs (not unusual emergency situations) without great administrative inputs overlapping or duplicating those of welfare or without substantial detrimental effects on work behavior, then perhaps the income floor concept, with long-term averaging and reliance on other programs to fill in the gaps, will have to prevail. The experiment will certainly help us to make a better judgment on this.

As for the basic accounting system, this was the first area where we made a design adjustment after early field experience. Almost immediately in Trenton, we moved from a monthly to a multi-weekly basis for reports and payments — families reported their income and family size every four weeks
and received payments bi-weekly. This schedule conformed with the income pattern of participating families who were almost all paid weekly, and allowed them to file and to remember our time-table much more easily. Thus, the basic accounting system operated on a twelve-week (three report period) moving average, and the lagged accounting system, when it was introduced, on a fifty-two week (thirteen report period) moving average. As shall be seen later, the basic accounting system was to undergo a much more extensive renovation after a full years' experience in Trenton.
Chapter III
GETTING UNDERWAY: FEBRUARY - AUGUST 1968

This short chapter is useful primarily as continuity between the preliminary design stage of the project and the first year of operation of the negative tax program. It gives an opportunity to present two major operating decisions of that time which were to be of great value to the study.

The first decision concerned the mechanism for making negative tax payments. The original conception called for a total government image, with check-writing to be done by the New Jersey State Treasury which had offered to act as disbursing agent. That thinking is reflected in the following very early memorandum.
TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Legal Status of Graduated Work Incentive Experiment
DATE: October, 1967

Should participants in the experiment be enrolled on a formal contractual basis, with the rights and obligations of all parties spelled out in a legal document? This is certainly the most airtight approach to experimental organization, but it seems to have some drawbacks.

(1) It opens the possibility of lengthy litigation as, for instance, in the case where we might wish to apply some sanctions for misreporting income.

(2) It almost surely renders the experimental payments taxable as personal income. However, since they are not likely to be ruled wages, we will be unable to withhold income taxes on them. This places a legal and financial burden on participants for a year-end adjustment, which further complicates the experimental setup and would be better off avoided. Also, taxability of allowances raises the cost of providing any given net level of benefits to participants.

(3) It may deter families which would be willing to participate on a voluntary basis.

On the other hand, a more informal arrangement with participants does seem adequate for our purposes. Mr. Bernard Wolfman of the Law Faculty of the University of Pennsylvania, having researched certain precedents as to MDTA stipends, etc., is confident that our negative tax payments can be ruled non-taxable as transfers intended primarily to promote the welfare of recipients, with research results a secondary objective pursued in order to formulate a national program which will, in turn, promote the welfare of future recipients. This means that we may not require cooperation with interviewers as a precondition for receiving negative taxes. However, just as recipients of unemployment compensation must report periodically that they are still without a job, our recipients can be required to report their family
structure and income, as the bases of need upon which the benefit payments are predicated. For any further social-demographic-economic information, we must rely on the recipients' cooperation, though this need not be stressed in explaining the program to participants. But, in the end, it is voluntary cooperation that we are seeking — a contractual obligation is no assurance of accurate information if the respondent is unwilling. Also, this procedure conforms to a distinction which seems desirable on other grounds — that is, the divorce of income reporting from other data collection, the former requiring frequent, standard forms suitable for mail exchange, the latter involving less frequent, perhaps deeper, questioning on changing topics.

Mr. Wolfman also believes that an impartial panel set up by some administering arm of the experiment — e.g., within the State government — can provide ample due process for participants who wish to contest any of our findings as to income misreporting, etc., and the resulting sanctions, if any. He advises us to avoid the protracted and needless difficulties of going to court.

In sum, two main features of the informal approach may be considered drawbacks.

1. We must rely on the cooperation of participants to get information on topics other than family composition and income.

2. We will not be testing the willingness and ability of formerly non-taxable persons to comply with the personal income tax, except insofar as their before-negative tax income rises enough to become taxable under the personal tax.

Neither of these seems particularly damaging. In the first place, as noted above, accurate information will probably depend as much on good will as on a contractual obligation. And there will always be the ultimate threat that general uncooperativeness will lead to a termination of the entire program. In the second place, it is not clear that we want to involve participants in a new legal obligation, especially since we cannot withhold taxes on our payments. We will gain enough insight on tax compliance within the context of our own
program—a program which, given its government auspices and image, will not look much different from a genuine IRS operation to most recipients. All in all, it seems preferable to avoid legal contracts and keep our negative tax payments non-taxable.

The recommendation of an informal enrollment agreement as the basis for family participation was never seriously challenged, and was eventually adopted, with resulting gains in the non-taxability of negative tax payments and the independence of payment and interviewing operations. But the role of the state government came in for some second thoughts—specifically the feasibility and desirability of simulating in a limited experimental setting the governmental auspices of a national program. These second thoughts gave rise to the first formal discussions with New Jersey State officials, discussions which led to a mutual agreement of non-involvement by the State except for a modest local contribution required by OEO. This was an important modification for the experiment and a wise one. It removed any trace of dissemblance from attempting to portray the project as an operating public program. In doing so, it avoided certain kinds of confusions and threats to the experimental status of the project, and it eased our dealings with the public, other agencies, and individual families—both ineligible families who sought to join the program but could not, and eligible families who turned out to be most concerned about what government connections we might have. Our first public statement in almost every site where we entered included a disclaimer of association with any existing government agency or program in that community. This independence has differentiated us somewhat from any likely national program (whose government status I doubt we could have simulated satisfactorily), but it has contributed greatly to the continued successful operation of the study.

Our decision instead was to set up a special subdivision of MATHEMATICA called Council for Grants to Families, to handle all negative tax transactions. This name now appears on all negative tax checks and forms, and on field offices in our five cities. The results of this do-it-yourself approach
have been independence and great operating flexibility, both of which have served us well.

The second decision of importance came at the end of the tooling up period and concerned the survey component of the study. We had opened up the survey work to competitive bid in January of 1968. At that time, I had drafted the following memorandum which I include here for the overview it gives of our interviewing plans.
Memorandum of Field Work Needs of the Graduated Work Incentive Experiment

MATHEMATICA INC. is seeking bids from qualified organizations for the conduct of field operations for a Graduated Work Incentive Experiment. The purpose of this memorandum is to sketch the general nature of the experiment and to specify some of the technical details of the field work required.

I. Description of the Graduated Work Incentive Experiment

The Graduated Work Incentive Experiment is a large, long-range investigation, funded by the Office of Economic Opportunity, into the effects of a universal income supplementation program on the lives of low-income families in the urban United States. It is a pathbreaking piece of social science research which promises to provide unique guidance in the rational planning of social and economic policy.

A sample of 1,000 urban families in three cities will be selected for study. One group will receive graduated income support payments over a three-year period. The impact of the payments on work behavior and habits will be examined periodically through interviews and questionnaires. The second, or control, group will receive no payments. The purpose of work incentives is to encourage low-income people to work while continuing to provide them with a graduated scale-of-income support payments. The payments are reduced as they earn more money.

The experiment will seek to determine the extent to which payments increase or reduce work effort and the desire to work. It will also seek to measure the effect of such payments on marital stability, spending and saving patterns, mobility of families, and birth rates. It is expected to produce data on whether work incentive payments could help to eliminate poverty, the cost of a national program to the Federal government, and how changes in work patterns would affect the national income.

The experiment will be conducted in selected low-income areas of three New Jersey SMSA's. A field organization will sample and screen the low-
income population in these locations, enroll subsample of eligible families in the experiment, and then conduct quarterly interviews with the families over the course of the study. The major portion of the experiment will begin in late September of this year, with preliminary screening taking place about one month earlier. However, a preliminary phase of the experiment will begin this spring, preferably by May 1, following the necessary screening and enrollment processes.

MATHEMATICA wishes to contract with a field organization for the preliminary stage of the experiment, involving approximately 200 families in Trenton, as soon as possible. It is expected that the organization selected for the initial, Trenton phase of the experiment will continue as the field organization for the duration of the entire study. The choice of the field organization will be based principally on MATHEMATICA's judgment about the capabilities of bidders for the whole experiment, although an important consideration will also be the speed with which the selected organization can mobilize an effective field operation in Trenton.

II. Field Work Timetable

(1) MATHEMATICA will provide the field organization, immediately upon its selection, a list of approximately 10 Census tracts in the Trenton SMSA and instructions for sampling dwelling units in these target tracts.

(2) The field organization will undertake the tasks of identifying approximately 200 families in these tracts and enrolling them in the experiment according to the following three-step procedure:

(a) Following MATHEMATICA's specifications for randomly sampling dwelling units, the field organization will interview approximately 4,000 households, seeking families with

1. male head aged 18 to 58.
2. average annual income for past two years below 150% of the 1986 Social Security Administration poverty line for a family of their size (e.g., below $5,003 for a family of four).
The schedule of incomes will be provided by MATHEMATICA.

iii. Head who worked at least 20 days in calendar year 1967 and is not currently disabled or enrolled full-time in school.

Further information such as housing expenditure and status (public, owner-occupied, rental), and usual income source (wages, unincorporated business, etc.) will also be sought at this time. All households in which a male-head aged 18 to 59 is found will be paid for completing the screening interview. Evaluation of family eligibility will be based entirely on family responses during this interview.

(b) The field organization will supply to MATHEMATICA a list of all eligible families identified during the screening process. MATHEMATICA will select 200 of these families at random, and assign them to particular experimental treatments.

(c) The field organization will recontact the 200 selected families and offer them an opportunity to participate in the program. This will require explanation of an informational document, prepared by MATHEMATICA, which families who wish to take part must sign. Families which decline to participate will be replaced by other appropriate families selected by MATHEMATICA from the pool of families found eligible during screening.

(3) At the time families are enrolled, the field organization will conduct the first of its continuing quarterly interviews. This initial interview will focus on a limited number of objective features of families’ pre-experiment situations, but will include a few attitudinal questions as well. The results of these interviews will be rapidly conveyed to MATHEMATICA in raw questionnaire form. Control group families will be paid for completing this, and all subsequent, quarterly interviews.

(4) The preceding steps must be completed in time to inaugurate the three-year experimental payment period on May 1. If the field organization cannot meet this timetable, it should indicate its earliest possible completion date.
(5) Before the end of the initial contract period, MATHEMATICA will provide the field organization with similar information for the remainder of the experiment.

- a list of target Census tracts in two other New Jersey SMSA's.
- a procedure for sampling dwelling units in these tracts.

The field organization will proceed to identify and enroll 800 eligible families in the same manner as it did in Trenton. The target date for beginning experimental payments in these areas is October 1, with enrollment and first quarterly interview to take place in September.

III. Desirable Attributes of the Field Organization

(1) Interested field organizations should state their previous experience in working with urban, low-income populations. A detailed account of any such experience will be highly valuable in making the selection. The field organization should indicate its awareness of the special requirements for working with low-income respondents and operating in low-income neighborhoods, and its plans for meeting these requirements in this study.

(2) The field organization should be sufficiently flexible to conduct evening and weekend visits in order to obtain interviews with the male, working heads of families in the experiment.

(3) The initial contract will cover only the preliminary, Trenton phase of the experiment, but MATHEMATICA's overriding interests are in the long-range success of the entire study. Therefore, MATHEMATICA desires a field organization that can operate continuously over a three-year period in low-income neighborhoods. In its bid, the field organization should provide MATHEMATICA with its plans for maintaining a staff of interviewers that will undergo a minimum of turnover during this time. MATHEMATICA is also interested in the field organization's plans for holding the sample as intact as possible over the span of the experiment. As continued residence in New Jersey target tracts will not be a pre-requisite for continued receipt of work incentive payments, retaining an intact sample will require following families
which move within the territorial United States, and plans for carrying out this follow-up should be specified. In general, the field organization should indicate its plans for dealing with common problems encountered in long-range, recurrent interviewing.

(4) The project will be conducted as a realistic simulation of a public income maintenance program. Achieving this desired image will require an interviewing staff of high quality. The interviewers must have a satisfactory conceptual grasp of the experiment, as they will be required to explain it to participants when they enroll them. They must be able to communicate satisfactorily with participants, and their integrity must be beyond doubt.

(5) The field organization should indicate any further special contributions it can make to the experiment, such as aid in questionnaire design, etc.

IV. Proposal Submission

All proposals should be addressed to Miss Heather Ross, MATHEMATICA, One Palmer Square, Princeton, New Jersey 08540, and must be received by MATHEMATICA no later than February 1, 1968. Proposals should be submitted in two parts as follows:

PART I. A technical dissertation describing in detail how the organization would proceed if awarded the contract, a general history of the organization, a full description of its past and current experience in comparable work, and biographical summaries of key personnel who would be involved.

PART II. A separate cost estimate for:

Phase II — subsequent operations in Trenton and two other urban New Jersey sites to continue through September, 1971.

Representatives of interested organizations are encouraged to make arrangements to meet with MATHEMATICA as soon as possible, at which time
further details about the matters broached in this memorandum can be supplied and discussed. The target sites and family characteristics mentioned above are, of course, confidential information.
Of the four companies from whom bids were solicited, there was one whose submission was clearly superior and who was the unanimous choice of the evaluators. A contract with this organization for Trenton was signed in February and planning work got underway immediately. The basic outline of the soliciting memorandum was followed with the exception that two interviews were conducted with families before they were enrolled in the program. In other words, the first of the continuing quarterly interviews was administered to apparently eligible families before they were selected to participate in the program. This interview was used to verify responses to the brief screening questionnaire and to collect further baseline data. After the two interviews, families were declared eligible, randomly assigned to treatments, and enrolled. The enrollment was not done by the survey contractor, to its great relief, but by special representatives hired and trained by MATHEMATICA. Thus, the independence of the operating program from the survey evaluation of it was established right at the outset.

However, the actual field work was to become a major roadblock. As months dragged on, it was clear that the contractor could not meet our schedule. The screening job which they bid to do in three weeks was finally terminated by us after fourteen weeks. In addition, the feedback of field results to us was so slow and guarded that we could not adjust our sampling rate in time to achieve our target of 200 families.

After lengthy discussions we decided not to retain the contractor and not to reopen the remainder of the work to competitive bid, but to set up our own survey operation. This was a virtually unprecedented conclusion to reach, but we felt that our experience with the Trenton contractor had exploded certain myths about interviewing expertise and had pinpointed to us the shortcomings for urban poverty work of established survey organizations and techniques. Consequently, in late September, 1968, Urban Opinion Surveys division of MATHEMATICA was formed.

In direct parallel to Council for Grants to Families, Urban Opinion Survey turned out to be a great boon to the project. It gave us speed, flexibility,
control and major saving. It also gave us an opportunity to gain indepth knowledge of the communities and neighborhoods where we planned to operate, and a chance to establish our credibility, good will, and community involvement in advance. Much could be said about our distinctive interviewing operation and the contribution it has made to the project, but here it will suffice to state our overall finding — the value of an integrated operation and of hiring good people directly rather than contracting away responsibility.
Chapter IV

THE FIRST YEAR OF OPERATION: AUGUST 1968 - AUGUST 1969

The project’s first negative tax benefits were paid out to Trenton families during the last week of August 1968. Contrary to the optimistic schedule outlined in our memorandum to field organizations, it would be a full year before the final set of participants was enrolled. With Urban Opinion Surveys working as fast as it could (an average of 1,000 screening interviews a week), the following schedule emerged:

<table>
<thead>
<tr>
<th>Location</th>
<th>Enrollment Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>Trenton</td>
<td>August 1968</td>
</tr>
<tr>
<td>Paterson/Passaic</td>
<td>February 1969</td>
</tr>
<tr>
<td>Jersey City</td>
<td>July 1969</td>
</tr>
<tr>
<td>Scranton</td>
<td>September 1969</td>
</tr>
</tbody>
</table>

This full year was a period of trial and adjustment, especially with regard to Trenton whose small sample (125 families) we viewed as a pilot study within the study. On the basis of our experience in Trenton, and in other cities as time passed, we considered a number of significant modifications to the experimental design. The major issues are discussed in this chapter under four headings:

- (1) Eligibility requirements and negative tax parameter values.
- (2) Coexistence with welfare.
- (3) Treatment of persons who leave eligible family units.
(4) Changes in the basic accounting technique.
As will be seen in the following memoranda and notes, some of the proposed changes were adopted and some were not. But by the end of the period, the experimental design was effectively closed to all but minor alterations.

1. Eligibility Requirements and Negative Tax Parameter Values

Our first instructive finding began to emerge quite early in the Trenton survey work. We discovered that eligible families were very much rarer than had been estimated and that eligible poor families were very rare indeed. This meant two things. In the first place, it indicated that our eligibility criteria were much more restrictive than we had thought. Our interviews were identifying fairly large numbers of low-income families, but few of them met our family composition requirements. In the second place, it suggested that we might have to rethink our policy space in light of the large proportion of non-poor who were being drawn into the sample. These matters were the subject of two memos which I wrote while the interviewing and enrolling process was still going on.
TO: Members of GWIE

FROM: Heather Ross

SUBJECT: Proposals for Changing the Experimental Design

DATE: September 4, 1968

I. Background

We have learned several things in Trenton.

(1) Our field organization has been able to enumerate only 3/4 of the poverty tract dwelling units listed in the 1960 Census, and only 9/10 of the non-poverty tract dwelling units. (See my memo of August 9 for reasons.)

(2) The majority of dwelling units contacted have had their screening interview terminated before completion, primarily for lack of a male aged 18-58. Mike Taussig has some preliminary information on terminations but a thorough analysis must await return of the uncompleted questionnaires from the field.

(3) Among completed interviews, eligibility rates on the basis of screening only are running around 11% in poverty tracts and 5% in non-poverty tracts, both lower than expected. Early pre-enrollment returns have shown a significant proportion of these families, especially in non-poverty tracts, to be ineligible on income grounds.

(4) Of families judged eligible for participation at the present time (74), only 31% are poor.

As a result, we have had to undertake in Trenton a lengthier and more intensive screening operation than we had planned, in order to identify perhaps half as many eligible families as we had intended, fewer of whom are poor than we expected and very few of whom are smaller than size four. Our payments are presently running about 40% below the $1,500/year target we set several months ago.

II. Need for Adjustment

Our experience in Trenton suggests that since 1960, poverty has become increasingly confined to incomplete family units and to deteriorating core areas
of cities. Our search in non-poverty Census tracts has been largely unpro-
ductive, and our search in general has screened out the commonest type of
poor family to settle upon the atypical case and the lower reaches of the middle
class. We are in danger of merely skimming the surface of poverty, and in
danger of spending millions to set up and evaluate a program which makes ex-
ceedingly small payments. Our operating costs in this fiscal year will defi-
nitely exceed our direct payments under the present setup.

III. Proposals

(1) Confine our sampling in the next two cities to official poverty tracts
or to some comparably sized set of tracts ranked according to some more re-
cent index of poverty. Mike Taussig has estimated that this limitation could
cut our screening requirements in Paterson-Passaic by about 20%. This esti-
mate does not include three factors on which we have recently had experience
— loss of families due to inability to administer the pre-enrollment interview
(moves, refusals), ineligibility of families based on pre-enrollment data, and
refusals to participate at time of enrollment. Since these losses have been
heavy in non-poverty tracts, Mike's estimate may understate the true savings.

If we do decide to sample more than poverty areas, we should certainly
do it on a cluster basis. But sticking to poverty tracts, of which there are 26
in Paterson-Passaic, seems now to be a preferred course of action.

(2) Extend eligibility in the next two cities to families whose head is a
woman between the ages of 18 and 58, who has at least one child or aged depen-
dent living with her, and who has worked for pay at least ten weeks during 1968.

We originally decided to exclude such families (among others) from the
sample on the grounds that families with and without male earners were sig-
nificantly different tax units for purposes of our analysis, and that the scope of
the experiment permitted us to deal only with our primary interest, intact fam-
ilies.

A further reason for excluding female-headed families was to avoid over-
lap and possible conflict with welfare. Now that New Jersey has enacted a UP
law (i.e., a provision which extends eligibility for Aid to Families with Dependent Children to families with a male earner who is unemployed or underemployed), this argument is only one of degree — that is, of minimizing overlap.

Given the Trenton results on family composition at poverty levels, and the need (anticipating the next section) to make some adjustments for the shortfall of our own payments to date and for the coming of UP, including families with female heads seems a desirable extension of the study. Perhaps the labor force requirement in the above proposal is too strict, though some evidence of earning capacity should be called for. We might consider putting such families in the sample in Trenton as well as in the other two cities — we have spent a good deal of time and money identifying them (partially) already and further interviewing of a sample of terminations is not out of the question, though it would set off the Trenton payments timing. We might wish to allow female-headed families in the sample if there is any family member between the ages of 18 and 58 who is a full-time earner. The above proposal is primarily for purposes of discussion.

(3) Raise our 100% guarantee from approximately the Social Security Administration poverty line to approximately the AFDC support standard for the State of New Jersey, and adjust the 75% guarantee and 50% guarantee accordingly.

The new UP program requires us to make some adjustment of this sort if we are to be sure of retaining any but the largest poor families under our experimental control. Acceptance of proposal 2 makes adjustment doubly necessary.

The present and proposed guarantee levels are shown in Table I for comparison purposes. The new levels were calculated by raising the 100% guarantee for all family sizes in proportion to the excess of the New Jersey AFDC standard for a family of four over our poverty standard. This adjustment will mean a rise in basic income payments ranging from a minimum of 15% up to infinity. Based on the average payment made to date in Trenton and the average
tax plan, the change in guarantee levels should raise payments an average of 45% — from $920 to $1334 on average. The new guarantee will also help us cut down on the number of $2.50 minimum payments we make to families with incomes above their break-even levels — currently more than one out of every four families get such payments, a proportion which I believe is too high.

Changing the payments structure will cause us some difficulty in informing Trenton households of the change, allowing for the liberalization in our analysis of Trenton response (if necessary), and printing up new tax tables. But these inconveniences are far out-weighed by the resulting gains:

(1) bringing down the excessive proportion of experimental families who receive virtually nothing from the program,

(2) decreasing substantially the likelihood that AFDC-UP will seriously disrupt our program, and

(3) raising our average payments to achieve something near our original target level and to avoid an embarrassingly high ratio of operating costs to direct payments.

I think the inauguration of UP has brought into focus the entire issue of how relevant our policy space is. It is hard to accept as forward-looking a program which must go into a state with backward welfare laws to operate successfully. This is not to say that a negative tax program would necessarily operate nationally at the present AFDC support levels of northern industrialized states. But in an experimental program to inform future policy decisions, I find it hard to believe that the highest support level of interest is significantly below the level already featured in existing income maintenance programs.
Table I
Guarantees for Basic Income Plan

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Present: Standard = SSA Poverty Line</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>50% Standard</td>
</tr>
<tr>
<td>2</td>
<td>$1,000</td>
</tr>
<tr>
<td>3</td>
<td>1,375</td>
</tr>
<tr>
<td>4</td>
<td>1,650</td>
</tr>
<tr>
<td>5</td>
<td>1,850</td>
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<tr>
<td>6</td>
<td>2,025</td>
</tr>
<tr>
<td>7</td>
<td>2,175</td>
</tr>
<tr>
<td>8+</td>
<td>2,300</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Proposed: Standard = New Jersey AFDC Standard</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>50% Standard</td>
</tr>
<tr>
<td>2</td>
<td>$1,150</td>
</tr>
<tr>
<td>3</td>
<td>1,588</td>
</tr>
<tr>
<td>4</td>
<td>1,900</td>
</tr>
<tr>
<td>5</td>
<td>2,125</td>
</tr>
<tr>
<td>6</td>
<td>2,325</td>
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<tr>
<td>7</td>
<td>2,500</td>
</tr>
<tr>
<td>8+</td>
<td>2,650</td>
</tr>
</tbody>
</table>
TO: GWIE Staff

FROM: Heather Ross

SUBJECT: Improving the Experimental Design

DATE: September 9, 1968

Our work in Trenton has raised two fundamental questions:

(1) Are our eligibility requirements too narrow to yield a representative group of families of national policy interest?

(2) Are our payments too limited to cover the relevant policy space for future income maintenance decisions?

Consider the following sets of facts:

(1) From 2450 dwelling unit contacts to date (Sept. 3) in Trenton, it appears that our maximum net yield of enrolled sample families will be 6%. This is an estimate because our field organization still has approximately 70 pre-enrollment assignments outstanding. It is a maximum estimate because we sent the most-likely-eligible prospects into the field for pre-enrollment first, and because we have not yet followed up control group families to seek their continued cooperation. When the sample is finally formalized, the net yield will probably lie between 4% and 5%.

(2) Of the 2450 contacts, 1573 or 64% had their screening interview terminated before completion. Over 70% of these terminations were caused by lack of a male aged 18-58 in the household. The number of terminations due to lack of a male exceeded the total of completed interviews by 28%.

(3) The great majority (70%) of the families judged eligible on the basis of screening are above our poverty lines. Evidences of bias in this non-poor group have already begun to appear. Refusals to participate in the pre-enrollment interview are reported to have occurred primarily with higher income respondents. All six families which have refused enrollment have been substantially non-poor; 4 have been above the break-even lines of their assigned plans. Further unbalanced attrition may occur over three years in the experimental group as interest wanes and incomes rise.
(4) The proportion of experimental families receiving $2.50 checks was 26% in late August. As of Sept. 3, it had risen to 30%. This trend may be expected to continue, since low-income families whose eligibility was most certain were sent out for pre-enrollment first.

(5) The Social Security Administration poverty lines on which we based our poverty standards are inadequate for our purposes and out of date. The SSA is the first to admit that its original poverty lines (an average of 75 cents per person per day for food and $1.50 for all other needs) were drawn appallingly low so that no one could possibly doubt the desperate need of families falling under them. They are not appropriate ceilings for an experimental program providing data for future policy design. Furthermore, the present levels, which relate to the calendar year 1966, are hopelessly outdated. Between 1959 and 1966, poverty levels were raised 11% to account for increasing prices. 1967 and 1968 have seen price rises of almost 4% per annum which must soon be incorporated in the SSA index. By the end of our study, the 1966 poverty lines will have become a thoroughly irrelevant vestige of the past.

I. On Eligibility

The old arguments about tax units of different composition responding differently, about avoiding conflicts with welfare, and about letting welfare experiment with family types which characterize its caseload still hold, although the latter two ring somewhat hollow with the advent of AFDC-UP in New Jersey. What we must face is that the type of family we have chosen to focus on is exceedingly rare in urban non-poverty facts and surprisingly scarce even in the impoverished core area. The increasing confinement of poverty status to fragmented family units is a trend we can well document in Trenton and one which short of a general economic downtown, is bound to continue. The 1970 Census may show us far out on a limb in terms of representativeness.

I would like to see us admit to the experiment families headed by a female with some prescribed amount of work experience in the recent past. We already have half a dozen or so of these families in the sample by virtue of
their having a son 18 or over in the household. Extending eligibility in this way now will allow us to cut down on our needle in a haystack search and will yield us the type of family which over three years will come more and more to dominate the national poverty population.

However, the group may wish to reaffirm its original focus on intact families, even in the face of their considerable uniqueness. I do not doubt that, with an appropriately intensive sampling scheme, we can find enough families in northern New Jersey to fill our quota using present definitions. Our Trenton experience probably overstates the difficulty — Trenton has the tightest labor market of the six potential sites and we screened there during the high-employment summer months.

Furthermore, there can be no question that these families, though predominately "near poor," are living in some degree of deprivation. Since these families exist and have a genuine financial need, it seems legitimate for the experiment to focus on them if it wants to. I would prefer that we alter our eligibility requirements to speak more directly to the problem of poverty, but I grant that standing by our original definitions will not hopelessly damage the study.

II. On Payments

Whether or not we broaden our screening criteria, it is imperative that we liberalize our payments structure. We now have a mismatch between payments and recipients, with predominately lower-middle class families getting what amount to small wage supplements. This has led to payments 40% below those anticipated, a crush of $2.50 checks, and pronounced self-selection among higher income families.

With the coming of AFDC-UP in January, we will be in danger of losing part of our limited poverty cohort to welfare — leaving us a very small, biased sample of poor to add to our larger biased sample of non-poor. What if the bulk of this sample should show no detectable work incentives or disincentives either among experimental groups or between experimental and control group?
Given the present income distribution of these families relative to our present payments structure, this seems a likely outcome. We will have shown that stable working class families do not opt for (below) minimum subsistence guarantees or worse, neither a surprising nor a particularly valuable ($4 million) result. We will no doubt turn up some interesting findings about retirement, overtime, secondary workers, etc., but I doubt that we will have spoken importantly to the issues of income maintenance and poverty. Our carefully structured set of tax variants will have proved to be largely outside the relevant range.

We can take action to avoid this situation by increasing our payments levels to:

1. come near our original budget estimates for direct payments (a modest $1500/family/year),
2. reduce our spiraling ratio of operating to direct costs,
3. limit our potential problems with AFDC-UP,
4. bring us at least abreast of current practice in welfare, and
5. present the majority of our sample families with something close to an economic choice.

In my memo of Sept. 4, I proposed that we raise our guarantee levels 15% across the board. This seems to me to be a minimal response — it would bring our 100% guarantee up to approximately the current AFDC standard in New Jersey.

Another possibility would be to add one or more variants at 125% of our present poverty standard. For a family of four this would mean a guarantee of $4125 — slightly less than the total take-home income at which New Jersey welfare now begins to apply its 100% tax. Adding such a variant at a 50% tax rate would keep our maximum break-even level just where it is now — at 2 1/2 times the poverty line.

We are almost certainly going to have to adjust our payments over the next three years if we are to avoid being undercut by the passage of time alone. Since the need for adjustment is clear right now, I urge that we take immediate steps to update our policy space.
III. A Final Point

At a recent Brookings Institution meeting on poverty research, great interest was expressed in longitudinal studies of families moving up the income scale, in order to test the hypothesis that such families soon take on characteristics associated in cross section studies with the income strata into which they move. This goes to the root of the debate over poverty as an economic phenomenon versus poverty as a social/cultural phenomenon. We will have virtually nothing to contribute here, since we are not significantly raising family income in most cases.

This further limits us by denying us a chance to look at the possible incentive effects of large additions to income. Our sociologists are geared up to investigate the changes in attitudes, aspirations, methods of achieving goals, etc. which may be expected to accompany such additions, but it is doubtful that they will get the chance. For many families the most significant sociological/psychological aspects of the experiment will likely be the sense of uniqueness and the repeated interviews. As an economist, I would be especially interested in possible threshold effects of sizeable injections of money, but this sort of analysis is largely closed to us now.

Finally, the Brookings meeting turned up considerable interest in income adequacy as an experimental parameter in income maintenance research. Some good results here could end the present era of carping about SSA poverty levels while using them for lack of anything better. At the present rate, we will almost surely have nothing to contribute on this topic.

The work incentive implications of any income maintenance program can be investigated, providing anyone cares to. But analysis of these other important questions requires substantial levels of payments. By increasing our benefits or adding new, generous variants to our present set up, we can address these questions while bolstering our work incentive analysis by extending our policy space to include rate/level combinations which are more relevant to future income maintenance decisions and to the population we have chosen to sample. We can easily make the required adjustments within the scope of our present budget. Our failure to do so will be a needless and damaging mistake.
NOTES:

There was general agreement about the need to confine our sampling in other cities to predominately low-income tracts. Our basic procedure in those cities was to begin with official Census poverty tracts and to add by on-site inspection any contiguous areas which appeared to have deteriorated to poverty levels. But on the more basic issues of eligibility and the policy space, we entered into a period of extended discussion, during which the following memos were written.
TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Reassessing our Eligibility Requirements
DATE: October 21, 1968

I. Final Trenton Tally

Our sample in Trenton is finally closed. Even after abandoning stratification and accepting into the sample all families which met our basic eligibility criteria, we ended up with a final contingent of only 4.3% of all families contacted. (See Table 1 in Appendix for detailed Trenton results).

This differs greatly from the expected eligibility proportion, on the basis of which our selection criteria, our target tracts, and our sampling ratio were determined. Last spring, I estimated from Census data that approximately 10% of all households in our 15-tract Trenton site would have been eligible for our program and poor in 1959. (See Table 2 in Appendix). Special tabulations of the Current Population Survey prepared for the Social Security Administration indicated that such families were broadly representative of about 40% of all poor families nationally with demonstrated work capability in 1964.

The Trenton results suggest that about 2.5% of administered interviews were, in fact, conducted with eligible poor families. This estimate of the proportion of such families in the sampled population is high or low depending on whether entries such as refused, not at home, received late, etc., are under- or overweighted with eligible poor.

What we cannot determine is how representative our present sample is of poor or near-poor families in Trenton target tracts or what impact certain changes in our screening criteria would have on that representativeness and on our eligibility yield. This is a consequence of our terminating interviews with

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ineligible families at any one of several points before income questions are asked. The procedure is a reasonable one -- it minimizes respondent hostility by leaving sensitive income questions to the end, and it maximizes efficiency by stopping interviews as soon as it is apparent that the respondent's family is ineligible. It becomes unreasonable only when our eligibility criteria, around which the questionnaire terminations are structured, are called into doubt. Our Trenton short fall has certainly raised such doubt, and some reassessment is in order before we carry over present procedures to Paterson-Passaic, where we face a task over four times as large as the Trenton pilot.

II. Objectives

There seems to be a consensus that our Trenton outcome, in terms for percent eligible, is acceptable and that a repetition of it in other cities will not be grounds for altering our criteria. But the percentage is small enough, and our uncertainty about the "typicalness" of Trenton trends since 1960 large enough, that precautions must be taken. We need to know as quickly as possible the likely proportion of eligibles in Paterson/Passaic using present criteria. This will reassure us and permit an early decision on the optimal sampling ratio to achieve our desired quota(s) of families.

There is always the chance that we will not be reassured. There is some result which we would not be able to accept and from which we would have to take immediate steps to recover. Thus, while estimating the eligibility proportion, we must also be gathering data which will guide us on just what eligibility changes, if any, are appropriate.

III. Method

We have instructed our field staff in Paterson/Passaic to begin the screening process by conducting approximately 500 non-termination interviews with randomly selected families in our target area. As noted in William Branson's memo of September 30, 1968, estimating that the true percentage of eligible families is 11.1 (the actual figure for Trenton), and assuming that the
proportion of eligibles x/n for samples of size n is normally distributed, we find that n = 500 will allow us 95% certainty that the true proportion of eligibles in the population lies within plus or minus three percentage points of the proportion calculated from the sample. This is a manageable number of preliminary interviews and provides an adequate degree of precision, at least for a first pass.

We have set a tentative target of 15,000 total screening interviews in Paterson/Passaic. This is really an upper bound which makes generous allowance for a number of contingencies. If our Trenton experience is repeated, we will get from this 15,000 approximately 1,665 eligible families after screening, and 1020 eligible families after pre-enrollment. This will give us considerable leeway to stratify families by income and raise our present low percentage of poor families enrolled.

I propose that we set the minimum acceptable ratio of screening eligibility at that percentage which, if all 15,000 interviews are administered, will just fill our planned quota of 550 enrolled families, without regard to stratification.

From the following calculation:

\[
\frac{550 \text{ enrollment slots}}{15,000 \text{ screening interviews}} = 3.7\% \text{ minimum final eligibility rate.}
\]

\[
\frac{6.2\% \text{ final eligibility rate (}}\%\text{ of administered interviews actually enrolled}}{11.1\% \text{ Trenton eligibility rate after screening}} = 55.8\% \text{ eligibility loss between screening and enrollment.}
\]

---

2 All Trenton figures relate to the 10 official poverty tracts, not to the entire 15 tract target area. This seems to be the most relevant basis for comparison with Paterson/Passaic where we are entering only poverty tracts.
we see that an eligibility ratio at screening of less than 6.6% will likely produce another enrollment short fall (which we would be very hard pressed to make up in a third city). Using our non-termination sample of 500, we can be 97.5% certain that the true eligibility ratio does not lie below 6.6% if the actual ratio for the sample is 9.6%.

I urge that we agree to broaden our eligibility requirements if the eligibility ratio on non-termination interviews falls below 9.6%, the exact new definition of eligibility to be determined on the basis of information gotten from the interviews themselves.

I find this a very minimal bound. It again, discards stratification and it disregards arguments about representativeness which I think are quite important. Instead it relies solely on practicality — how rare can subjects be and still be found in large enough numbers to conduct the experiment without going over cities with a fine tooth comb.

IV. Implementation

(1) There are 600 segments (blocks) in our Paterson/Passaic target area (21 poverty tracts) according to the 1960 census. A decision has been

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3 It is true that, seeing our shortfall in Trenton, we were fairly lenient in our determination of eligibility at screening. Thus, the eligibility loss between screening and enrollment is probably on the high side in Trenton. Also, we have had some worries about the field organizations diligence at follow-up, etc. (one of the reasons we elected to take on the phenomenal survey burden in P/P). If we can improve on their record this will further cut our losses between eligibility and enrollment. However, these items are uncertain enough that I would prefer to see them allowed as safety factors rather than explicitly entered in the calculation.
reached to sample the area by selecting entire segments at random and screening them 100%. This allows more efficient use of listing and interviewing personnel, and makes survey quality control much simpler — an important factor where local people are being used to interview local people for a potentially beneficial program. (Which they don't yet know about but may stumble onto at any time). Given our low eligibility yield, potential interaction problems from such clustering seem small.

(2) Applying the "Trenton factor" to 1960 Census data for Paterson/Passaic indicates that slightly over half of the 600 segments will have to be screened to obtain 15,000 interviews. (See Tables 3 and 4 in the Appendix). To begin the task, approximately one-quarter of the segments (148) were selected using Rand tables. It is expected that this first pass will not suffice and that some proportion (1/3) of the remaining segments will have to be screened in a later pass.

(3) Of the 148 segments first designated for interviewing, approximately half (72) were selected at random as sites for non-termination interviews. 1960 Census data (modified for Trenton) suggest that such interviews must be attempted at one out of every four dwelling units in these segments in order to obtain 500 completed ones. Given the wish to move on quickly with the complete screening operation, the likelihood that a second screening pass will be necessary to obtain enough regular interviews, and the unlikelihood that the eligibility yield will be so low as to prompt a change in criteria, we have lowered the rate for non-termination interviews to one in six, which will greatly speed our work and should yield 300 – 400 completed interviews of this sort. If these interviews should show an eligibility ratio hovering in some grey area around the lower bound, we will necessarily have to go into a second pass and can schedule more non-termination interviews in the new segments selected.

(4) Dwelling units for non-completion interviews were designated by placing red asterisks on randomly selected lines on listing sheets which field personnel will fill in showing all dwelling units in each target segment. After listing all dwelling units in his segment, the interviewer will go back and
interview first at all dwelling units which fall on lines having asterisks. He
will complete all these interviews regardless of termination instructions printed
on the questionnaire. Then he will conduct regular interviews at the other dwel-
ing units in his segment. On each listing sheet, the first asterisk has an equal
chance of falling on one of the first six lines; thereafter, asterisks appear on
approximately every 6th line.

V. Result

Interviewing began on Monday, October 14, 1968, so that by the end of
this week we would have results from the non-termination interviews in and
analyzed. It should be noted that these data will be very helpful in evaluating
the nature of sampled population, and therefore, the nature of the sample,
quite apart from whether they prompt us to change our eligibility requirements.
Table I
Trenton Sample

<table>
<thead>
<tr>
<th></th>
<th>Poverty Tracts</th>
<th>Non-Poverty Tracts</th>
<th>Totals</th>
</tr>
</thead>
<tbody>
<tr>
<td>I. Housing Units Expected by 1960 Census</td>
<td>3162</td>
<td>1037</td>
<td>4199</td>
</tr>
<tr>
<td>II. Housing Units Estimated by ORC</td>
<td>2570</td>
<td>930</td>
<td>3530</td>
</tr>
<tr>
<td>III. Screening Interviewers not Administered</td>
<td>793</td>
<td>277</td>
<td>1070</td>
</tr>
<tr>
<td>Vacant</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Refused</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not at Home</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Other</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV. Screening Interviews Administered</td>
<td>1777</td>
<td>683</td>
<td>2460</td>
</tr>
<tr>
<td>V. Screening Interviews Terminated</td>
<td>1082</td>
<td>486</td>
<td>1578</td>
</tr>
<tr>
<td>No Male</td>
<td>821</td>
<td>308</td>
<td>1129</td>
</tr>
<tr>
<td>Income too High</td>
<td>224</td>
<td>143</td>
<td>367</td>
</tr>
<tr>
<td>Other</td>
<td>47</td>
<td>35</td>
<td>82</td>
</tr>
<tr>
<td>VI. Screening Interviews Completed</td>
<td>685</td>
<td>197</td>
<td>882</td>
</tr>
<tr>
<td>Ineligible for Pre-enrollment</td>
<td>488</td>
<td>162</td>
<td>650</td>
</tr>
<tr>
<td>Eligible for Pre-enrollment</td>
<td>197</td>
<td>35</td>
<td>232</td>
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<tr>
<td>Refused pre-enrollment</td>
<td></td>
<td></td>
<td>15</td>
</tr>
<tr>
<td>Not at Home or Vacant</td>
<td></td>
<td></td>
<td>10</td>
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<tr>
<td>Family Broken Up</td>
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<td>Head in Jail</td>
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<td>Ineligible for Enrollment</td>
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<td>Received too late for pre-enrollment</td>
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<td>Eligible for Enrollment</td>
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<td></td>
<td>137</td>
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<td>Refused Enrollment</td>
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<td>2</td>
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<td>Experimental group</td>
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<td>8</td>
<td>87</td>
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<tr>
<td>Control Group</td>
<td>32</td>
<td>7</td>
<td>39</td>
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<td>Moved</td>
<td>2</td>
<td>0</td>
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</tr>
<tr>
<td>Tract number</td>
<td>Racial pattern</td>
<td>Total households</td>
<td>Married couple households</td>
</tr>
<tr>
<td>--------------</td>
<td>----------------</td>
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<td>--------------------------</td>
</tr>
<tr>
<td>1</td>
<td>W</td>
<td>925</td>
<td>634</td>
</tr>
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<td>2</td>
<td>W</td>
<td>1219</td>
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<td>W</td>
<td>1479</td>
<td>1125</td>
</tr>
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<td>4</td>
<td>W</td>
<td>1755</td>
<td>1209</td>
</tr>
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<td>5</td>
<td>W</td>
<td>1351</td>
<td>993</td>
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<td>6</td>
<td>W</td>
<td>1463</td>
<td>1083</td>
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<td>7</td>
<td>W</td>
<td>987</td>
<td>711</td>
</tr>
<tr>
<td>8</td>
<td>W*</td>
<td>898</td>
<td>597</td>
</tr>
<tr>
<td>9</td>
<td>W*</td>
<td>2427</td>
<td>1098</td>
</tr>
<tr>
<td>10</td>
<td>W*</td>
<td>1718</td>
<td>1027</td>
</tr>
<tr>
<td>11</td>
<td>W*</td>
<td>2425</td>
<td>1233</td>
</tr>
<tr>
<td>12</td>
<td>W</td>
<td>1532</td>
<td>1033</td>
</tr>
<tr>
<td>13</td>
<td>W</td>
<td>1158</td>
<td>853</td>
</tr>
<tr>
<td>14</td>
<td>W*</td>
<td>2529</td>
<td>1776</td>
</tr>
<tr>
<td>15</td>
<td>N</td>
<td>1273</td>
<td>783</td>
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<tr>
<td>16A</td>
<td>W</td>
<td>1303</td>
<td>464</td>
</tr>
<tr>
<td>16B</td>
<td>N*</td>
<td>620</td>
<td>354</td>
</tr>
<tr>
<td>17</td>
<td>W*</td>
<td>1864</td>
<td>1242</td>
</tr>
<tr>
<td>18</td>
<td>W</td>
<td>1266</td>
<td>860</td>
</tr>
<tr>
<td>19</td>
<td>W</td>
<td>723</td>
<td>488</td>
</tr>
<tr>
<td>20</td>
<td>N</td>
<td>753</td>
<td>439</td>
</tr>
<tr>
<td>21</td>
<td>W*</td>
<td>1928</td>
<td>1244</td>
</tr>
<tr>
<td>22</td>
<td>W</td>
<td>1998</td>
<td>1440</td>
</tr>
</tbody>
</table>

W — White  
N — Non-white  
* — Significantly mixed
Table 3
Poverty Tracts – 1960 Census Paterson

<table>
<thead>
<tr>
<th></th>
<th>PT 4</th>
<th>PT 5</th>
<th>PT 7</th>
<th>PT 8</th>
<th>PT 9</th>
<th>PT 14</th>
<th>PT 15</th>
<th>PT 16A</th>
<th>PT 16B</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. All housing units</td>
<td>839</td>
<td>1306</td>
<td>1252</td>
<td>1150</td>
<td>1173</td>
<td>1445</td>
<td>1820</td>
<td>679</td>
<td>847</td>
</tr>
<tr>
<td>2. All occupied housing</td>
<td>662</td>
<td>772</td>
<td>1204</td>
<td>1137</td>
<td>1158</td>
<td>1410</td>
<td>1743</td>
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<td>847</td>
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<td>units</td>
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<td></td>
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<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Total segments</td>
<td>17</td>
<td>15</td>
<td>34</td>
<td>43</td>
<td>29</td>
<td>16</td>
<td>24</td>
<td>31</td>
<td>17</td>
</tr>
<tr>
<td>4. No. of target segments/</td>
<td>7</td>
<td>3</td>
<td>7</td>
<td>12</td>
<td>5</td>
<td>4</td>
<td>5</td>
<td>7</td>
<td>4</td>
</tr>
<tr>
<td>1st pass</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. No. of target segments/</td>
<td>3</td>
<td>1</td>
<td>4</td>
<td>7</td>
<td>2</td>
<td>4</td>
<td>1</td>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>non-terminations</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. Non-termination segments</td>
<td>17.6%</td>
<td>6.7%</td>
<td>11.8%</td>
<td>16.7%</td>
<td>6.9%</td>
<td>25.0%</td>
<td>4.2%</td>
<td>16.1%</td>
<td>17.6%</td>
</tr>
<tr>
<td>as % of total (5 ÷ 3)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>7. Total interview</td>
<td>377</td>
<td>440</td>
<td>686</td>
<td>648</td>
<td>660</td>
<td>804</td>
<td>994</td>
<td>370</td>
<td>465</td>
</tr>
<tr>
<td>opportunities expected*</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2 x Trenton factor,57%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8. Interview opportunities</td>
<td>66</td>
<td>29</td>
<td>81</td>
<td>108</td>
<td>46</td>
<td>201</td>
<td>42</td>
<td>60</td>
<td>82</td>
</tr>
<tr>
<td>expected in non-termination</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>segments (7 x 6)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
</tbody>
</table>

* This includes a safety factor of five percent as a result of applying our Trenton result to all occupied housing units rather than all housing units which was the basis for Trenton calculations.
Table 3 (Cont.)

Poverty Tracts – 1960 Census Paterson

<table>
<thead>
<tr>
<th></th>
<th>PT 17A</th>
<th>PT 17B</th>
<th>PT 18</th>
<th>PT 20</th>
<th>PT 22</th>
<th>PT 23</th>
<th>PT 28</th>
<th>PT 29</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. All housing units</td>
<td>640</td>
<td>1400</td>
<td>1151</td>
<td>1308</td>
<td>1286</td>
<td>2454</td>
<td>1123</td>
<td>860</td>
</tr>
<tr>
<td>2. All occupied housing units</td>
<td>588</td>
<td>1364</td>
<td>1075</td>
<td>1260</td>
<td>1252</td>
<td>2340</td>
<td>1095</td>
<td>845</td>
</tr>
<tr>
<td>3. Total segments</td>
<td>24</td>
<td>21</td>
<td>21</td>
<td>15</td>
<td>31</td>
<td>37</td>
<td>25</td>
<td>22</td>
</tr>
<tr>
<td>4. No. of target segments/1st pass</td>
<td>4</td>
<td>8</td>
<td>4</td>
<td>3</td>
<td>11</td>
<td>8</td>
<td>7</td>
<td>3</td>
</tr>
<tr>
<td>5. No. of target segments/non-terminations</td>
<td>2</td>
<td>3</td>
<td>3</td>
<td>2</td>
<td>5</td>
<td>2</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>6. Non-termination segments as % of total (5 ÷ 3)</td>
<td>8.3%</td>
<td>14.3%</td>
<td>14.3%</td>
<td>13.3%</td>
<td>16.1%</td>
<td>5.4%</td>
<td>8.0%</td>
<td>9.1%</td>
</tr>
<tr>
<td>7. Total interview opportunities expected* (2 × Trenton factor, 57%)</td>
<td>335</td>
<td>777</td>
<td>613</td>
<td>718</td>
<td>714</td>
<td>1334</td>
<td>624</td>
<td>482</td>
</tr>
<tr>
<td>8. Interview opportunities expected in non-termination segments (7 × 6)</td>
<td>28</td>
<td>111</td>
<td>88</td>
<td>95</td>
<td>115</td>
<td>72</td>
<td>50</td>
<td>44</td>
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* This includes a safety factor of five percent as a result of applying our Trenton result to all occupied housing units rather than all housing units which was the basis for Trenton calculations.
Table 4
Poverty Tracts — 1960 Census Passaic

<table>
<thead>
<tr>
<th></th>
<th>PA 52</th>
<th>PA 53</th>
<th>PA 54</th>
<th>PA 55</th>
<th>PA 59</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. All housing units</td>
<td>1343</td>
<td>2783</td>
<td>2412</td>
<td>1728</td>
<td>1997</td>
</tr>
<tr>
<td>2. All occupied housing units</td>
<td>1249</td>
<td>2678</td>
<td>2360</td>
<td>1690</td>
<td>1897</td>
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<tr>
<td>3. Total segments</td>
<td>19</td>
<td>28</td>
<td>53</td>
<td>40</td>
<td>30</td>
</tr>
<tr>
<td>4. No. of target segments/1st pass</td>
<td>3</td>
<td>10</td>
<td>15</td>
<td>10</td>
<td>7</td>
</tr>
<tr>
<td>5. No. of target segments/non-terminations</td>
<td>1</td>
<td>5</td>
<td>7</td>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>6. Non-termination segments as % of total (5 ÷ 3)</td>
<td>5.3%</td>
<td>17.8%</td>
<td>13.2%</td>
<td>12.5%</td>
<td>10.0%</td>
</tr>
<tr>
<td>7. Total interview opportunities expected* (2 × Trenton factor, 57%)</td>
<td>712</td>
<td>1526</td>
<td>1345</td>
<td>963</td>
<td>1081</td>
</tr>
<tr>
<td>8. Interview opportunities expected in non-termination segments (7 × 6)</td>
<td>38</td>
<td>272</td>
<td>178</td>
<td>120</td>
<td>108</td>
</tr>
</tbody>
</table>

Interview opportunities in non-termination segments

| Paterson | 716 |
| Passaic  | 1318/2034 | \(\frac{500}{2034} \approx \frac{1}{4}\) |

*This includes a safety factor of five percent as a result of applying our Trenton result to all occupied housing units rather than all housing units which was the basis for Trenton calculations.
TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Broadening the Range of Tax Parameter Variation
DATE: October 30, 1968

I. Proposal

Add two new tax variants to the policy space:

<table>
<thead>
<tr>
<th>Guarantee Level</th>
<th>Tax Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Plan K</td>
<td>125% Poverty</td>
</tr>
<tr>
<td>Plan L</td>
<td>125% Poverty</td>
</tr>
</tbody>
</table>

II. Objectives

(1) Insure that the policy space does not become substantially outdated by the end of the experiment.

A special federal task force has already been convened to reconsider current social security poverty lines. The BLS will soon publish a new low-income standard for city workers which is expected to be in the vicinity of $5000 for a family of four.

(2) Allow the negative tax program to compete somewhat more successfully with existing and projected welfare programs in New Jersey.

Our effective losses to welfare are already significant (14%) and may be expected to increase over time.

(3) Enlarge the range of experimental variation to include tax treatments which may be expected to entail some clearly observable disincentive effects.

Most of our tax plans are too modest to be of real economic interest to the families in our sample, 70% of whom are not poor. We need more generous plans to discern clear-cut variation in disincentive response.
III. Money Costs

Trenton Experimental Sample (October 22, 1968)

Average Annual Family Income $4700
Average Family Size 6
Average Annual Negative Tax Payment 960
Estimated Average Annual Payment Under Plan K 2712
Estimated Average Annual Payment Under Plan L 1772

Assuming nine tax treatments (seven current treatments plus two proposed additions) with ninety families enrolled in each:

Estimated Average Annual Negative Tax Payment $1245
Estimated Increase in Direct Payment Costs over Present Arrangement [(1245 - 960)/960] 30%
Estimated Three Year Direct Payment Cost $3,025,080

IV. Experimental Costs

To get an idea of the statistical costs involved in fitting more tax variants into approximately the present sample size, I have gone back to Glen Cain's estimates which first reassured us on samples of size 1,000. The table below follows directly from Table 3 in his draft paper of 6/6/67.

Sizes of Coefficients ($\beta$) of a Treatment Variable as a Regressor on Changes in Earned Income, for $r = .063$, sample size = 1000, and different values of $\sigma_u$ and $\sigma_x$.

<table>
<thead>
<tr>
<th>Number in Treatment Group = $n_i$</th>
<th>Variation in (Dummy) Treatment Variable $X$ not explained by other Independent Variables = $\sigma_{x'}$</th>
<th>Variance of Underlying Year-to-Year Change in Income = $\sigma_u$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$250$</td>
</tr>
<tr>
<td>70</td>
<td>.2422</td>
<td>$65$</td>
</tr>
<tr>
<td>80</td>
<td>.2575</td>
<td>$61$</td>
</tr>
<tr>
<td>90</td>
<td>.2716</td>
<td>$58$</td>
</tr>
<tr>
<td>100</td>
<td>.2848</td>
<td>$56$</td>
</tr>
<tr>
<td>110</td>
<td>.2970</td>
<td>$53$</td>
</tr>
<tr>
<td>120</td>
<td>.3084</td>
<td>$51$</td>
</tr>
</tbody>
</table>
FORMULA:  \[ \beta = \frac{\sigma_u}{\sigma_x} \sqrt{\frac{r^2}{1 - r^2}} \]

NOTE: The combination of \( r = .063 \) and \( N = 1000 \) (with assumed degrees of freedom = 980) is such that the regression coefficients in the cells of the table are just significant (i.e., a t-ratio of 2).

Under our present arrangement, purely random assignment of families to tax plans will result in approximately 114 families enrolled in each plan. The preceding table suggests that if we were to cut representation in (some) tax plans as low as 70, the experimentally induced change in income for those plans which we would just be able to distinguish with 95% confidence would rise about $50, if the underlying variation in family income from year-to-year were as high as $1000 (unlikely for our present sample). If the underlying income variation were $500, the distinguishable change would rise only $25 above its present level.

V. Adjustments

As of October 22, the following situation prevailed in Trenton:

<table>
<thead>
<tr>
<th>Plan</th>
<th>Number of Families</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Enrolled</td>
</tr>
<tr>
<td>B (50% Guarantee, 50% Tax)</td>
<td>14</td>
</tr>
<tr>
<td>E (75% Guarantee, 70% Tax)</td>
<td>9</td>
</tr>
</tbody>
</table>

It appears that the one Plan B family which is not receiving welfare and not now receiving $2.50 will get $2.50 in the next pay period as our moving average catches up with them. Our income estimate from screening data clearly understated their present earning power, and they will soon be well above their break-even point. Thus, we will have no experimental observa-
tions whatsoever on work response of families eligible to receive negative taxes under Plan B.

For Plan E we have only two such observations. One of our $2.50 families in Plan E has already indicated that he will not be continuing in the program. We do not, I think, need several hundred families to indicate that these two tax plans are of only the most marginal interest or help to the type of people we have in our sample.

These Trenton figures plus the foregoing Cain table suggest to me that we ought to shift from equal cell sizes and weight more heavily tax plans some substantial range of individual and/or family work response may be expected. I propose that we enroll 100 families in each of seven tax plans (including Plans K and L, but not B and E) and no more than 75 each in Plans B and E. This will give us a total experimental group of 850 families. If some cut is necessary in this total, I suggest that it be made in Plans B and E.

VI. Summary Tables
(1) Plan K (125% Guarantee, 50% Tax)

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Guarantee</th>
<th>Break-even Point*</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>$2500</td>
<td>$5,000</td>
</tr>
<tr>
<td>3</td>
<td>3438</td>
<td>6,876</td>
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<tr>
<td>4</td>
<td>4125</td>
<td>8,250</td>
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<tr>
<td>5</td>
<td>4625</td>
<td>9,250</td>
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<tr>
<td>6</td>
<td>5062</td>
<td>10,124</td>
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<tr>
<td>7</td>
<td>5438</td>
<td>10,876</td>
</tr>
<tr>
<td>8+</td>
<td>5750</td>
<td>11,500</td>
</tr>
</tbody>
</table>

* These break-even points exactly equal the highest break-even points currently in use.
(2) Plan L (125% Guarantee, 70% Tax)

<table>
<thead>
<tr>
<th>Family Size</th>
<th>Guarantee</th>
<th>Break-even Point</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>$2500</td>
<td>$3571</td>
</tr>
<tr>
<td>3</td>
<td>3438</td>
<td>4911</td>
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<tr>
<td>5</td>
<td>4625</td>
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<td>6</td>
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<td>7</td>
<td>5438</td>
<td>7768</td>
</tr>
<tr>
<td>8+</td>
<td>5750</td>
<td>8214</td>
</tr>
</tbody>
</table>

(3) New Policy Space (entries are numbers of enrolled families)

<table>
<thead>
<tr>
<th>Guarantee Level</th>
<th>Tax Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>30%</td>
</tr>
<tr>
<td>125% P</td>
<td></td>
</tr>
<tr>
<td>100% P</td>
<td></td>
</tr>
<tr>
<td>75% P</td>
<td>100</td>
</tr>
<tr>
<td>50% P</td>
<td>100</td>
</tr>
</tbody>
</table>

NOTES:

There was substantial acceptance of the reasoning in the eligibility criteria memorandum at the time it was written. However, the additional task of non-termination interviews proved too much for our new interviewing staff, with both them and us performing unfamiliar duties for the first time. An early field check indicated that many of the non-termination families had been improperly interviewed, these being the very first interviews administered. Although the project was carried on to completion, the results were of little use.

The main body of the screening operation was carried out in two phases using the target quota of 15,000 households. The eligibility rate at screening turned out to be 8.9%, somewhat above the minimum acceptable rate of 6.6%. But the rate of loss through non-contact (moved, not at home after four callbacks), refusals (both to be interviewed and to enroll), and in-
eligibility (at time of interviews or enrollment) was high enough that the final sample ended up with only 352 families, for an overall eligibility rate of 3.4%. As in Trenton, every family found eligible was admitted to the sample.

Assuming that we adhered to our original estimate of 1200 initial sample families, this meant that we would now have to find 692 eligible families in Jersey City, a virtually impossible task using our existing criteria. However, there seemed to be little interest in reopening the eligibility discussion, which had lain dormant for many months. It was at this point that mention of a fourth city began to be heard, both because of the sheer numbers problem and because of the particular racial composition, existing and projected, of the three city sample. Inability to stratify in Paterson/Passaic had produced a sample in those cities approaching 50% Spanish, and the prospect in Jersey City was for an even higher proportion. Up to that point, only 59 white families had been enrolled, and again Jersey City did not hold out much hope of achieving a more equal racial balance. The decision that we would definitely enter a fourth city, and that that city would be Scranton, Pennsylvania, was made as we prepared to survey Jersey City. The eligibility criteria were never again challenged, or at least not from within the project.

On the matter of the new tax variants, a decision was reached in early November, 1968, to add the two proposed 125% plans to the policy space. The principal objective was to widen the range of treatment variation in order to cover a wider range of potential disincentive outcomes. Permission to introduce the plans in cities other than Trenton was obtained from OEO, and the first families were enrolled in Paterson and Passaic during February 1969. However, the unsophisticated allocation of families over treatments implied in the memo, although it was used in Trenton, was soon to be rejected in favor of a more elaborate design model which will be discussed in the next chapter.

Finally, we agreed in July 1969 to incorporate a cost of living adjustment in the tax plans in order to maintain the value of their benefits in real
terms. Each August 1st, the anniversary of the beginning of payments in Trenton, all negative tax guarantees are raised by the percentage rise in the Consumer Price Index since the previous August. The first year's rise amounted to 5.5%.
2. Coexistence with Welfare

As has been suggested previously, one of the reasons for choosing to operate in New Jersey was the state's lack of an AFDC-UP program which would pay benefits to our target population. But even before negative tax payments began, the conservative State legislature unexpectedly enacted a very liberally worded UP (unemployed parent) law to take effect on January 1, 1969. This prompted an immediate interest on our part, a subsequent consideration of alternative responses we might make, and a final decision to change our treatment of welfare benefits. The following memoranda, the first one written shortly after the enactment of UP, indicate this sequence.
I. The New AFDC-UP Program

New Jersey will add an Unemployed Parent segment to its Aid to Families with Dependent Children program on January 1, 1969. The following summarizes the expectations of various officials and agencies in the state welfare structure regarding the new program.

• State

The New Jersey State Director of Public Welfare, Irving Engleman, has indicated that the new UP law will be fully effective on January 1. Standards for the UP program will be the same as those presently used in AFDC, but a new budgeting procedure will be inaugurated (apparently in both AFDC and AFDC-UP) whereby the first $100 a month of earned income and 1/3 of all additional earned income per month will be disregarded in determining payments. For a family of size four, this will mean a maximum cash payment of approximately $3,800 per year if they have no other income, and a breakeven level of up to $6,900 per year. There is additional coverage for medical expenses.

Engleman estimates that 19,000 UP cases will be opened in New Jersey in 1969. Apportioning these cases crudely according to the present statewide distribution of the welfare caseload suggests that approximately 400 of these new cases will be opened in Trenton, 4,000 in Newark, 1,400 in Jersey City-Hoboken, and 700 in Paterson/Passaic.

• Mercer County Welfare Board

Mr. Raymond Dougherty, Director of the MCWB, and his assistant, Mr. Kutalek, expect 400 - 500 UP openings in Mercer County in 1969. As the
city of Trenton presently accounts for 83% of the county (categorical assistance) program, this suggests that perhaps 375 UP openings in 1969 will take place in Trenton.

The bulk of these cases are expected to come on the rolls on January 1, as a result of reclassification from municipal (general assistance) caseloads. Engleman's office has advised the county board that its monthly caseload in 1969 will increase by an average of 275 cases due to transfers into UP from municipal (notably Trenton) programs.

The county board says it presently adheres to the full state standards, but has heard nothing about the proposed budgeting procedure mentioned by Engleman. This is not unusual, apparently. Local boards learn of changes in operating rules when they receive notices from the state to implement them, sometimes even retroactively. Right now, the county allows all direct expenses of employment plus a "personal employment deduction" (i.e., pocket money) of $40/month to be subtracted from earned income before applying its 100% tax. The new UP law says that "there shall be disregarded the amounts of income and resources required by Federal law as a condition of Federal financial participation." It seems clear that New Jersey will have the new Federal budgeting provisions by January 1, 1969. It may or may not have the more generous Engleman treatment.

- Trenton Public Welfare

Mrs. Jane Browne, Director of Welfare in Trenton, indicates that a "substantial" majority of her department's present total caseload of 414 general assistance cases is awaiting determination of eligibility for county programs — some for disability assistance but mostly for AFDC. The county will not take on cases until a thorough "financial and social investigation" has proven them eligible for one of the categories. Most of the city's program is emergency assistance to families undergoing this investigation process, which is supposed to take 30 days for AFDC and 60 days for DA, but actually takes much longer. When the new "presumptive eligibility" clause in the categorical
programs goes into effect on January 1, 1969, these "pending" cases will by and large disappear from the city rolls.

It is clear that January 1, 1969, will see the transfer of the large majority of city cases to the county, but city statistics (available only by word of month) suggest that few of these will go into UP. A city survey in May turned up only twelve employable males receiving general assistance.

Those transferred from city to county are likely to receive more generous benefits thereby although the city maintains that it adheres to the same state standards and budgeting provisions as does the county. Few people in Trenton believe this latter contention, and the clients definitely do not.

II. Joint Eligibility for Welfare and GWIE

Families with low incomes and whose entire membership is included in the budget unit for welfare purposes, will find welfare a more remunerative program than any of our tax treatments. Our most generous treatment at low-income levels; the 100% guarantee/50% tax combination, pays less to families of four than welfare (assuming the new federal budgeting provisions) until the family earns over $3400/year. This circumstance may or may not prompt families to go on Welfare, depending on their expectations, tastes, etc.

Families which do go on welfare will have their basic income payments budgeted as unearned income — i.e., subject to 100% tax. The effective marginal tax rate on such families will be set by welfare, either 100% tax on unearned income or first 0% then 66 2/3% on earned income.

The detailed state standards which we received from Engleman last spring are somewhat out of date now, but they suggest that welfare's largest budget unit is a family of size 6. Since our basic payments rise with family size until families exceed size eight, the gap between welfare payments and our payments is narrowed for families larger than 6. This is of some interest given the high proportion of large families we are enrolling in Trenton.

Finally, it should be noted that both the city and county welfare boards can require us to divulge the amount of any payments we make to families
which they are investigating, by court order if necessary. With state permission, the boards can disclose to us the amounts of welfare which they are paying to our families. The state grants such permission when the agency requesting the information is engaged in activities which promote the well-being of the recipient.

III. Employment/Training Opportunities

- Trenton

The city welfare board continuously surveys its caseload to determine the number of employable adults in families it is aiding. The most recent (May) result — 12 men, 52 women. People judged employable must fill out employment seeking forms in order to get or keep payments. They must register with the State Employment Service and must contact at least five employers directly in some prescribed period of time. If they fail to do this or to take a job which is offered to them and which welfare finds appropriate, they, but not necessarily their dependents, are dropped from the rolls. There is a small multi-skill center to which welfare can refer unskilled workers for training.

- Mercer County

The State of New Jersey, under its "Work Incentive Employment and Training Act of 1968" will take part in new training and employment programs for AFDC and AFDC-UP recipients authorized by the 1967 Amendments to Title IV of the Social Security Act. The new state law directs the State Departments of Institutions and Agencies (including Division of Welfare) and Labor and Industry to cooperate in setting up work and training opportunities.

Mercer County will get 200 slots in these new programs beginning October 1 of this year. Three types of work will be available:

- private employment, if worker skilled
- on the job training
- "government sponsored" employment.
These opportunities seem limited in light of the present county AFDC case-load (1,987 cases with several hundred pending) and the projections of UP in 1969. Again, individual members of families can be removed from welfare rolls for refusing to take part in these programs.

IV. Conclusion

The State and local welfare agencies seem to have fairly consistent views on the likely UP caseload in Mercer County in 1969, and there seems to be little evidence to cast doubt on this projection or on the state's overall projection of 19,000 UP cases in 1969. However, the state's expectation that a significant percentage of these cases will come on the UP rolls on January 1 as a result of reclassification from general assistance does not appear to be borne out in Mercer County. More likely, presumptive eligibility plus higher support standards in categorical assistance will attract the anticipated number of UP cases over the year.

No one is willing to predict the average duration of stay on UP, which is known to be relatively short in other jurisdictions (one estimate for New York City shows three months). However, if the Engleman $100 earned income exclusion is coupled with present state standards and a tax rate of 2/3, the break-even levels will be so high that considerable numbers of families could remain on UP indefinitely, even while working to capacity. Families' incomes will have to fall below the state welfare standard before being accepted for UP, but Engleman says that once on there is no time limitation on eligibility. The potential disparities in total income between families that are earning identical amounts, but only some of whom are on welfare, seem large enough to present the state with a real problem — in terms of families artificially lowering their incomes to become initially eligible for welfare (not a legal basis for eligibility) or in terms of public outcry.

The expected number of UP families in Mercer County seems quite small in absolute terms, though not unreasonably small given the potentially eligible population as we have observed it in our Trenton screening. However,
the numbers are quite large relative to our sample size, which now appears likely to reach no more than 150 in Trenton. Of the 74 families in our sample to date, 10, or 14%, received some welfare, apparently general assistance, last year. All these families received welfare for less than a full year last year most of them during periods when the main breadwinner was ill or injured and unable to work. All of them had dependent children and would appear to be potentially eligible for UP (not APTD or Disability Assistance as it is called in New Jersey) in the event their income should fall again in 1969.

To sum up, our chief concern must be with the numbers of families who go on UP. Once on, the payments levels are probably generous enough and the likely length of stay long enough to deny us experimental control over these families' incomes for significant periods of time. This being the case, they might drop out of the sample altogether. This is in contrast to general assistance where standards are less generous and turnover fairly rapid. The UP projections for 1969 suggest that we could live with the problem but might be better advised to take some reciprocal action.

NOTES:

As a result of this and several other attempts by other staff members to anticipate the coming of UP, it seemed evident that the project ought to take some action to limit costly and unrewarding overlaps with welfare. Our Trenton rules had no special provisions for welfare recipients other than the exclusion of welfare benefits from income for negative tax purposes. In October, Harold Watts wrote a memorandum outlining various steps we might take, and shortly thereafter the following memo was written in response.
TO: GWIE Staff
FROM: Heather Ross
SUBJECT: Negative Tax Treatment of Welfare Benefits
DATE: October 29, 1968

I. Introduction

In a recent memorandum, Harold Watts expresses concern that changes in New Jersey's welfare law could prove very costly to us, both financially and experimentally. He notes that our exclusion of welfare benefits from income for negative tax purposes becomes less and less tenable as more and more families are able to establish eligibility for welfare.

He suggests three alternative treatments of welfare benefits:

1. Include benefits in our definition of income and move immediately to the equilibrium level of negative tax payments which would be approached by an infinite series of mutual welfare and negative tax adjustments.

2. Deduct benefits from our negative tax payments.

3. Declare welfare recipients ineligible to receive negative tax payments.

Given our uncertainties about the impact of the new AFDC-UP law, about the new welfare budget standards also being planned for January 1st, and about our own experimental design, it is tempting to adopt a wait-and-see approach. However, waiting for a genuine problem to arise before taking action would be disruptive, risky in terms of maintaining an intact sample, and to some extent deceitful. I agree with Harold Watts that, if we anticipate that some action will eventually be necessary, we should decide upon it now and implement it immediately.

II. Choosing Among Alternative Treatments

Both alternative (1) and (2) above require extensive coordination with welfare in order to work out uniformly and correctly. I believe this degree of coordination is unrealistic and potentially damaging to the image of the
program. In the case of alternative (1), anticipating equilibrium levels in the face of myriad budgeting decisions by individual agencies and caseworkers would be very difficult. (It should be noted that the State sets standards as guidelines, but cannot enforce them in some jurisdictions and does not in others.) Furthermore, since welfare will also have tax rates less than 100% on earned income, we would not be able to set our payments at one level and keep them there, relying on welfare to compensate dollar for dollar for all changes in other family income. Every time family earned income changed we would have to find a new equilibrium.

In the case of the alternative (2), an inevitable outcome would be merely delayed. As long as welfare successfully carried out its investigation of family resources, any family on our program who established and/or maintained eligibility for welfare, would eventually have its welfare benefit eliminate its negative tax payment through a series of growing deductions.

At best, under alternatives (1) and (2), we would simply get no experimental data from families receiving welfare, since welfare would be their controlling treatment. More likely we would introduce a good deal of chaos into the program — confusing families and complicating our own administrative procedure.

Under alternative (3), families would have to choose between welfare and negative income tax. For families choosing welfare, alternative (3) would produce the same net tax-transfer result (i.e., the welfare treatment) as would alternatives (1) and (2) for all families eligible for welfare — but more simply, more cheaply, without involved transition, and without close links to welfare agencies. At most we would lose the same number of families to welfare under alternative (3) as under the other alternatives; more likely some families would prefer our program. It might be argued that observing such a choice would constitute a positive research finding. Certainly this was not our original intent, but it is interesting to note that many people inquiring about the program are surprised to learn that we are not doing this now.
III. Is a Change Necessary?

It seems to me that alternative (3) provides the best solution if some change in our treatment of welfare benefits is necessary. But the repercussions of even this move are severe enough that any change from our present treatment should be carefully weighed. The determining factor is the number of sample families who maybe expected to establish eligibility for welfare over the next three years, and on this topic the Trenton sample can give us some insight.

**Welfare Recipients in Trenton Sample — October 22, 1968**

<table>
<thead>
<tr>
<th>Sex of head</th>
<th>Work situation of head</th>
<th>Number of families</th>
<th>Range of monthly payments</th>
<th>$79 - $247</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male</td>
<td>Not working — health problem unemployed</td>
<td>6</td>
<td>Under $100</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td>Working — low wage and/or large family</td>
<td>2</td>
<td>$100 - $200</td>
<td>6</td>
</tr>
<tr>
<td>Female</td>
<td>Working</td>
<td>2</td>
<td>Over $200</td>
<td>4</td>
</tr>
</tbody>
</table>

In Trenton, 12 of 87 experimental families, or nearly 1 in 7, are presently receiving welfare. Since the new UP law, if administered in any reasonable fashion, can only serve to raise this proportion, I conclude that we must take some action to avoid wholesale loss of negative tax money to people who are made no better off thereby and from whom we get no experimental observations on work response.

Moving to alternative (3) will stem the loss of money, but not necessarily the loss of observations. Again, from the Trenton sample, where all 12 families presently receiving welfare were also receiving welfare when our program began (August 1), it is tempting to conclude that families on welfare at the time of their enrollment who choose welfare over negative taxation
should be dropped from the sample and replaced by other families. I hope this is too hard a conclusion to draw since the resulting self-selection biases would be substantial.

My preferred response to Trenton would be to tighten our definition of eligibility somewhat to screen out certain welfare recipients. For example, it appears that as many as half of the welfare families in our Trenton sample are receiving Disability Assistance related to some health problem of the head. All these family heads have had some recent work experience, but the family may now be expected to remain on welfare indefinitely. We may want to reconsider eligibility for families of this sort.

IV. Three Final Suggestions

(1) That we adopt Harold Watts' method of annual adjustment under alternative (3) — i.e., that we calculate the annual payment which a family would have received if its annual income had equaled 13 times the average four-weekly income which it reported while receiving negative tax payments, that we reduce this annual payment by the fraction of the year that the family was receiving welfare, and that we compare this reduced amount with actual negative tax payments made to the family to determine the amount or any under- or over-payment.

(2) That we make every effort to have families in the sample file income reports during the time they are receiving welfare. To this end, they will continue to receive $2.50 bi-weekly throughout their welfare tenure.

(3) That we not impose any new treatment of welfare benefits on our pilot Trenton sample. To do so would be a breach of the rules of operation under which they enrolled, and would deny us a potentially valuable opportunity to control for our action vis-à-vis welfare benefits in other cities.

NOTES:

At a meeting in Washington with OEO and Wisconsin on November 1, 1968, it was agreed that joint eligibility for welfare and negative tax payments
would be ruled out in all cities other than Trenton. The following procedure was approved for use in Paterson and Passaic, New Jersey, and if satisfactory there, in the final city (later cities).

(1) All families receiving Disability Assistance or Aid to the Blind were to be excluded from the sample, since for most families tenure on these programs was fairly permanent and a strong evidence of inability to participate in the labor market. We would try to rely on families' own statements as to type of welfare to make this determination.

(2) All families receiving AFDC, AFDC-UP, and General Assistance would, if otherwise eligible for the program, be asked to make a choice between welfare and negative income tax at their time of enrollment. All such families who chose negative tax would be enrolled in the usual way. If at any point they wished to switch back to welfare and forfeit negative tax payments, they would be free to do so. While receiving welfare, they would still be considered part of the sample, would receive $2.50 biweekly, and would be asked to submit a regular income report and to be interviewed quarterly. They could shift between welfare and negative tax without limit imposed by us during the three years of the program.

Eligible families who chose welfare over negative taxation would be enrolled as usual, subject to some limit on the proportion of the sample which could be receiving welfare at the beginning of the program. The limiting proportion discussed was ten to fifteen percent. Families who chose welfare and were enrolled anyway would receive $2.50 biweekly as long as they remained welfare recipients, but would be free to switch between welfare and negative taxation as described above.

We further agreed that the Council for Grants to Families would answer all requests from welfare agencies for information about specific named families, and would make a periodic effort to check welfare caseloads against its roster of sample families. This cooperation would in general be in the families' interest, since lack of communication with welfare could result in some families receiving both sets of payments (either inadvertently or not), a
situation which would very likely come to the attention of the welfare Boards at some time during the three years, resulting in severe penalties for the families. (New Jersey law calls for imprisonment as well as restitution in cases of welfare fraud, and many prosecuted families find it very difficult to re-establish eligibility for welfare.) This agreement resolved some of the potential conflict between welfare's legal right to know and the Council's contractual confidentiality requirements, but stopped short of wholesale opening up of Council records to local agencies.

All those present at the November 1st meeting were convinced that some action regarding welfare needed to be taken, and there was substantial confidence that the procedure outlines above was the correct one. After enrollment in Paterson/Passaic, I wrote the following memo to assess that decision.
TO: NIT Staff
FROM: Heather Ross
SUBJECT: Another Look at Welfare
DATE: March 11, 1969

We are now in a position to evaluate our decision of last November that sample families in cities other than Trenton would not be eligible for negative tax payments and welfare payments simultaneously. The first section below indicates what success we have had in implementing the negative tax-welfare choice among families in Paterson/Passaic. The second discusses the wisdom of this approach in light of initial evidence of the impact of new welfare legislation in New Jersey. A final section reports on a recent conversation with the New Jersey State Director of Public Welfare concerning methods of involving welfare families more directly in our experiment.

I. Paterson/Passaic

Families were enrolled in Paterson/Passaic beginning the week of February 2, 1969. Families who were known to be welfare recipients at that time (either from our earlier questionnaires or from the first income report from which enrollers helped them fill out on the spot) were told about their negative tax plan and about the choice open to them. The outcome is summarized below:

Total welfare families contacted for enrollment 44

| Refused | 7 |
| Enrolled | 37 |
| Chose Welfare | 29 |
| Chose Negative Tax | 8 |

The following chart shows refusals, enrollments, and shifts from welfare by tax plan:

<table>
<thead>
<tr>
<th>Tax Plan</th>
<th>50/30</th>
<th>50/50</th>
<th>75/30</th>
<th>75/50</th>
<th>75/70</th>
<th>100/50</th>
<th>100/70</th>
<th>125/50</th>
</tr>
</thead>
<tbody>
<tr>
<td>Refused</td>
<td>4</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Chose Welfare</td>
<td>2</td>
<td>9</td>
<td>3</td>
<td>1</td>
<td>6</td>
<td>4</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>Chose NIT</td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tbody>
</table>
These numbers suggest what a reading of enroller observation sheets strongly bears out — that families choices are dominated by considerations of financial security. In no case did a family opt for a negative tax plan which would pay it less than it was receiving in welfare. In several cases, families elected to remain on welfare though their immediate negative tax payments would have been higher. The primary reason for this was concern over forfeiting medical vendor payments, although in some instances they were clearly uncertain about the reliability of the negative tax program. The interesting thing is not so much that families made the choices they did — the choices themselves are not surprising, although they do fail to support contentions that people discount welfare income because of the stigma and harassment attached to it. The interesting thing is the knowledgeable and careful way in which families went about maximizing their expected material benefits.

On the matter of verifying the welfare status of sample families, a satisfactory arrangement seems to have been worked out with both the Paterson and Passaic City Welfare Agencies and the Passaic County Welfare Board. Things are now proceeding smoothly, not withstanding an inclination on the part of the County Board to prosecute for fraud families who receive both sets of payments. This latter prospect has caused us to send out a note with our March 8 payment (see attachment) which should make our position clear and bring in to our office any unidentified welfare recipients.

Finally, it should be noted that the total of 29 welfare families presently receiving $2.50 from the experiment is well below the 15% maximum for such families agreed upon last November.

II. New Welfare Legislation

According to the statistics from the State Division of Public Welfare, the total General Assistance caseload for all municipalities in New Jersey dropped from 40,000 to 17,000 families on January 1st as a result of transfers to the new AFDC-UP program. The Division expects the UP caseload to rise rapidly in the future as a result of new "presumptive eligibility"
provisions and a broad definition of eligibility which includes families with an "underemployed" male head. This expectation is also held by Republican members of the legislature who have already publicized their plans for welfare "reform" — elimination of the presumptive eligibility and underemployed parent provisions.

Regardless of the legislative outcome, UP rolls are certain to rise. On March 1st, support levels in AFDC were raised 5% — to $4164 for a family of four. This dominates our highest guarantee — $4125 for a family of four — and extends welfare eligibility higher up the income scale than before. March 1st also saw the introduction in New Jersey of the 1967 Social Security Amendment relating to budgeting of earned income — the so-called 30 and 1/3 "disregard" provision. However, the State Division considered this so costly and so inequitable (as between recipients and non-recipients) that it set a ceiling on income to which the new procedure could apply — effectively disregarding the disregard for families with appreciable earned income. Nonetheless the provision does, to the extent it operates, serve to increase the competitive range between the experiment and welfare.

All in all, the prospects for increasing overlap of eligibility are considerable, independent of any forecast of economic downturn. This, on top of the numbers of welfare families already enrolled (49 or about 10%) suggests that we were well-advised to rule out joint eligibility and that we should continue this policy.

III. Expanding the Negative Tax Sample

We have been giving serious thought to ways of incorporating female-headed families, including welfare families, in the negative tax experiment. There are several reasons for this:

(1) The most frequent criticism of the experiment concerns the narrowness of its eligibility requirements.

(2) We have received a request for proposal and other evidences from HEW that they are interested in examining work incentives for AFDC mothers.
(3) We already have the basis for selecting a random sample of female-headed families as a result of our screening activities.

One of the most attractive ways of including these families would be to conduct a coordinate experiment within welfare, using a waiver under paragraph 1115 of the Social Security Act to set up experimental variations in budgeting arrangements for a sample of recipients. I discussed this possibility last week with Mr. Irving Engleman, the State Director of Public Welfare and a man who has given us much help in the past. He expressed his opinion that intra-state variations in budgeting procedures, with or without a federal waiver, would be illegal. He made a distinction between variation in amounts of money and variation in any other feature of welfare treatment. He said that as a lawyer he would take the case of a family complaining about inferior welfare benefits, and would expect to win it in the courts.

Whether he is correct in his judgment is an interesting question in its own right, and one which Professor Wolfman says he could give us a law student to research. However, if we do wish to pursue the matter of coordinate samples, we must try to meet his reservations, and this will take time. The prospect of drawing female-headed families into the experiment seems to have dimmed somewhat for the moment.
COUNCIL FOR GRANTS TO FAMILIES

Main Office:
92A Nassau St. Princeton, N. J. 08540
Tel. 609-921-6686

March 7, 1969

We wish to remind you that families are not eligible to receive basic income payments during any period in which they are receiving welfare payments from any government agency. In cases where the Council is uncertain about whether a family is receiving welfare, it will be able to obtain this information from welfare officials.

If by some chance your family is presently receiving both welfare payments and basic income payments of more than $2.50, you should call or visit one of the local Council offices right away. This will avoid serious future problems. Also, feel free to contact the offices if you have any questions about this policy.

COUNCIL FOR GRANTS TO FAMILIES

Paterson Office:
77 Broadway
Paterson, New Jersey
742-0833

Passaic Office:
299 Passaic Street
Passaic, New Jersey
471-2905
My feeling about our treatment of welfare recipients now is approximately what it was at the time this memo was written — that we made not a good decision, but a necessary one. My discussion with Engleman was an effort to address the welfare issue and the welfare population more directly, and to explore another avenue of expanding our eligibility requirements. His initial unreceptiveness toward the State's requesting a waiver from HEW, plus the lack of aggressive interest on the part of other staff members toward broadening our sample characteristics, meant that the issue of including female-headed families was effectively laid to rest. Wisconsin had already determined that a quota of female-headed families would be enrolled in its rural study.

The necessity of our decision about welfare recipients has been borne out over time by unspectacular but steady rise in the number of welfare families among our New Jersey sample.
3. Treatment of Persons Who Leave Eligible Family Units

As we accumulated experience in our first three cities, we began to observe an appreciable number of family splits — i.e., separations of the members of an original family unit such that two or more eligible filing units were created. We found that we were having difficulty keeping contact with eligible filers and even, in some cases, learning of splits which had occurred. This stemmed in part from families' unawareness of our provisions for continuing to pay individuals who left original households. But it also resulted from the generally small amounts which such persons were eligible to receive after leaving.

In Chapter II, I discussed the particular poverty standards adopted for one and two person households, and the special weight given to family stability in setting these standards. At the same time that the standards were established, it was ruled that persons other than the first two members of a unit (usually head and wife) who left their original unit and were eligible to file separately would take with them the marginal guarantee of the unit they left. Thus, payments to these new units depended on the size of the original unit from which they came. This produced nine possible tax treatments for families of size \( n, n \geq 2 \), subject to the same tax plan.\(^1\) It also produced very low guarantees for new units — e.g., the first several dependent splits we encountered came from families of size greater than eight, so that the new unit guarantees equaled zero.

Discussions during the planning stage of the project has already considered allowing dependents over 18 who left eligible units and set up their

\(^1\) This just counts families where all persons are eligible members of the tax unit (including eligible for zero guarantee due to family size). If families including one or more ineligible persons are counted, the number of possible treatments with a single assigned tax plan rises to 26.
own households to assume the guarantee attributable to the first family member. This procedure had been ruled out and reopening it did not look propitious. However, MATHEMATICA did wish to see reconsideration of the prevailing policy and so made a compromise proposal, as presented in the following memorandum which I wrote for the signature of other staff members.
TO: OEO and Wisconsin NIT staffs
FROM: David Kershaw, Heather Ross, Michael Taussig, Albert Rees
SUBJECT: Negative Tax Treatment of Family Units Which Split
DATE: July 22, 1969

I. Background

Under our present rules, dependents 18 years of age or older who leave an original family unit are eligible to take with them the marginal guarantee, i.e., the guarantee attributable to the last family members, at the time of their departure. (See Appendix A for a schedule of individual guarantees.) The family unit which they leave loses the marginal guarantee. Thus, the sum of the guarantees attributable to all members of the original family unit is the same whether dependent members continue to live with the family or move out and file separately.

II. Recommendation

We recommend a modification of the rules to allow dependents 18 years or over who leave home to assume the first dependent's guarantee. If the dependent takes with him or her another dependent member of the original household, or if he or she acquires an eligible dependent at some later time, that other dependent will assume the second dependent's guarantee, and so on. In all cases, the unit from which the dependent(s) split will lose the marginal guarantee(s).

III. Discussion

Two main arguments have been raised against moving in the direction we are recommending:

1. Allowing families to increase their guarantees by splitting will produce a financial incentive to family instability or fraud.

2. Allowing families to increase their guarantees by splitting opens the door to unknown cost increases which could exceed our budget capability.
It seems to us that the first argument is an important matter for research. One of the most frequent inquiries we get here concerns the effects of our payments on family structure. As it stands now, we are not going to be able to say anything about the implications of our economies-of-scale benefit schedules for family stability unless, of course, someone wants to know about the restricted variant we are currently operating. There doesn't seem to be much interest in the latter, it being unlikely that a national program would condition payments to an individual or individuals on the basis of the family size from which they came. If we continue this treatment, we will fail to speak meaningfully to an important issue on which we could provide valuable information through a modest change in our rules.

On the question of costs, it is difficult to estimate the total additional expense of modifying our rules as proposed, since this total depends in large part on whether or not the new treatment produces family instability, and to what degree. For the five cases of dependent splits which have occurred to date, the cost differential between the two treatments has been estimated to be quite limited. It is only an estimate because we have had great difficulty getting former dependents to file income reports under our present rules.

We believe that the overall costs of the proposed change are likely to be modest for several reasons:

(1) the low guarantees involved,

(2) the appreciable income levels of those few new households for whom we have some information,

(3) the lack of evidence so far that benefit provisions are playing any role in family decisions about splitting,¹

(4) the difficulty of tracking down some former dependents, regardless of their guarantee.

¹ There is not a single case to date in which families either informed us of a split or inquired about payments beforehand, or in which a member who left contacted us of his own accord afterward.
The calculation in Appendix B indicates that if the new rule on dependents produced a doubling of the current rate at which dependents leave households, the maximum annual cost of adopting the new rule would be about $30,000. This is very much an outside estimate because,

- a doubling of the rate is unlikely,
- each dependent who leaves is assumed to move from dependent 6 to dependent 1,
- most new units will have earned income or welfare benefits,
- some new units will probably be lost.

IV. Conclusion

Our conclusion is that we can buy a good deal of valuable information about family stability under negative taxation at a small probable cost. Note that our recommendation is a compromise one. Allowing the incentives built into our benefit schedules to operate freely would mean permitting dependents who set up a new household to assume the guarantee attributable to the first family member, not just to the first dependent member. This has been discussed before and not adopted. We now seek to move part way in this direction for several reasons:

- the rural NIT study has asked if we might reconsider our rule in order to come more in line with their projected treatment,
- one might argue that the rule we are proposing is a plausible basis for national policy, e.g., one might wish to set up a program in which independent teenagers or persons under 21 would occupy a kind of halfway status, eligible to file but not yet eligible for full benefits.
- we wish to make it at least minimally attractive for dependents who leave households to keep up participation in the experimental program, so that we can do a better job of analyzing who it is who splits and why and at what juncture.

All in all, we see little reason to continue our present treatment. If we wish to study the family stability question with respect to persons other than
head and spouse, then we ought to move to a procedure which has some possibility of policy interest. If we do not want to make this a topic for analysis, then we ought to stop spending time, money, and goodwill on a procedure in which families are no more interested than we are. Our own hope is to study the question, and we strongly recommend that an appropriate rule change be made to allow this.
Appendix A

Payments Attributable to Each Family Member

<table>
<thead>
<tr>
<th>Family Member</th>
<th>Guarantee (% of Poverty Line)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>50%</td>
</tr>
<tr>
<td>Spouse</td>
<td>500</td>
</tr>
<tr>
<td>Spouse</td>
<td>500</td>
</tr>
<tr>
<td>Dependent 1</td>
<td>375</td>
</tr>
<tr>
<td>Dependent 2</td>
<td>275</td>
</tr>
<tr>
<td>Dependent 3</td>
<td>200</td>
</tr>
<tr>
<td>Dependent 4</td>
<td>175</td>
</tr>
<tr>
<td>Dependent 5</td>
<td>150</td>
</tr>
<tr>
<td>Dependent 6</td>
<td>125</td>
</tr>
<tr>
<td>Dependent 7</td>
<td>0</td>
</tr>
</tbody>
</table>
Assuming:

i. Average size of families from which eligible dependents leave = 8,

ii. Each eligible dependent who leaves takes on guarantee of first dependent,

iii. Dependent splits are double the rate experienced to date,

iv. New dependent units can all be traced and have no income or welfare benefits,

v. Average guarantee level for all experimental families equal to 86.5% of the full poverty standard.

Then we have:

1. Average annual guarantee differential between current and proposed rule for each individual who splits = (750 - 250)(86.5%) $ 432

2. Total number of splits per year under new rule 44

3. First year additional payment cost of new rule $9,504

4. Second year additional payment cost of new rule $28,512

5. Third year additional payment cost of new rule $47,520

6. Three-year additional filing fee cost of new rule $6,765

7. Total three-year additional cost $92,301

8. Average annual additional cost $30,767
NOTES:

The Wisconsin group did not agree that MATHEMATICA's proposed modification should be made at that time. In a memo summarizing their position, Harold Watts argued that family composition effects of negative taxation were an important matter for experimentation, but that the design of the New Jersey study would not permit such an inquiry without major alteration. Specifically, "it would be necessary to vary experimental treatment along at least two parameters:

(1) The level of guarantee provided to the dependent upon establishment as an independent claimant (this variation to be partly independent of the tax parameters g and r).

(2) The ability of the dependent-becoming-claimant to create new family members (either through marriage or shared household)."

Furthermore, measuring response to these parameters would affect requirements for both sample size and duration of the experiment. Since such an overhaul of the New Jersey design was unrealistic, the study should attempt instead to simulate the expected outcome of a national program — i.e., "As nearly as possible we would like to so rig the experimental situation that we keep families at the size and structure we expect they might have under a real negative income tax (probably not much different than families are generally structured now) recognizing that in many cases rigging of the situation will be a lot more complicated than simply mimicking the terms of some desirable national negative income tax plan."

Even granting the arguments that:

(1) little of value on the issue of family structure under negative taxation can be learned without controlled experimental variation along a set of parameters designed specifically to test this issue,

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3 Ibid., p. 5.
(2) the New Jersey study cannot be modified to incorporate this kind of inquiry and therefore,

(3) the study should be structured so as to achieve the outcome which is expected (by some people) from a national program, it is hard to believe that the treatment adopted and later confirmed could produce such an outcome. As a number of people have pointed out, the existence of negative tax benefits in a limited experimental setting entails two counterbalancing effects:

(a) a dowry effect — members of an original unit carry with them some benefit which is not generally available, making them especially attractive as marriage partners or other household members.

(b) a solidarity effect — members of an original unit may find it particularly unattractive to leave home and enter another family unit whose members do not have benefits attached to them.

The present New Jersey treatment limits the operation of (a) by assigning purposely reduced amounts to family members who leave home. This arrangement, plus the stipulation that under no circumstances can adults who are not members of original units establish eligibility for negative tax payments, suggests that a better rationalization of the New Jersey treatment would be as one end of a spectrum — i.e., that the experimental results will provide something approximating a lower bound on the separation rates of dependents under a national program. An upper bound could be achieved by allowing dependents who leave home and establish new filing units to assume the guarantee of the first member of a household and to include as eligible members of the new tax unit all persons who become part of that household. This is, in fact, the treatment being incorporated in the rural study.

However, a major difficulty of New Jersey providing this lower bound is the implication therein for the work incentive focus of the study. The work behavior of secondary earners is expected to be especially sensitive to negative tax programs. This is particularly true for the New Jersey study where a substantial proportion of sample families had more than one earner at the beginning of the program, and where benefit levels are such that most families
can make only marginal work adjustments without greatly altering their economic status. A payments structure which tends to group people (earnings) in unrealistic ways — in this case, to discourage dependent splits — could damage the work incentive analysis of the study and its estimate of program costs.

Within the context of our present treatment, we will try to determine the implication for the tax unit income and benefits of changes in family size and composition. But this analysis will be of limited usefulness since we will not know the rates at which various kinds of family restructuring will in fact occur under a national program. And we will probably not retain many split units for analysis, due in considerable part to the low guarantees for which they are eligible. In fact, the outcome of the above memorandum was an agreement to remind all known dependent splits of the program provisions for paying separate filers, but not to expend effort in trying to get them to begin or continue filing.
4. Changes in the Basic Accounting Technique

The last major change in the design of the negative tax study came in August 1969, after an extensive evaluation of the basic accounting procedure as it had operated for a year in Trenton. The following memo, which I wrote for the record, provides a self-contained discussion of the change-over and of our reasons for making it.
TO: The Record
FROM: Heather Ross
SUBJECT: Changes in the Regular Accounting Technique
DATE: September 1, 1969

I

During the preliminary design phase of the experiment, a decision was made to operate the negative tax program on the basis of a three-period moving average plus a year-end adjustment. Each period consisted of four weeks. The year-end adjustment involved calculation of the annual payment for which families were eligible on the basis of their total income for the year, and the comparison of this amount with the sum of the moving-average-based payments which the family received over the year. Any underpayments to families would be made up immediately, and overpayments would be recaptured by deduction from later payments until fully accounted for.

The major purpose of the year-end adjustment was to achieve horizontal equity among families with the same annual income. Income patterns such as seasonality or erratic fluctuations would affect payments over the course of the year, but differences in payments due solely to differences in the timing of income would be washed out at year's end.

Two other arguments were also made in favor of the year-end adjustment:

1) An annual reconciliation would follow standard income tax procedure and facilitate coordination of the positive and negative tax;

2) An annual reconciliation would limit the possibilities for manipulating income to increase negative tax payments, avoid placing a premium on income instability, and rule out certain kinds of "horror stories," where negative tax payments are made to wealthy families temporarily without income.
According to plan, we operated the three-period moving average in Trenton during 1968, and calculated the amount of year-end adjustment. A summary of adjustment results are shown below:

### Trenton — Year-end Adjustment, 1968

<table>
<thead>
<tr>
<th>Adjustment Amount</th>
<th>Number of Families</th>
<th>Average Adjustment Amount</th>
<th>Average Annual NIT Payment for 1969</th>
<th>Average Adjustment as Percent of Average Payment</th>
</tr>
</thead>
<tbody>
<tr>
<td>+ $310.</td>
<td>8</td>
<td>+ $135</td>
<td>$1050</td>
<td>12.8%</td>
</tr>
<tr>
<td>0</td>
<td>7</td>
<td>- $174</td>
<td>$1050</td>
<td>16.6%</td>
</tr>
<tr>
<td>- $463.</td>
<td>29</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>44</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

1 Positive amounts are underpayments to families, negative amounts are overpayments to families.

2 Results are for 44 families for whom a special year-end adjustment program was first run. The program includes calculation of Federal tax rebates, and the early run was done for families on who we had full tax information already in hand.

3 The average annual payment is estimated on the basis of our experience with actual payments thru the middle of August, 1969.

All families who received negative tax payments (other than the minimum $2.50 payment), at some time during 1968 were subject to some adjustment. (The seven families showing zero adjustment above were all consistently above their break-even lines and so had no payments to be adjusted.) This is what one would expect from a procedure of this sort. Only if family income were perfectly stable over the year, or rose and fell by a perfectly balanced amount or amounts, would there be no year-end adjustment.
This large volume of year-end adjustments, some for very small amounts, could be considered to have a large nuisance value, although some deminimus rule could be set on actual paybacks. More worrisome than the number of adjustments, however, were the amounts, ranging in the Trenton summary sample from a further out-payment of $310, to a recapture of $463. These were quite substantial in relation to regular negative tax payments.

The large adjustments appeared to stem from two factors. The first was a possible mis-estimation of initial income, a situation which would have had unusual leverage in Trenton since, due to the slowness of the interviewing operation there, four bi-weekly payments were based on the initial estimate which came from family responses on preliminary interviews. When we took over the interviewing ourselves in later cities, we cut the number of checks based solely on interview data to one. I say a possible mis-estimate, since rather than the estimate being out of date or otherwise inaccurate, it might have been the case that families responded to the introduction of the program almost immediately. This too would explain the differences between initial income estimates and the first income reports filed after the start of the program, although such an explanation appears unlikely except in the case of one or two families where it is quite evidently true.

The second factor in the large adjustment was the steady upward trend in average income over 1968, a trend which seemed to accelerate in the closing months of the year. Thus, the moving average itself operating with a four week lag,¹ was continually working behind the thrust of the rising income, and was badly outdistanced by it as the year ended.

¹ The shortest possible lag in our system would be two weeks — the time between checks. Families would report their income for the four weeks ending on a payment Friday and that income would be reflected in their next negative tax payment two weeks later. We found very early in Trenton, that we could not operate this procedure — the filing speed and accuracy required of families was too great. As a result, we introduced a further two week lag into the accounting process, allowing families a full two weeks to file or correct an inadequate report, and this system has worked satisfactorily as the sample has grown and the volume of checks expanded.
This produced substantial overpayments on an annual basis, overpayments which accounted for the preponderance of negative adjustments since the initial income factor cut about evenly both ways.

III

Any decision to proceed with a year-end adjustment involves two separate considerations. The first is whether an annual period is indeed the best basis for a national negative tax program, and if so, whether annualizing should be achieved by the proposed method.

There is ample evidence that substantial over-the-year fluctuations are a standard income pattern for many low-income households, and that these fluctuations are not necessarily seasonal in nature, nor do they bear any necessary relation to a calendar year. Interrupting the operation of a short-term moving average such as ours because some arbitrary date, say December 31st, has been reached automatically produces adjustment amounts, except where incomes are stable or balanced over the year. But the continued operation of the moving average would itself follow along behind current income and make the necessary adjustment smoothly for all families who remain below their break-even line. For families who rise above their break-even line, of course, the unadjusted moving average produces an unbalanced result since income below the break-even point is taxed and income above it is untaxed. This suggests that what is required is an adjustment procedure which annualizes for all people who are not consistently in need (i.e., below their break-even point) throughout a calendar year, but does not affect other recipients. As will be seen, this is the kind of procedure to which we are now moving.

Still on the question of desirable national policy, it is useful to point out that other national income maintenance programs do not operate on an annual basis — e.g., Welfare operates on a monthly basis, and Unemployment Compensation on a quarterly basis. Also, although it may be desirable to place
the negative tax on an annual basis as a matter of image and ease of coordina-
tion with the personal tax, it is not necessary to do so for purposes of coor-
dination. Even in our experimental set up, we have achieved some crude
coordination by full repayment of federal taxes at year's end to families who
are below their break-even point, and diminishing partial repayment to fami-
lies above that point (à la Tobin).

The second consideration on proceeding with a year-end adjustment of
the sort we had planned, involves the ability of the experiment to simulate the
 corresponding feature of a national program. If the national program were
to operate on a calendar year basis for all families, it might still be imple-
mented in ways not open to the experiment. In particular families it might
be required to pay back money at year's end, or over the course of the next
year. Families in the experiment, however, are assured that they will never
have to pay back any part of the money they receive (unless they obtain it
through knowingly false statements), and this assurance plays an important
role in encouraging them to enter the program. As a result, recapture of
overpayments can only take place by deduction, which means that not all fam-
ilies must "repay," and that families who do must do so when they next fall
below their break-even line, i.e., when they are less able to.

A further experimental limitation lies in the question of knowledge and
its repercussions. Early efforts to explain accounting procedures — moving
averages, year-end adjustments, etc. — to families were only occasionally
successful, and it was clear throughout 1968 that very few Trenton families
understood these details or even grasped that there was anything to under-
stand. Consequently, the year-end adjustment, in the form of substantially
reduced bi-weekly checks, would have come as a definite surprise to families,
and the "repayment" explanation would have produced a good deal of confusion
and suspicion.

The lower checks, plus the suspicions, would almost certainly have
caused attrition from the sample, as our experience with small checks and
family concerns about Council credibility has clearly shown in other situations.
Such attrition would have been biased — i.e., weighted toward those with rising incomes over the year — with resulting damage to the work behavior objective of the study. (In addition, families who remained in the sample might have been influenced by the differential year-end tax treatment of income rises and losses — in Trenton, the largest year-end out-payments to families would have gone to the two families who quit work immediately upon entering the program and reported no further income during 1968. This would have been interesting treatment for a work incentive program.)

Thus, the needs of the experiment, in particular the wish not to heighten families' awareness of their unique experimental status by engulfing them with explanations about their tax treatment, and the wish to retain the sample intact, worked against introduction of the year-end adjustment.

IV

At about the time that discussions on the advisability of the year-end adjustment were taking place in New Jersey, two staff members of the Institute for Research on Poverty were developing a new accounting technique for use in a companion rural study of the negative income tax. The new technique — a carryover system — was designed particularly to deal with the pronounced income seasonability of farm operators, but its originators recommended its use in both programs.

The system, as originally proposed, worked as follows. Families reported their income every four weeks, and negative tax payments in each four-week period were based on the report of the previous period, plus any applicable carryover. Carryovers were generated in any period where the reported income exceeded the families' four-week break-even level, and were equal in amount to the amount of the excess. These carryovers were available to be added to reported income in any period in which reported income was deficient — i.e., fell below the four-week break-even level. In such a period, the carryover was applied either until it was exhausted or until the
deficiency was eliminated (i.e., until income for the period was raised to the
break-even level), whichever came first. If the deficiency was eliminated
before the carryover was used up, the remaining carryover was available for
application in later deficient-income periods, subject to the limitation that
carryovers could be carried forward no more than thirteen periods (one year).
Three alternative conventions for applying carryovers were proposed: first-
in/first-out, last-in/first-out; and a ratable drawdown, the last being a
weighted average of the first two. Preference was expressed for the first-in/
first-out approach.

The Klein-Bawden carryover system was received with great interest
in New Jersey, as it appeared to avoid several of the kinds of problems which
troubled the year-end adjustment. Specifically, it achieved long-term equity
without imposing calendar year reconciliations on all families, and it achieved
this equity by preventing outpayments rather than by attempting later recapture
of payments already made.

As pointed out by Harold Watts, the new system had the same two-parameter
flexibility as the old — the income averaging provision determined the
sensitivity of response to current family income change, while the maximum
allowable carryoverage set the period for achieving long-term equity. These
two parameters could be set independently, just as the period for the moving
average and the period for the final (annual) reconciliation could be set inde-
dependently in the old system. As part of his analysis, Watts did some exten-
sive computer simulations of different carryover systems as applied to actual
income histories of Trenton families and to constructed five-year income
histories of hypothetical families. The latter histories consisted of stable
incomes, upward and downward trends, strong seasonal variations, erratic
windfall gains, and various random patterns. For the income sensitivity
factor, in addition to the original Klein-Bawden proposal of a single period,
moving averages of three periods and thirteen periods were used. Alterna-
tive maximum carryover ages of 8, 13, and 18 periods were also used. The
simulations did not produce surprising or unexpected results, but did give
confidence of the carryover's ability to handle satisfactorily a wide range of income patterns and provided a basis for selecting a preferred set of parameter values. The principal standard of comparison among various systems was how close they came to yielding negative tax payments over the year which were equal in sum to the annual negative tax benefit due on the basis of annual income.

V

We are now in the process of introducing a carryover system in New Jersey. By September 26th, when Scranton families are sent their first mailed check, the system will be fully operable. The parameter values are the same as those used in the old accounting system, and for the same reasons that the original choices were made (plus, of course, the further consideration that continuation of these values will be least disruptive to operation of the experiment).

Benefits in any period are based on the average of reported income in the previous three periods, plus any applicable carryover. Carryovers are generated in any period in which the moving average exceeds the four-week break-even line. They are applied on a first-in/first-out basis and have a maximum life of one year. The three-period moving average allows sensitive but smoothed negative tax payments in response to changing family income. The carryover limit of one year allows appropriately for seasonal income variations, follows personal income tax precedent, and incorporates a reasonable assumption about the length of time over which earlier receipts may be available to meet later expenses.

For Trenton only, however, income reported during the period from August to December 1968 will be eligible for carryover through December 1969. This special provision has been adopted to avoid disregarding entirely excess income generated in Trenton during 1968. It is a fairly lenient treatment which will affect the payments of only a few families. Only seventeen
families in Trenton generated any carryover at any time during 1968, and seven of these have been continuously above their break-even point since the beginning of the program. Thus, only ten families are likely to have their negative tax payments affected by 1968 carryovers, and some of these will not be affected if they maintain their current income level through the end of the year.

For some families coming into the program, the carryovers they would have accumulated over the year prior to the start of the program would have been enough to wipe out negative tax benefits to them for an extended period of time after the start of the program, even if their income after entering the program had dropped substantially. Because of the adverse affects this waiting period could have had on enrollment acceptance rates, because of the demonstrated imprecision of recall data on income, and because of a reluctance to have current negative tax benefits and, consequently, current work behavior influenced in this way by pre-program earnings, we thought it desirable to start families from scratch with a zero carryover at the beginning of the program. This decision was reinforced by the fact that only Scranton families were left to be enrolled after the introduction of the carryover. For other families, reconstructing pre-program carryovers and applying them now would have conditioned current negative tax benefits on income as much as two years old.

VI

Contrasting the outcomes of applying the two different accounting procedures in Trenton provides ample evidence that the two treatments are not equivalent. The only families who receive identical treatment under the two plans are those who are consistently above their break-even points. Even those families whose average income is above the break-even point may receive different treatment in the first year of operation depending on whether their income showed a net rise or fall round the break-even point over the
year. This, of course, is purely a start-up phenomenon — if their average income remains above the break-even point year after year, they will receive no benefits regardless of how far their income may drop below the break-even point in any single period or set of periods.

For families below the break-even point, both the amount and pattern of income will affect negative tax benefits. A family whose income is steadily rising will receive more than a family whose income is steadily dropping around the same average. This may not be desirable as preventive medicine — it does not provide substantial resources to families at the first sign of need. But it does have more agreeable incentive implications than the year-end adjustment, of which it is the mirror image. Families who achieve a steady increase in income up to and beyond the break-even line, never to fall back again, receive a bonus from lagged negative tax benefits which is never offset or recaptured.

Of course, it follows that since almost no one below the break-even line under the old year-end adjustment procedure required no adjustment, almost no one under the new carryover procedure will receive exactly the annual benefit to which he would be entitled on the basis of annual income. These discrepancies will tend to net out over time for families with fluctuating incomes, and for other families they may come to be seen as good incentive policy. Also, as mentioned above, allowing the moving average to operate without adjustment will avoid the cost and annoyance of carrying out year-end transactions with virtually every recipient unit. This is no small gain, as we in New Jersey can attest.²

² This treatment also involves an implicit assumption about saving — namely, that current income in excess of some defined level of need, in this case the break-even point at which eligibility for negative taxes ceases, need not be used for current consumption but may be put away to meet expenses in later periods. These savings, providing they are not over a year old, must be drawn down to meet current needs before eligibility for negative taxes can be established.

This arrangement is analogous to the requirement in many welfare programs that family resources be exhausted before welfare payments can begin. It is more lenient in that resources are eligible for carryover for no more than one year and that the only resources considered for carryover
In summary, faced with the need to incorporate some kind of income averaging provision for families whose incomes fluctuate around the break-even level, plus the experimental restriction on direct assessment of families as a means of making averaging adjustments, the carryover system provides a tailor-made solution. It makes the necessary averaging adjustment by deduction at the time when families are most likely to be able to tolerate such deduction — directly after a period of relatively high income. The introduction of the carryover appears at this point to be a major improvement in the design of the study, and the system itself a matter of real interest for a national program.

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2 (Cont.)
are cash income and a housing imputation for homeowners and subsidized renters. However, this resource definition is largely an experimental artifact — made possible by strict control over entry into the sample. Many proposals for national negative tax programs feature asset tests or imputed income on assets. But these proposals reflect a view that families should rely in part on their own assets to meet current needs, not that families should become utterly destitute before receiving assistance.
Chapter V
SAMPLE SIZE AND ALLOCATION: 1967 - 1969

The major experimental design question for the negative tax project was determination of the total number of families to be included in the program, and their distribution over admissible tax treatments. This turned out to be a controversial matter, which, after a late start, became the subject of lengthy debate. This chapter deals with development of a design model for sample size and allocation from preliminary work at OEO in June 1967 to a final, binding decision in May 1969. In what follows, the overall two-year period will be organized into seven stages:

- (1) Memorandum: Glen Cain
- (2) Proposal: Heather Ross
- (3) Design Model: John Conlisk and Harold Watts
- (4) Analysis: MATHEMATICA
- (5) Discussion: Pre-Paterson/Passaic
- (6) Discussion: Post-Paterson/Passaic
- (7) Decision: James Tobin

(1) In its original proposal submitted to OEO, MATHEMATICA projected a sample size of 1,000 observations for the negative income tax study. (This was the sample size suggested in a prototype proposal which OEO had considered earlier) and which had formed the basis for its decision to explore.

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seriously the possibility of an experiment.) MATHEMATICA indicated that
precise determination of the sample size and allocation over various treat-
ments would be a major task of its preliminary design phase.

While MATHEMATICA was putting its proposal in final form, Glen Cain
in the Office of Research and Plans at OEO prepared a memorandum\(^2\) which
reassured the agency that a sample of size 1,000 would be adequate to sup-
port the proposed work incentive analysis. His approach was to estimate,
for various sample allocations within a sample size of 1,000, what magnitude
of response to experimental treatment could just be distinguished.

He considered a regression framework in which change in earnings of
sample families (the dependent variable) was explained by a dummy variable
representing families' negative tax treatment (the treatment variable) and by
other independent variables indicating families' socio-demographic-economic
status. Utilizing the relationship

\[
\beta = \frac{\sigma_u}{\sigma_x'} \sqrt{\frac{1 - r^2}{1 - r^2}},
\]

where \(\beta\) = the partial regression coefficient of the treatment variable \(x\) on the
dependent variable, change in family earnings
\(\sigma_u\) = the standard error of the estimate of changes in family earnings
\(\sigma_{x'}\) = the standard error of the treatment variable \(x\) when \(x\) is regressed
on all other independent variables in the regression equation
\(r\) = the partial correlation coefficient between the treatment variable
\(x\) and the dependent variable, change in family earnings,

he considered the case where, assuming a sample of 1,000 with twenty inde-
dependent variables, the size of \(r\) is just large enough to produce a statistically
significant \(\beta\). This is conventionally accepted to be the point where the \(t\)-ratio
equals two, since at this point, for a reasonably large number of degrees of
freedom, the confidence coefficient associated with the \(t\)-ratio is approxi-

\(^2\) Cain, Glen C., "A Discussion of Sample Size for the Experimental Study of
mately .95. The relationship between the t-ratio, \( r \), and degrees of freedom, \( n \), can then be solved for the desired \( r \).

\[
t = \frac{\beta}{\sigma_\beta} = \sqrt{\frac{nr^2}{1 - r^2}} = 2
\]

\[r^2(n + 4) = 4\]

\[r^2 = \frac{4}{984} = .0041\]

\[r^2 = .064\]

(Cain's calculated value of \( r \) was .063 which will be used in the rest of this discussion.)

A value for \( \sigma_{x'} \) was calculated using the assumption that the treatment variable took the form of a dummy variable in the regression equation.\(^3\) It is easy to determine the standard deviation of such a treatment variable since experimenters have control over the assignment of treatments and the variance of a dummy variable depends only on the number of observations assigned to the dummy classification \( n_1 \), and the total number of observations, \( N \).

\[
\sigma_{x'}^2 = \frac{n_1(N - n_1)}{N(N - 1)}
\]

Setting \( N = 1,000 \) and varying the value of \( n_1 \) (the allocation of families over various numbers of treatments) yielded different values for \( \sigma_{x'}^2 \). These values were then reduced by 10% to obtain a residual variance \( \sigma_{x}^2 \), left over after

\(^3\) Each experimental tax plan was represented by a dummy variable, coded for each participant as "1" if the treatment applied to that unit, and "0" otherwise.
some part (i.e., 10%) of the variance of the treatment variable was assumed to be explained by chance by other independent variables.

Further assumptions were made to estimate likely values for the standard error of the estimate, $\sigma_u$, from data on income of a panel of Negro renters from the one in 1,000 sample of the 1960 Census. Assuming that the known variance of income over the panel in one year was equal to (or, alternatively, twice as great as) the expected variance of income among families in the negative tax study, that the same variance held in the next year, and further assuming different values of the correlation coefficient between income levels from year to year, Cain calculated a range of possible values for the variance of year-to-year changes in income. Assuming finally that independent variables (other than previous year’s income) would explain twenty percent of this calculated variance, he arrived at a range of values for the standard error of the estimate, the smallest of which was $441 when correlation between income in successive years was set at .95.

All these components were used to produce the following table:

Sizes of Coefficients of a Treatment Variable as a Regressor on Changes in Earned Income, for $r = .063$, $N = 1,000$, and Different Values of $\sigma_u$ and $\sigma_x$

<table>
<thead>
<tr>
<th>No. in treatment group $n_j$</th>
<th>No. of treatment groups with experimental group $= 500$</th>
<th>800</th>
<th>$\sigma_x^*$</th>
<th>$\sigma_u$</th>
<th>$\sigma_x^*$</th>
<th>$\sigma_u$</th>
<th>$\sigma_x^*$</th>
<th>$\sigma_u$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10</td>
<td>50</td>
<td>80</td>
<td>.0944</td>
<td>167</td>
<td>335</td>
<td>668</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25</td>
<td>20</td>
<td>32</td>
<td>.1483</td>
<td>167</td>
<td>335</td>
<td>668</td>
<td></td>
<td></td>
</tr>
<tr>
<td>50</td>
<td>10</td>
<td>16</td>
<td>.2069</td>
<td>107</td>
<td>213</td>
<td>426</td>
<td></td>
<td></td>
</tr>
<tr>
<td>100</td>
<td>5</td>
<td>8</td>
<td>.2848</td>
<td>76</td>
<td>153</td>
<td>306</td>
<td></td>
<td></td>
</tr>
<tr>
<td>200</td>
<td>2.5</td>
<td>4</td>
<td>.3795</td>
<td>56</td>
<td>111</td>
<td>222</td>
<td></td>
<td></td>
</tr>
<tr>
<td>500</td>
<td>1</td>
<td>1.6</td>
<td>.4746</td>
<td>33</td>
<td>67</td>
<td>133</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Referring to the table, Cain concluded that a sample of size 1,000 was large enough to discern effects which researchers would want to be sure to see if those effects, in fact, existed. For example, even with a residual
standard deviation in year-to-year income changes as high as $1,000, a sample of 1,000 observations with 800 experimental units divided equally into eight treatment groups could detect a treatment effect in any particular group as small as $222 per year.

On the basis of his work, Cain made several further observations. Noting that the size of the just significant coefficient was directly proportional to $\sigma_u$, he recommended efforts to construct a comprehensive income determination model which would reduce this residual variance. Judging from the low $R^2$'s generally obtained in econometric analysis of microeconomic data, the gains along this line from refining a model would probably be small, but they would be well worth the time and degrees of freedom used up, and the presence of appropriate economic variables in the equation would avoid bias in the estimates of the effects of the treatment variables which would arise if the treatment and economic variables were correlated.

He also suggested a strategy for allocating families to treatments based on the magnitude of expected work disincentive effects. Those treatments which economic theory indicates would have relatively larger effects — specifically, those with higher levels of support and higher taxes — would need fewer observations assigned to them to detect the anticipated response. He ventured that different magnitudes of response would be accepted as just significant — i.e., just important enough to distinguish — for different tax treatments, and he noted that this allocation strategy held agreeable implications for the total budget since it led to a concentration of observations in low disincentive low cost plans.

He further pointed out the efficiency of thoroughgoing randomization in the assignment of treatments to families, since the size of the just discernible

4 (footnote of page 175)
This analysis neglects (random) observational errors, and therefore overstates the precision of the coefficient estimates to the extent that such errors occur.
treatment coefficient declined as the residual variance of the treatment variable rose toward its total variance. Threats to complete orthogonality of design might come from chance correlation of treatments with other explanatory variables in a small sample, or from systematic bias introduced by differential rates of refusal to enroll or to continue to participate in different treatments.

Finally, he concluded from experimenting with a substantially larger sample size that the gains in terms of precision of estimates were not worth the extra costs of the added observations. In particular, he noted that gains from expanding the control group were virtually nonexistent.

This memorandum significantly increased OEO’s confidence in samples of the size being proposed by MATHEMATICA, and eased one of their few remaining scientific concerns about finding the project. However, once the project was underway, despite a number of discussions and working papers undertaken during the design phase, no formal model for determining exact sample size and allocating families to tax treatments was adopted by MATHEMATICA and Wisconsin before time came to enroll the first participants in the program. Thus, in August, 1968, the first contingent of 136 Trenton families was randomly assigned to treatments, with families having a one-third chance of falling into the control group and a 2/21 chance of falling into any one of the seven admissible tax treatment groups. No one found this equal allocation of experimental families satisfactory for the body of the program, and work went forward at both Wisconsin and MATHEMATICA to develop a more sophisticated allocation model. In January 1969, about one month before assignment was due to take place in Paterson and Passaic, I finished a draft of the following paper.

(2) Optimal Allocation of Families to Negative Tax Treatments.

I. Analysis of Variance Model

Assume all family income other than negative tax payment is earned.

Let
\( Y^*_i \) = normal earned income of families in income stratum \( i \) \( (i = 1, 2, 3) \).

\( Y_{ij} \) = actual earned income of families in income stratum \( i \) enrolled in tax plan \( j \) \( (j = 1, \ldots, 10) \).

\( R^*_{ij} = R(Y^*_i, r_j, G_j) \) = normal earned income response of families in income stratum \( i \) to tax plan \( j \) (tax rate \( r_j \), guarantee level \( G_j \)).

We are dividing the sample into three strata, each characterized by a normal income level \( Y^*_i \). Families in the null plan \( (j = 1) \) are expected to earn on average their normal income.

\[
Y_{i1} = Y^*_i + u_i, \quad u_i \sim N(0, \sigma^2_{u_i}) .
\]  \hspace{1cm} (1)

In other words, normal income is defined as that income which families in any given income stratum are expected to earn on average in the absence of negative tax treatment.

Families in the experimental tax plans \( (j = 2, \ldots, 10) \); earn what they would have earned in the null plan plus some amount in response to their experimental treatment.

\[
Y_{ij} = Y^*_i + R_{ij} + u_i , \hspace{1cm} j = 2, \ldots, 10 ,
\]  \hspace{1cm} (2)

where \( R_{ij} = R^*_{ij} + e_{ij} \); \( e_{ij} \sim N(0, \sigma^2_{e_{ij}}) \).

Assuming \( u_i \) and \( e_{ij} \) independent, \(^5\) we have for all \( i, j \)

\(^5\) This may not be strictly true, since a family's response to its negative tax plan may be affected by its perception of derivations from "normal" in its underlying income prospects. However, this will be a second-order effect and may be accommodated later by including in the arguments of \( R \) a variable for income instability. For the issue at hand the stratification of families by income — the assumption of independence throughout all income ranges should not bias the result.
\[ Y_{ij} = Y_i^* + R_{ij}^* + e_{ij} + u_i \]  

(3)

as a general expression of income behavior of families in the sample.\(^6\) Then an estimate of the final earned income of \(K_{ij}\) families in stratum \(i\) subject to tax plan \(j\) is

\[ Y_{ij}^* = \sum_{k=1}^{K_{ij}} \frac{Y_{ijk}}{K_{ij}} = Y_i^* + R_{ij}^* + \sum_{k=1}^{K_{ij}} \frac{e_{ijk} + u_{ik}}{K_{ij}}. \]  

(4)

The variance of this estimate is

\[ \sigma_{Y_{ij}}^2 = \frac{\sigma_e^2 + \sigma_u^2}{K_{ij}}. \]  

(5)

To achieve an optimal allocation of observations over the 30 treatment cells (3 income strata by 10 tax plans), we wish to minimize the weighted sum of the relevant variance for each cell over all cells, where the weights, \(f_{ij}\), are measures of our interest in each of the \(ij\) cells.

\[ \text{Minimize} \sum_{i=1}^{3} \sum_{j=1}^{10} f_{ij} \frac{(\sigma_e^2 + \sigma_u^2)}{K_{ij}} \]

subject to the cost constraint

\[ B = \sum_{i=1}^{3} \sum_{j=1}^{10} K_{ij} C_{ij}, \]

where \(C_{ij}\) = average cost of a family in stratum \(i\) enrolled in tax plan \(j\).

---

\(^6\) We are assuming that \(R_{ij} = 0\) for \(j = 1\). This will not be true in the experiment if control group families react to their own treatment (quarterly interviews) or to the fact that other families in the sample are receiving money.
Forming the Lagrangian

$$L(K_{ij}, \lambda) = \sum_{i=1}^{10} \sum_{j=1}^{10} f_{ij} \left( \frac{\sigma_{e_{ij}}^2 + \sigma_{u_i}^2}{K_{ij}} \right) - \lambda \left( B - \sum_{i=1}^{10} \sum_{j=1}^{10} K_{ij} C_{ij} \right)$$

and differentiating

$$\frac{\delta L}{\delta K_{ij}} = - \frac{f_{ij} \left( \frac{\sigma_{e_{ij}}^2 + \sigma_{u_i}^2}{K_{ij}^2} \right)}{K_{ij}^2} + \lambda C_{ij} = 0$$

$$K_{ij}^2 = \frac{f_{ij} \left( \frac{\sigma_{e_{ij}}^2 + \sigma_{u_i}^2}{\lambda C_{ij}} \right)}{\lambda C_{ij}}$$

$$K_{ij} = \left[ \frac{f_{ij} \left( \frac{\sigma_{e_{ij}}^2 + \sigma_{u_i}^2}{\lambda C_{ij}} \right)}{\lambda C_{ij}} \right]^{1/2}$$

we arrive at a formula for the optimal allocation of sample families over income strata and tax plans.

II. Response Hypothesis

In order to apply the analysis of variance model just presented, we need to develop estimates for each of the parameters appearing on the right-hand side of Eq. (6). The major task is to anticipate the response of families to negative income taxation so as to arrive at estimates of $e_{ij}$ and $C_{ij}$ for each cell. We wish to apply whatever knowledge we may have about response to come up with a work behavior hypothesis which we are willing to test with maximum efficiency — that is, which we are willing to make the basis for our optimizing procedure. In this section, a model of income determination under negative income taxation will be developed to provide such a basis.
Traditional static analysis of the impact of negative income taxation leads unequivocally to an expectation of work disincentives. The income and substitution effects of the negative tax rate operate as they do for any other tax on work effort. The marginal rate lowers the net wage (the price of leisure), thereby producing a substitution effect in favor of leisure, while the average rate reduces total income, thereby producing an income effect in favor of work effort. The net result of these two effects cannot be resolved by theory but must be determined empirically case by case. However, the negative tax program couples its tax rate with cash benefits which produce a net rise in income, and thus an income effect in favor of leisure. That is, at the new higher income level, families purchase more leisure as long as leisure is not an inferior good. Thus, both the overall income effect and the substitution effect of the program point in the direction of more leisure and less work.7

There are a number of limitations with this standard tax incentive analysis in any context, but it may be particularly inadequate when applied to a negative tax program for low-income families.8 Major assumptions may not hold. Leisure idleness may not be a homogeneous, superior good throughout relevant income ranges. The utility of work may not derive solely from its financial return. Utility functions may not be smoothly convex due to physical, institutional and social constraints on hours of work — in particular, corners may occur in indifference curves reflecting marked preference changes in the vicinity of the eight-hour work day, so that net wage rates could change markedly without much altering hours worked. Budget constraints may not be smooth, fixed, continuous, or even known. Limited, unstable employment


opportunities at low-income levels suggest budget lines which are impermanent and discontinuous, while limited market information suggests worker uncertainty or ignorance as to even those lines as may exist.

Finally, and most importantly, truly marginal adjustments may not be the only, or even the dominant responses taking place in the presence of a substantial negative tax program. Those who see poverty as an economic phenomenon might best view whatever utility maximization takes place at low-income levels as a striving for inferior local optima defined by a set of severe financial constraints. By easing these constraints significantly, the negative tax program can open up new areas of the utility map which were formerly closed, and in doing so, could markedly change expectations and aspirations and, correspondingly, work-leisure preferences. For example, negative taxes could, over varying periods of time, produce higher real wages and/or more hours of work per family through support of day care, health care, education, job search and training, transport and mobility, etc. Changes in objective economic situation could significantly change recipients' view of themselves and of the world, leading to greater awareness of their situation and opportunities, to a feeling of control over their lives and environment, and perhaps to a perception of society as more responsive to their needs and more promising for them and their children. The changes in attitudes and aspirations could have marked work incentive repercussions.

Thus, work behavior under negative taxation is most uncertain — the range of potential rational response is very wide and the great diversity of human preferences is entirely capable of scattering individuals throughout the range (and even beyond). This is not a surprising conclusion to reach at this point; it is precisely the situation which gave rise to the need for an experiment. But it is important in designing the experiment to sift through the possible responses and select those that appear to provide the soundest basis for structuring the work analysis.

We will choose to focus on work disincentive responses for several reasons:
(1) Disincentive responses are suggested by static utility-maximization theory, and although that theory almost certainly does not capture fully the likely effect of negative taxation, it does constitute a well-developed body of thought which does provide a clear-cut framework for the analysis of work response.

(2) A great many people covering a wide range of prejudice, opinion, and judgment, believe work disincentive responses to be the most likely outcome of any income maintenance program.

(3) Policy concern in the area of work response is directed mainly toward disincentive effects which may operate to undermine the primary goal of the negative income tax, which is income maintenance not positive work incentives.

Having decided to test for the existence and magnitude of work disincentive effects, we must ask if there is any further information on the likely contours or extent of disincentive responses which we feel confident enough of to build into the experimental design. We observe that people seldom retreat by choice to a lower standard of living than they had previously experienced, and seldom purposefully lose rank in the relative income scale. We will assume that there exists a lower bound to disincentive responses set by an overriding disutility which people attach to absolute losses in disposable income and/or to a decline in relative income status. An upper bound to disincentive response will be set at zero — i.e., no work reaction at all to the introduction of the negative tax. Thus, we have the hypothesis:

The income and substitution effects of negative taxation combine to produce zero or positive work disincentives, as measured by change in earnings after tax, but these disincentives are limited in their operation to that range of total incomes wherein families are at least as well off financially after negative tax as before.

Employing this hypothesis and the income determination model from Section I, we see that the maximum response (greatest earnings loss) shown by families in income stratum i to tax plan j occurs, when they reduce their earned income to level $Y_{ij}'$, where
That is, families with normal income above their break-even line have no latitude for response to negative taxation and continue to earn on average their normal income. Families with normal income below their break-even level but above their guarantee level may reduce their earnings down to the point where their total income including negative tax benefits equals their normal income. Families with normal income below their guarantee level are free to reduce their earnings to zero if they choose.

Furthermore, the minimum response (least earnings loss) shown by families in income stratum $i$ to tax plan $j$ occurs when they continue to earn their normal income $Y_i^*$. So the range of earned income response of families in treatment cell $ij$ ($i=1, 2, 3; j=2, \ldots, 10$), can be expressed as:

\[
Y_i^* - Y_{ij}' = \begin{cases} 
0, & Y_i^* \geq G_j/r_j \\
\frac{G_j - r_j Y_i^*}{1 - r_j}, & G_j/r_j > Y_i^* > G_j \\
Y_i^*, & Y_i^* \leq G_j 
\end{cases}
\]  

Also from Section I, families in each treatment cell are normally distributed across the range of response, so that the mean earned income after negative tax of families in each cell can be expressed as:
\[ \bar{Y}_{ij} = Y_i^* - \frac{1}{2} \left[ Y_i^* - Y_{ij}^* \right] = \begin{cases} 
Y_i^* , & Y_i^* \geq G_j/r_j \\
\frac{(2 - r_j) Y_i^* - G_j}{2(1 - r_j)} , & G_j/r_j > Y_i^* > G_j \\
Y_i^*/2 , & Y_i^* \leq G_j .
\end{cases} \tag{9} \]

We can now obtain an estimate of the variability of income response to negative income tax, \( \sigma_{e_{ij}} \), as a function of the range of such response. We will set the standard deviation equal to a quarter of the range, the relationship which holds between the standard deviation and the range of observed values of a normal random variable for samples of size 27.\(^9\)

\[ \sigma_{e_{ij}} = \begin{cases} 
0 , & Y_i^* \geq G_j/r_j \\
\frac{1}{4} \left[ \frac{G_j - r_j Y_i^*}{1 - r_j} \right] , & G_j/r_j > Y_i^* > G_j \\
Y_i^*/4 , & Y_i^* \leq G_j .
\end{cases} \tag{10} \]

We also have an estimate of the average variable cost, \( \bar{V}_{ij} \), associated with each treatment cell — i.e., the average annual negative tax benefit paid to families in that cell.

\[ \bar{V}_{ij} = G_j - r_j (Y_{ij}) = G_j - r_j Y_i^* = 0 , \quad j = 1 \\
= G_j - r_j Y_i^* , \quad j = 2, \ldots, 10 ; \quad Y_i^* \geq G_j/r_j \tag{11} \]

\(^9\) Owen, Donald B., Handbook of Statistical Tables, Addison-Wesley, Inc., Reading, Mass., p. 140.
\[
\overline{V}_{ij} = G_j - r_j \left( \overline{Y}_{ij} \right) = G_j - r_j \left[ \frac{(2 - r_j) Y^*_i - G_j}{2(1 - r_j)} \right] \\
= \frac{2G_j - 2r_j G_j - 2r_j Y^*_i + r_j^2 Y^*_i + r_j G_j}{2(1 - r_j)} \\
= \frac{(2 - r_j)(G_j - r_j Y^*_i)}{2(1 - r_j)}, \quad j = 2, \ldots, 10; \quad G_j / r_j > Y^*_i > G_j
\]

\[
\overline{V}_{ij} = C_j - r_j \left( \overline{Y}_{ij} \right) = C_j - r_j \left( \frac{Y^*_i}{2} \right), \quad j = 2, \ldots, 10; \quad Y^*_i < G_j.
\]

These variable costs will be added to fixed costs representing expenses of identifying participants and administering the experimental program, to obtain an estimate of the overall observation costs, \(C_{ij}\).

III. Various Parameter Values

A. Underlying income variation

Estimates for \(\sigma_{u_i}\), the standard deviation of year-to-year changes in income which occur independent of the negative income tax treatment, can be calculated from data presented by Harold Watts from an analysis of year-to-year changes in income reported on tax returns to the State of Wisconsin.\(^{10}\)

These data show the distribution of standard errors by mean income level for regressions of income on lagged income using the tax files of approximately 1,000 male individuals over ten years.

\(^{10}\) Watts, Harold W., "Notes on the Variability of a Sample of Male Wisconsin Taxpayers," undated memo, University of Wisconsin. The comprehensive coverage of the Wisconsin State income tax, which allows few exemptions and deductions, makes it a good source of data for even low-income households.
Table A. Underlying Income Variation

<table>
<thead>
<tr>
<th>Income level (% of poverty line)</th>
<th>Standard deviation of year-to-year changes in income ($\sigma_{ij}$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - 50%</td>
<td>$244</td>
</tr>
<tr>
<td>50 - 75%</td>
<td>$586</td>
</tr>
<tr>
<td>75 - 100%</td>
<td>$640</td>
</tr>
<tr>
<td>100 - 125%</td>
<td>$762</td>
</tr>
<tr>
<td>125 - 150%</td>
<td>$1168</td>
</tr>
</tbody>
</table>

B. Interest weights

We must next give values to the weights, $f_{ij}$, which measure our interest in the income/tax plan cells. Let

$$f_{ij} = g_i h_j,$$

where $g_i$ = percent of families nationwide meeting experimental eligibility requirements whose normal incomes lie in stratum i.

$h_j$ = number expressing our relative policy interest in various tax plans.

For the moment, we will set $h_j = 1$ for all $j$, primarily because of no strong wish to build in any other judgment at the present time. This says we are equally interested in response at all observation points in the policy space and avoids arbitrary judgments as to which plans are more likely or more desirable, and to what degree. A good estimate of the $g_i$'s can probably best be obtained from the actual population frequencies in the Trenton sample, since we enrolled all eligible families screened in Trenton regardless of income, and since we presumably feel Trenton is representative of the national (urban, intact, non-aged family) population to which we plan to generalize. In particular, these frequencies are probably more valid than data on family income in the urban northeast during 1959 (updated by some rule of thumb) which have
previously been used for this purpose. The following table shows the Trenton results.

Table B-1. Distribution by Family Size and Relative Income of Eligible Households in Trenton

<table>
<thead>
<tr>
<th>% of Poverty line</th>
<th>Family size</th>
<th>Total number of households</th>
<th>Average household size</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2 3 4 5 6 7 8+</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 - 50%</td>
<td>0 1 1 3 1 0 1</td>
<td>7</td>
<td>5.3</td>
</tr>
<tr>
<td>50 - 75%</td>
<td>0 0 3 1 2 0 3</td>
<td>9</td>
<td>6.2</td>
</tr>
<tr>
<td>75 - 100%</td>
<td>1 2 6 1 2 8 8</td>
<td>26</td>
<td>6.3</td>
</tr>
<tr>
<td>100 - 125%</td>
<td>1 6 3 10 7 10 6</td>
<td>43</td>
<td>5.8</td>
</tr>
<tr>
<td>125 - 150%</td>
<td>0 3 9 10 8 10 10</td>
<td>50</td>
<td>6.0</td>
</tr>
</tbody>
</table>

Since there is a gain in efficiency from having strata of roughly equal frequency, we employ the following three strata in making our calculations.

Table B-2. Population Frequencies

<table>
<thead>
<tr>
<th>Income level</th>
<th>Number of Trenton families</th>
<th>Percent distribution ($g_1$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - 100% of Poverty line</td>
<td>42</td>
<td>31.1</td>
</tr>
<tr>
<td>100 - 125% of Poverty line</td>
<td>43</td>
<td>31.9</td>
</tr>
<tr>
<td>125 - 150% of Poverty line</td>
<td>50</td>
<td>37.0</td>
</tr>
</tbody>
</table>

These population frequencies will be used to represent our interest weights, $f_{ij}$.

C. Tax parameter values

Tax plans are defined in terms of percentages of the poverty standard adopted by the experiment and can be converted to dollar amounts only by
referring to family size, a variable which we will not attempt to control.

From Table B-1, we see that the average family size among eligible families in Trenton is approximately 6, and does not vary greatly among income strata. Thus, in the calculations which follow, each of the ten tax plans will be represented by a single standardized plan for a family of size six.

The following table shows the parameter values for each of these ten standard plans, where the table entry is the tax plan number \( j = 1, \ldots, 10 \).

<table>
<thead>
<tr>
<th>Tax rate, ( r_j )</th>
<th>Income guarantee, ( G_j )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0%</td>
</tr>
<tr>
<td></td>
<td>$0</td>
</tr>
<tr>
<td>0%</td>
<td>1</td>
</tr>
<tr>
<td>30%</td>
<td>-</td>
</tr>
<tr>
<td>50%</td>
<td>-</td>
</tr>
<tr>
<td>70%</td>
<td>-</td>
</tr>
</tbody>
</table>

D. Normal incomes

From the Trenton sample we can also get reasonable estimates of the normal income \( Y^*_i \) which characterize each income stratum (i.e., the average income in each stratum before experimental treatment).

---

11 This relatively large average family size stems from exclusion of households with an aged head and from income ceilings on eligibility which rise with family size.
Table D-1. Mean Incomes by Income Stratum and Family Size for Eligible Households in Trenton

<table>
<thead>
<tr>
<th>% of Poverty line</th>
<th>Family size</th>
<th>Average</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>0 - 50%</td>
<td>-</td>
<td>1300</td>
</tr>
<tr>
<td>50 - 75%</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>75 - 100%</td>
<td>1884</td>
<td>2210</td>
</tr>
<tr>
<td>100 - 125%</td>
<td>2340</td>
<td>3133</td>
</tr>
<tr>
<td>125 - 150%</td>
<td>-</td>
<td>3818</td>
</tr>
<tr>
<td>Average</td>
<td>2112</td>
<td>2923</td>
</tr>
</tbody>
</table>

From these figures we arrive at an estimate of the normal income for each of the three income strata.

Table D-2. Normal Income

<table>
<thead>
<tr>
<th>Income level</th>
<th>Normal income</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - 100% of Poverty line</td>
<td>$2882</td>
</tr>
<tr>
<td>100 - 125% of Poverty line</td>
<td>$4316</td>
</tr>
<tr>
<td>125 - 150% of Poverty line</td>
<td>$5446</td>
</tr>
</tbody>
</table>

E. Observation costs

The average annual cost, \( C_{ij} \), of a family in income stratum \( i \) enrolled in tax plan \( j \) is the sum of an average operating cost and an average transfer cost,

\[
C_{ij} = \bar{F}_j + \bar{V}_{ij}, \quad i = 1, 2, 3; \quad j = 1, \ldots, 10.
\]

We have already developed an expression for \( \bar{V}_{ij} \) in connection with our earlier work response hypothesis. The average operating cost includes the initial
cost of interviewing and selecting families spread over three years plus the ongoing fixed costs of administering the payment and interviewing activities. On the basis of our experience in Trenton and in Paterson/Passaic we estimate this cost as follows:

Average annual operating cost = \( \frac{1}{3} \) (initial identification cost) + 4(average cost per quarterly interview) + (average annual cost of payments administration)

\[
\bar{F}_j = \frac{1}{3} (210) + 4(25) = $170, \quad j = 1
\]

\[
\bar{F}_j = \frac{1}{3} (210) + 4(25) + 80 = $250, \quad j = 2, \ldots, 10.
\]

Thus we have expressions for the cost components \( C_{ij} \):

\[
C_{ij} = \begin{cases} 
$170, & j=1 \\
$250 + $65, & j=2, \ldots, 10; \quad Y_i^* \geq G_j/r_j \\
$250 + \frac{(2-r_j)(G_j-r_jY_i^*)}{2(1-r_j)}, & j=2, \ldots, 10; \quad G_j/r_j > Y_i^* > G_j \\
$250 + G_j - r_j(Y_i^*/2), & j=2, \ldots, 10; \quad Y_i^* \leq G_j
\end{cases}
\]

with the further requirement \( \bar{V}_{ij} \geq $65, j=2, \ldots, 10, \) since we are paying families in the experimental plans a minimum of $2.50 each bi-weekly period in order to keep them filing income reports.

IV. Attrition

We expect that there will be attrition from the sample over three years at a rate inversely related to the size of transfer benefits which families receive. We will assume that this rate is a linear function of benefits, which
reaches a maximum of 50% in cells where the average benefit is zero and
drops to zero in cells where the average benefit is $1,000 per year. Express-
ing this relationship in terms of a retention rate, \( p_{ij} \), we have

\[
p_{ij} = \min \left\{ 1 ; .0005 \overline{V}_{ij} + 0.5 \right\} .
\]

(13)

The presence of attrition will affect both observation costs and the ef-
fective number of observations. In each case, the length of time families re-
main in the sample will be an important factor — the longer they stay the
more they will cost and the more information they will provide. We expect
that attrition is more likely to occur in the early part of the program for
several reasons.

(1) Families will develop a familiarity with the program, a lack of app-
prehension about it, and an understanding of their role in it over time, making
losses and refusals less likely.

(2) Over time, the project staff will develop techniques of tracing fam-
ilies with whom contact is lost and techniques of presenting the program to
families who are confused, fearful, or hostile.

(3) Families who, with more or less full knowledge of the program
goals and their own tax treatment, do not want to continue as participants will
probably make this decision fairly early in the program.

Accordingly, we shall assume that families who drop out do so on aver-
age after one year of participation. Our new cost equation, adjusted for at-
trition, will be

\[
C_{ij}' = p_{ij} C_{ij} + (1 - p_{ij}) \left[ \frac{1}{3} \left( 210 + (C_{ij} - 70) + 300 \right) \right]
\]

\[
= p_{ij} C_{ij} + (1 - p_{ij}) \left[ \frac{C_{ij}}{3} + 147 \right]
\]

\[
= \left( \frac{1 + 2p_{ij}}{3} \right) C_{ij} + 147(1 - p_{ij}) .
\]

(14)
which states that families who do not dropout cost an average of \( C_{ij} \) while families who do dropout cost the full initial selection cost plus one third of the ongoing administrative and benefit costs plus an amount related to efforts to maintain the sample intact. Our field activities in recent months have been heavily directed toward preventing attrition, which is very costly to us in terms of inference and outside confidence in our work. We would be willing to spend more than $300 to retain a family if it were at all productive.

We shall also assume that the amount of information which families provide us is proportional to the duration of their stay in the program.\(^{12}\) This gives us the following relationship for effective observations.

\[
K'_{ij} = p_{ij} K_{ij} + \left(1 - p_{ij}\right) \frac{K_{ij}}{3} = \left[1 + \frac{2p_{ij}}{3}\right] K_{ij} .
\] (15)

Using Eqs. (14) and (15) in conjunction with Eq. (6), we introduce an adjustment for attrition as follows

\[
K'_{ij} = \left[\frac{f_{ij} \left( \sigma^2_{e_{ij}} + \sigma^2_{u_{ij}} \right)}{\lambda C'_{ij}}\right]^{1/2}
\]

\[
\left[\frac{1 + 2p_{ij}}{3}\right] K_{ij} = \left[\frac{f_{ij} \left( \sigma^2_{e_{ij}} + \sigma^2_{u_{ij}} \right)}{\lambda \left[1 + \frac{2p_{ij}}{3}\right] C_{ij} + 147 \left(1 - p_{ij}\right)}\right]^{1/2}
\]

\[
K_{ij} = \left[\frac{f_{ij} \left( \sigma^2_{e_{ij}} + \sigma^2_{u_{ij}} \right)}{\lambda \left[1 + \frac{2p_{ij}}{3}\right] C_{ij} + \frac{1323 \left(1 - p_{ij}\right)}{\left(1 + 2p_{ij}\right)^2}}\right]^{1/2} .
\] (16)

\(^{12}\) This assumption probably overstates the eventual value of drop-out data, but has the effect of providing a margin of error by increasing the allocation to cells where attrition is expected to occur.
V. Allocation Result

The following table shows the allocation of observations $K_{ij}$ over tax treatments obtained by applying the analysis of variance model and the various parameter estimates developed in Sections I thru IV. Also shown are values for the income variances and observation costs for each cell.

The critical determinants of this allocation are, of course, the income response variances $\sigma^2_{cilj}$ and the transfer costs $V_{ij}$. These two parameters are directly related to one another — the more generous the program relative to initial income, the higher the transfer cost and wider the options for response without suffering a loss in total income. The wider the range of response the greater the variation in response, as families make independent decisions about where to position themselves in the eligible range. Within each stratum, the relationship between variance and cost produces a fairly even allocation of observations over tax plans. But the allocation among strata is heavily balanced toward Stratum III, where the average transfer cost in all plans is relatively low while the underlying income variance $\sigma^2_{ij}$ is relatively high.

Given our experience with very low eligibility rates in Trenton, Paterson, and Passaic, it is clear that we will not be able to find enough families in our projected cities to permit sample stratification to the extent required by the allocation model. We will, therefore, impose the requirement that the sample breakdown by stratum equal approximately the proportions found in nature — specifically that Stratum I contain 30% of the observations, Stratum II, 30%, and Stratum III, 40%. We are taking the tack here of imposing a practical constraint, but a move in this direction could be defended on other counts. For example, one might argue that OEO policymakers and/or the public in general are specifically interested in the behavior of poor families, and wish to assure enough of such families in the sample to allow independent estimates of their response, as well as estimates of overall response. One would then adjust the various $f_{ij}$ 's to reflect this further policy weight, yielding a sample with greater poverty representation. Alternatively, one might
Table V-1. Optimal Allocation of Families over Negative Tax Treatments

<table>
<thead>
<tr>
<th>Plan</th>
<th>Stratum I</th>
<th>Stratum II</th>
<th>Stratum III</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$K_{ij}$</td>
<td>$\sigma^2_{u_i} + \sigma^2_{e_{ij}}$</td>
<td>$C_{ij}$</td>
</tr>
<tr>
<td>(1)</td>
<td>0/0</td>
<td>.0366</td>
<td>315,844</td>
</tr>
<tr>
<td>(2)</td>
<td>.50/.30</td>
<td>.0165</td>
<td>488,119</td>
</tr>
<tr>
<td>(3)</td>
<td>.50/.50</td>
<td>.0192</td>
<td>401,108</td>
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<tr>
<td>(4)</td>
<td>.75/.30</td>
<td>.0165</td>
<td>834,244</td>
</tr>
<tr>
<td>(5)</td>
<td>.75/.50</td>
<td>.0177</td>
<td>834,244</td>
</tr>
<tr>
<td>(6)</td>
<td>.75/.70</td>
<td>.0201</td>
<td>834,244</td>
</tr>
<tr>
<td>(7)</td>
<td>1.00/.50</td>
<td>.0146</td>
<td>834,244</td>
</tr>
<tr>
<td>(8)</td>
<td>1.00/.70</td>
<td>.0153</td>
<td>834,244</td>
</tr>
<tr>
<td>(9)</td>
<td>1.25/.50</td>
<td>.0128</td>
<td>834,244</td>
</tr>
<tr>
<td>(10)</td>
<td>1.25/.70</td>
<td>.0128</td>
<td>834,244</td>
</tr>
</tbody>
</table>

|      | .1826     | .2932      | .5242       |
argue that the response magnitude of interest should be stated in relative rather than absolute dollar terms. This would also tend to shift observations from higher to lower income levels.

A final question concerns the overall constraint to be applied in converting percentages to numbers of observations. We will choose to express this constraint in terms of total sample size rather than in terms of total dollar budget. There are several reasons for this choice. In the first place, no budget limit has ever been set on the project, except through sample size considerations. OEO's financial commitment has from the outset been defined in terms of support for a sample of approximately 1,000 observations, which its preliminary calculations showed to be adequate for the desired work incentive analysis. On the other hand, our survey experience has indicated that we face a binding constraint on sample size in our three metropolitan areas. Even with the most intensive of sampling procedures, we cannot hope to draw many more than 1,000 families into the program without expanding our number of sites. Finally, there is reason to believe that, although the relative transfer costs estimated above may be reasonable for purposes of sample allocation, the absolute dollar costs, reflecting as they do a very conservative assumption about work response, are probably much overstated. Thus, the lack of an official budget constraint, the shortage of eligible families, plus the uncertainty about absolute dollar costs per observation lead us to apply an overall constraint in terms of number of enrolled families, which we shall set at 1200 to allow for anticipated attrition. This yields the final result (Table V-2):

(3) The draft of this paper came in for only limited discussion in the first few weeks after it appeared. Wisconsin was busy developing its own design model, featuring a regression equation to specify response over the policy space. This regression approach had previously been explored by Wisconsin in a preliminary way, without much success, and had been recommended by Guy Orcutt and Alice Orcutt in their valuable article on income maintenance experimentation. 13

In their article, the Orcutts favored a regression model for precision of parameter estimation and generality of results. On precision:
Table V-2. Optimal Allocation of Families over Negative Tax Treatments: Constrained

<table>
<thead>
<tr>
<th>Plan</th>
<th>Number of Families</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Stratum I</td>
<td>Stratum II</td>
<td>Stratum III</td>
<td>Total</td>
<td></td>
</tr>
<tr>
<td>(1)</td>
<td>0/0</td>
<td>72</td>
<td>62</td>
<td>76</td>
<td>210</td>
</tr>
<tr>
<td>(2)</td>
<td>.50/.30</td>
<td>32</td>
<td>30</td>
<td>52</td>
<td>114</td>
</tr>
<tr>
<td>(3)</td>
<td>.50/.50</td>
<td>38</td>
<td>57</td>
<td>70</td>
<td>165</td>
</tr>
<tr>
<td>(4)</td>
<td>.75/.30</td>
<td>33</td>
<td>24</td>
<td>28</td>
<td>85</td>
</tr>
<tr>
<td>(5)</td>
<td>.75/.50</td>
<td>35</td>
<td>27</td>
<td>52</td>
<td>114</td>
</tr>
<tr>
<td>(6)</td>
<td>.75/.70</td>
<td>40</td>
<td>57</td>
<td>70</td>
<td>167</td>
</tr>
<tr>
<td>(7)</td>
<td>1.00/.50</td>
<td>29</td>
<td>26</td>
<td>27</td>
<td>82</td>
</tr>
<tr>
<td>(8)</td>
<td>1.00/.70</td>
<td>30</td>
<td>27</td>
<td>51</td>
<td>108</td>
</tr>
<tr>
<td>(9)</td>
<td>1.25/.50</td>
<td>25</td>
<td>24</td>
<td>26</td>
<td>75</td>
</tr>
<tr>
<td>(10)</td>
<td>1.25/.70</td>
<td>26</td>
<td>26</td>
<td>28</td>
<td>180</td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td>360</td>
<td>360</td>
<td>480</td>
<td>1200</td>
</tr>
</tbody>
</table>

"A parsimonious use of parameters is important because it becomes impossible to estimate parameters with precision as their number approaches the number of experimental units .... This does not mean that the usual analyses of variance approach should be discarded. However, a microanalytic multiple regression approach is almost certainly going to be attractive, and should be provided for. In such an approach, primary interest will center on the accuracy with which the influence of $\alpha$ and $\beta$ (tax law parameters) can be determined; not on the accuracy with which individual $Y_{tii}$ (earned income during time period, $t$, of tax unit, $i$) can be provided."\textsuperscript{14}

\textsuperscript{13} (footnote of page 196)

\textsuperscript{14} Ibid., p. 764 (parentheses mine).
On generality:

"Use of a regression approach based on data from micro units would permit estimating relations which were intended to apply to micro units, and, by doing so, facilitate generalizing from results based on particular samples to the results which might be expected for other populations and other specific negative income tax laws. Such multiple regression equations, once estimated, could be used to predict the response of each member of a new population to a specific tax law. Appropriate values for the tax law parameters and appropriate values for those variables needed to describe the members' individual and environmental characteristics would simply be plugged into the regression equations. Having predicted the response of each member of a new population, or at least of a random sample of the members of a new population, predictions about the aggregative implications of the new tax could be obtained by summation of the micro predictions."\(^{15}\)

This would be an ambitious effort. Assuming that a regression relationship could be estimated using microeconomic data which explained a large part of the variation in earned income after tax, it would be as risky to apply this relationship to a new and different population on which it had not been estimated as to apply it to negative tax plans with parameter values outside the original policy space. Although the Orcutt's made a number of other interesting design proposals,\(^{16}\) they did not utilize their regression model approach

---


\(^{16}\) Some of their suggestions:

i. a different parametrization of linear negative tax plans with the tax rate variable specified in the usual way but the benefit level variable specified as the amount of transfer received at initial (before start of program) earned income.

ii. a sequential sampling procedure to determine something about response before making a final decision on sample size.

iii. a staggered duration of the program to see if response depends on length of treatment, and if so, how such dependence can be allowed for in the analysis.

iv. a participant group for the study consisting of a "probability sample of tax units drawn from the entire U. S. population."

v. economizing on observations by using a single set of untreated units to control for a whole range of experimental treatments, including but not limited to variations in rate and level parameters.

None of these proposals was adopted in New Jersey.
to make any recommendations about sample size or allocation over treatments.

It remained for Wisconsin, specifically Harold Watts and John Conlisk, to make this extension. They presented their refined model17 late in January 1969, just before allocation in Paterson and Passaic. Their approach was also to minimize a weighted sum of response variances subject to a cost constraint. But their response behavior was specified by a quadratic function of the form

\[ Z = \beta_1 + D(\beta_2 U + \beta_3 U^2)g + D(\beta_4 U + \beta_5 U^2)r + D(\beta_6 U + \beta_7 U^2)g^2 + D(\beta_8 U + \beta_9 U^2)r^2 + D(\beta_{10} U + \beta_{11} U^2)gr + \text{error}, \]

where \( Z \) = ratio of actual earnings \( E \) (i.e., observed earnings after treatment) to normal earnings \( E^* \) (i.e., expected earnings in absence of treatment)

\( g \) = ratio of negative tax guarantee to full poverty standard

\( r \) = negative tax rate

\( w \) = ratio of normal earnings to full poverty standard

\( U = [M(g,r) - w]/M(g,r) \), where \( M(g,r) \) is the maximum value of \( w \) for which the negative tax is assumed to have any effect

\[ D = \begin{cases} 1, & U \geq 0 \\ 0, & U < 0 \end{cases}. \]

In other words, response was expressed purely in ratio variables as a smooth function of the negative tax guarantee and rate, and the tax unit's initial income. Initial income was assumed to capture the effect of all personal and environmental variables which would influence income determination after negative tax.

The upper bound for negative tax response was set as follows:

\[(2) \quad M(g, r) = \frac{(1.3 + r) g}{1 + r}.\]

Admissible values for \(g\) and \(r\) were the parameters of the nine tax plans in the expanded-policy space, while \(w\) took on four values (.4, .8, 1.15, 1.4) representing the mean normal income ratio in four income strata between zero and 150% of the poverty standard.

Then, in a departure from previous discussions, they asserted that the magnitudes to be predicted by the study were not the observed work patterns of tax units in each tax plan but the "national cost per family of enacting (each of the) ... nine non-zero tax plans ..., minus what this cost would be if the tax had no effect on work behavior — that is, the national cost per family due to reduced work effort." To obtain the appropriate objective function, they considered a family of size \(n\) with assigned tax parameters \(g\) and \(r\) and after tax earnings below breakeven, where \(G(n) = \) the poverty standard for a family of size \(n\).

\[
\text{tax payment cost under plan) = } gG(n) - rE = gG(n) - rZ(g, r, w) E^* \quad \text{since } Z = E/E^*
\]

\[
\text{tax payment cost under plan if } E \text{ remained at } E^* \)
\[
= gG(n) - rE^*
\]

so the cost due to reduced work effort for this family was

\[
(3) \quad c(g, r) = rE^*[1 - Z(g, r, w)]
\]

and the average cost over all families was

\[
(4) \quad C(g, r) = \sum_i \sum_j rE^*_i [1 - Z(g, r, w)] f(E^*_i, n_j) h(g, r, E^*_i, n_j),
\]
where \( f(E^*_i, n_j) \) = joint frequency distribution of all families by normal earnings and family size (from U. S. Census figures).

\[ h(g, r, E^*_i, n_j) = \text{fraction of families with normal earnings } E^*_i \text{ and family size } n_j \text{ assumed to lie below the breakeven level}. \]

The form chosen for the latter function was as follows:

\[ h(g, r, E^*, n) \]

\[ g \leq \frac{0.9gG(n)/r}{M(g, r)G(n)} \]

(90% of breakeven) (max \( E^* \) level at which tax has work effect)

Plugging functional form (1) into Eq. (4) yielded the desired objective function as a linear expression in the response coefficients \( \beta \). The variance of this function could then be minimized with respect to estimates of the \( \beta \)'s, subject to a budget constraint. As can be seen from Eq. (4), the response variances associated with different plans, assumed to be a constant \( \sigma^2 \), were weighted by the tax rate, the population frequency, and some fraction to adjust for non-costly (i.e., above breakeven) response. In addition, a final set of weights, shown before normalization below, were applied to the variances to reflect differential policy interest in different plans. These weights were arrived at by joint discussions between Wisconsin and MATHEMATICA.
Finally, an overall budget constraint was applied

$$\sum_{i=1}^{40} \overline{C_i} n_i \leq 1,200,000$$,

where $n_i \geq 0 =$ number of observations in income stratum/tax treatment group $i$

$$\overline{C_i} = u_i C_i + \left(1 - u_i\right) C_i / 3 \quad \text{average annual cost of observations in group } i.$$

The $u_i$ factor represented the proportion of observations in group $i$ that was expected to remain in the program for the full three years, and was specified as follows.

![Graph showing expected annual tax payment in group i as a ratio to the poverty standard](image)

Observations which dropped out at any point during the three years were assumed to be of no value to the model, so the "effective number of observations" in group $i$ equaled $u_i n_i$, and depended on the average payment in the group and the family size distribution.

Two hypotheses were used to generate two alternative sets of costs, $c_i$. The first, the zero work response reaction, assumed that people took all the benefits of negative tax in the form of more income, and did not alter their work effort. The second, the target income reaction, assumed that people took all their benefits in the form of increased leisure, and reduced their earnings to the point where their total income equaled their previous earned income. To the transfer costs associated with these hypotheses, a fixed
administrative cost of $250 per year was added in all groups to yield total full-year cost per family.

The optimization process then yielded two specific allocation of families over treatment groups, one for each set of postulated costs. Both allocations were quite similar, and the one for the zero work response reaction is shown below. It should be noted that the model as specified was not equipped to determine control group size, which was set by flat to be approximately 20% of the total sample and allocated equally over the four income strata.

Optimal Allocation of Families over Negative Tax Treatment Basic Watts/Conlisk Specification (1/16/69) with Zero Work Response Reaction

<table>
<thead>
<tr>
<th>Tax Plan</th>
<th>Income Stratum</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>g=0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>250</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.9</td>
</tr>
<tr>
<td></td>
<td></td>
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<td>-</td>
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<tr>
<td></td>
<td></td>
<td>-</td>
</tr>
<tr>
<td></td>
<td></td>
<td>2626</td>
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<td></td>
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<td></td>
<td></td>
<td>3451</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td></td>
<td>10.8</td>
</tr>
</tbody>
</table>

**NOTE:** Top number in each cell is \( n_i \) in percentage terms. Bottom number is \( C_i \) in dollars for family of size four.
(4) Wisconsin presented this memorandum in Princeton on the day that the research staffs gathered there to carry out the allocation of families in Paterson/Passaic. Although each day's delay was certain to be costly, MATHEMATICA was so unfamiliar with the model's workings and so concerned about its highly unbalanced outcome (95% of all observations concentrated in 14 cells out of 40, only 10% of negative tax recipients drawn from poverty strata), that they asked time to consider it and to discuss modifications in it before proceeding. Wisconsin agreed, although they were seriously interested in adopting the model as it stood, noting calmly that the fact that most of the design points (treatment cells) were rejected by the model was not surprising since the Z function had only eleven parameters.

The rest of this section will outline some of my concerns with the model, a number of which were shared at the time by others at MATHEMATICA. The arguments will be ordered according to the model component to which they pertain.

(a) The regression function Z: Watts and Cnlsik indicated that the form of the Z function was chosen to have the following properties:

\[ Z \rightarrow \text{constant} \quad \text{as} \quad g, r \rightarrow 0 \quad \text{and/or} \quad w \rightarrow M(g, r) \]

\[ \frac{\partial Z}{\partial g}, \frac{\partial Z}{\partial r}, \frac{\partial Z}{\partial w} \rightarrow 0 \quad \text{as} \quad w \rightarrow M(g, r) \]

(the last condition was not satisfied). It was further chosen, they said, to have "good curvature potential." The only arguments made in its behalf were at the most general level — that there must certainly be some continuity in response and that response must taper off to nothing at some income level.

On the question of continuity, the quadratic Z was defended as "regular." In fact, it was a highly restricted formulation of expected behavior. A completely general specification would have required that 36 parameters be
estimated — a parameter for each of the eligible design points. One could have imposed considerable smoothness on the response function without restricting it down to 11 parameters. For example, one could have allowed for the possibility of inflection points which are so common in social science relationships.

In addition to the question of how many parameters, was the question of which parameters, and this is where the tapering off argument made itself felt. The compound variables entering into the Z function were chosen to force response to zero at selected points M. That is, the specification of response over the income and tax plan ranges of policy interest was dominated by a desire to anchor the response at (or beyond) one end of the income range rather than by any notion of the contours of response over the actual area of interest. This was defended as necessary to "turn the model off" from interest in high-income observations, but this explanation is dubious since the model was not allowed to consider observations above 150% of the poverty line anyway. In fact, the response ceilings M(g, r) were set just high enough that they didn't rule out response in any of the treatment cells.

(b) The response ceiling, M(g, r): Wisconsin had long been interested in the behavior of families around and somewhat above breakeven lines, because of the large numbers of such families in the national population and because of some special uncertainty surrounding their response. This interest was manifested in the design model in the form of values for M(g, r) well above breakeven lines, as shown below:

<table>
<thead>
<tr>
<th>Tax rate</th>
<th>Response ceiling</th>
<th>Breakeven point</th>
</tr>
</thead>
<tbody>
<tr>
<td>r = .3</td>
<td>M(g, r) = 4 g</td>
<td>B(g, r) = g/.3</td>
</tr>
<tr>
<td>r = .5</td>
<td>M(g, r) = 3 g</td>
<td>B(g, r) = g/.5</td>
</tr>
<tr>
<td>r = .7</td>
<td>M(g, r) = 2.5g</td>
<td>B(g, r) = g/.7</td>
</tr>
</tbody>
</table>

B(g, r) = 1.2 B(g, r)
M(g, r) = 1.5 B(g, r)
M(g, r) = 1.75B(g, r)

These values for M seemed quite high in view of our first six months' experience in Trenton, where no evidence of disincentive response had been detected
among families initially above breakeven. The ranges between \( M \) and \( B \) provided the model with a particularly valuable set of people on whom the test negative tax — they responded but were virtually costless. And from their behavior could be estimated the behavior of other families through appeal to \( Z \). Thus, the model assigned approximately 1/3 of its experimental observations to the several treatment cells which lay wholly or in part above break-even levels.

The exact positioning of \( M \) became a matter of considerable controversy. There was much doubt at MATHEMATICA that families initially above \( B \) were as revealing about response as the model assumed — partly because of concern that refusals and attrition, which had been significant for over-break-even families in Trenton, could greatly bias results at these levels, and partly because of skepticism that the function \( Z \), if it applied at all, could be extended perfectly smoothly from the range of costly response into an area of costless (in terms of benefits) response. For its part, Wisconsin was extremely resistant to the lowering of \( M \).

- (c) The conversion factor, \( h(g,r,E^*,n) \): This weighting factor in the objective function was needed to convert overall response into costly response, the assumed magnitude of interest. As specified, it was a simple rule of thumb used to approximate the net value of two quantities — the portion of the average response of families in any cell which lay below breakeven, and the scatter of families around the average response (which might, for example, entail some cost even in cells where the average response did not bring families below breakeven). This was, of course, a further assumption about response made independently of the basic response assumptions incorporated in \( Z \). A full specification could have been made by breaking down the cost expression \( C(g,r) \) into three pieces:

(i) for families with both normal earnings \( E^* \) and actual earnings \( E \) below breakeven \( B \), the cost would be \( r(E^* - E) \) which might be positive or negative.

(ii) for families with \( E^* > B \) and \( E < B \), the cost would be \( r(B - E) \) which would be positive but less than \( r(E^* - E) \).
(iii) for families with $E^* < B$ and $E > B$, the cost would be $r(E - B)$ which would be negative.

However, Conlisk chose to work with $r(E^* - E)$ alone, modifying it by the function $h$, because the more fully specified problem would be nonlinear in the $\beta$ parameters (since it involved a switching mechanism among the cost pieces depending on the $Z$ response) and he was not prepared to handle this. As was the case with the $M$ function, it was difficult to reach any consensus as to the goodness of the $h$ approximation.

Need for the $h$ function was introduced by Wisconsin's interest in above break-even response plus its attention to the transfer cost of response as the magnitude of policy interest. There were concerns about the latter as well as the former. In its proposal, MATHEMATICA had stated the primary purpose of the experiment as the estimation of negative tax effects on work effort. The focus was to be on withdrawal of work effort, which was costly in terms of both lost product and transfer payment. At OEO's request, MATHEMATICA and Wisconsin had both added sociologists to their research teams, in order to address important and secondary issues of individual goals and attitudes, family organization and functioning, community awareness and involvement, etc.

There were also secondary issues for economic research — family consumption patterns, savings and credit behavior, mobility, etc. Interest in these issues was not linked to the value of the negative tax rate. MATHEMATICA felt that Wisconsin's dollar-cost objective function, in which tax rate weights were applied to work response, did not allow adequately for the primary or the secondary objectives of the study. So, there was opposition both to the form of the $h$ function and to the basic specification which gave rise to the need for it.

(d) **The cost figures, $C_i$:** The two sets of cost values were chosen to bracket what seemed to be a reasonable range of response, but were determined independently of the basic response function $Z$ and were, in general, inconsistent with it. For example, under the zero work response
assumption used to generate one set of costs, the $C(g, r)$ function would have been identically zero, and there would have been nothing to estimate. Conlisk acknowledged this, but said it did not matter since the optimal allocation was quite insensitive to the cost specification.

Indeed the two allocations shown in his memorandum were quite similar. However, this was not quite the sensitivity test that it was reported to be since the two sets of costs at issue differed considerably in absolute value but not much in relative value. In each case, a single response relationship had been applied over all income ranges, resulting in roughly equiproporionate changes in costs. Relative costs associated with different design points actually played an important role in the allocation outcome.

The cost inconsistencies appeared to enhance the concentration of families in above break-even cells by understating their already low cost. Under both the zero work and target income response hypotheses, families in above breakeven cells were assigned a cost of $250/year, the fixed administrative fee. Yet observations in these cells were only of interest to the model because, through the operation of Z and h, they were thought to have costly response — that is, they were assumed eligible for transfer payments.

Finally, the average cost function $\bar{C}_i = u_i C_i + (1 - u_i) C_i/3$ did not allow for the substantial costs associated with attempting to retain in the sample those families who were eager to drop out or indifferent toward staying in. Finding wastage cheap — a drop-out cost only 1/3 as much as a regular observation — the model dealt with attrition by simply beefing up assignments to low-cost cells. In fact, each of these non-observations was very valuable, both because of the limitation on total available families and the damage done by self-selection out of the sample. This value was reflected in sizeable dollar outlays spent to contact (find) families and encourage their continued participation in (return to) the program. Such outlays were an important part of the average cost of low-benefit cells, and inclusion of them in the average cost relationship would have remedied a further understatement of the true costs in those cells.
(c) *Homoscedastic error variance of the Z function*, \( \sigma^2 \): Wisconsin made the same assumption about range of response to negative tax treatment that I had made earlier. But even though the range varied all the way from $0 to $4125 (for a family of four), they assumed that there would be equal variation in response in all cells. The main point of my paper had been to suggest that variance was directly related to range, and therefore, to cost. Families confronted with a wide range of disincentive options which would still leave them better off financially than before were likely to show greater diversity of response than families who could not make any disincentive adjustment without losing income. Treatments paying $2,000 at initial income were likely to spark more diversified response than treatments paying nothing. In that case, more families would be needed in broad response cells than in narrow response cells to estimate work reaction with equal precision, thereby partially offsetting the very powerful role played by costs in steering allocation away from expensive (broad response) cells.

Finally, there were concerns about the attrition and budget assumptions. The attrition factor had already been exceeded in six months in Trenton. The dollar budget ceiling was a Wisconsin construct which had no official (contractual) standing and constituted one more assumption of unknown validity.

As can be seen from the above, the model encompassed four separate hypotheses about response to negative tax treatment:

\[ Z \quad \text{the basic response function} \]
\[ h \quad \text{the proportion of response taking place under break-even} \]
\[ \overline{C_i} \quad \text{the average annual cost of families in treatment cell } i \]
\[ u_{ij} \quad \text{the proportion of families in cell } i \text{ who remain in the program for three years} \]

Each of these assumptions was independent of the others, and some were clearly inconsistent. Reasonable men could differ on the validity of all of them. When put together, they produced a highly unbalanced allocation, with
observations crowded along the two cheap edges of the design space and wide interior spaces left completely unpopulated.

I did not see how the model could be accepted without some economic argument or evidence in support of the specific functional forms incorporated in it. I felt that an indispensable burden of proof lay with those who claimed to know something about the contours of response. This kind of support would be necessary in any experimental design work, but it was especially critical in the present case where the behavior at issue was so little known, so controversial and so important for policy.

But no such support was forthcoming. Indeed it was generally recognized to be unavailable — its lack having occasioned the experiment in the first place. Thus, the door was opened to lengthy negotiation where substantive knowledge played only a partial role in determining the outcome. In an ironic way, the design issue became a mirror of the national debate on work incentives, where adherents to different positions voiced their beliefs but adequate evidence to judge among them was not at hand.

(5) Lacking a consensus on how to proceed with allocation, and facing an urgent need to assign families in Paterson/Passaic, MATHEMATICA and Wisconsin agreed to meet at O'Hare Airport the week after the latter's memo was presented to discuss modifications in the model. Several adjustments were decided upon in advance.

(i) Consolidating the four income strata into three, characterized by w values of .7, 1.15, and 1.4.

(ii) Increasing the $C_i$ costs to pertain to families of size six rather than size four.

(iii) Averaging the two cost figures for each design point to achieve a single $C_i$ specification.

(iv) Raising the attrition rate for the control group to .5 and adjusting the rate for the experimental group to the following:
(v) Altering the fixed component of the $C_1$ costs from $250$ to $105$ for control families and $185$ for experimental families.

(vi) Shifting the raw policy weights to the following:

(vii) Constructing population weights from census data on male-headed families rather than all families.

(viii) Changing the control group allocation from an even spread over income strata to proportions in each stratum equal to the stratum proportions in the overall experimental group.

Using these adjustments, John Conlisk made eight further runs of the Wisconsin model incorporating various sets of additional constraints. He presented the results of these runs at the O'Hare meeting. After a preliminary debate, majority agreement was reached to confine discussion to ad hoc modifications of a particular run featuring three constraints.
(ix) At least five percent of the total sample in each tax plan.  

(x) Sample proportions in the three income strata as follows:

\[ \begin{align*}
    w &= .7 & .2 \\
    w &= 1.15 & .3 \\
    w &= 1.4 & .5 \\
\end{align*} \]

(xi) Budget share of the two 125% guarantee plans no greater than 1/3.

At the end of the day’s negotiations, the following allocation emerged.

O’Hare Optimal Allocation Outcome

(Number of families)

<table>
<thead>
<tr>
<th>Tax plan</th>
<th>Income stratum</th>
</tr>
</thead>
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<td></td>
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<td>1.0</td>
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<tr>
<td>1.25</td>
<td>.5</td>
</tr>
<tr>
<td>Total</td>
<td>298</td>
</tr>
</tbody>
</table>

\[^{18}\] In the result presented, this constraint was not satisfied for three of the nine tax plans.
The major departure here was the elimination of the 125% guarantee — 70% tax rate plan. In return for giving up this ninth plan, which Wisconsin pointed out was rejected by the model as not a very informative set of design points, MATHEMATICA was allowed to move observations of total cost equal to that saved by ruling out plan nine into remaining points in the design space. It took this opportunity to assign families to five unpopulated cells, and similarly moved to populate a sixth cell by shifting dollar equivalents out of an adjacent cell containing several hundred observations. For the most part, these shifts were used to fill in the middle income stratum which had been zeroed out by the model except for the control group and two above-breaheven cells.

These tradeoffs reflected a position which Wisconsin adopted at the beginning of the discussion — that all proposed changes in the allocation outcome should be judged by their impact on the magnitude of the model's objective function and that changes which raised that magnitude markedly could not be accepted. This insistence on evaluating modifications by appeal to the model which was itself at issue disturbed some participants but nonetheless prevailed by and large.

However, the decision to adopt the foregoing allocation was not unanimous and, upon returning to Princeton, MATHEMATICA attempted to reopen the discussion. 537 families or 57% of the experiment group were still

19 The low interest attached by most members of the research staffs to plan nine had already been formalized in the low policy weight assigned to it in the model. I did not agree with this low weight and was opposed to eliminating the plan entirely. On a policy level, I was definitely interested in efficient transfer structures that would provide a substantial level of support and reduce payments rapidly as income rose. On a design level, I thought plan nine was a valuable design point holding down a very important corner of the policy space. [It is interesting to note that with all the work incentive fears of experts and laymen, the National Welfare Rights Organization, a group of welfare clients, has recently prepared income maintenance legislation which comes closer to plan nine than any other plan in the policy space.]
concentrated in the five treatment cells at or above breakeven. Arguments that low payments could anger the black communities in Paterson/Passaic and jeopardize the experiment were successful in getting a dozen observations upgraded to treatments that would pay some benefit at enrollment. In addition, MATHEMATICA declined to enroll 27 families until some of them were reassigned to more generous treatments. These efforts managed to get the proportion of experimental families receiving no negative tax payment at time of enrollment in Paterson/Passaic down to 40%.

(6) There was little satisfaction in any quarter with this compromise solution, and plans were made to lay the basis for a final decision before allocation in Jersey City. The accepted approach was to conduct further experiments with the Wisconsin model.

To this end, John Conlisk produced a third memo containing the results of nine new runs. The runs differed as to the number and type of constraints placed on the basic model, and the basic model was itself changed in two ways. First, the response function $Z$ was expressed in terms of $U^2$ and $U^3$ rather than $U$ and $U^2$. Second, control group families were no longer assumed to exhibit the behavior $E = E^*$, so that the average cost function became:

$$ C(g, r) = \sum_i \sum_j rE_i^* [Z(0, 0, w) - Z(g, r, w)] f(E_i^*, n_j) h(g, r, E_i^*, n_j) , $$

where $Z(0, 0, w) \neq 1$.

The $Z$ function change, which altered the nature but not the number of response parameters to be estimated, caused modest changes in the allocation outcome. But the new control group specification changed the allocation radically, with the unconstrained model assigning over 70% of all families to this non-treatment group.

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Wisconsin indicated that it had made the new response assumption for the control group in order to reduce the allocation weight given that group by eliminating the parameter $\beta_1$ (presumably near one, since $Z = E/E^* = \beta_1$ when $q=r=0$ or $w = M(e, r)$) from the objective function. The effect was the opposite, however, since there was now equal variance in the behavior of control observations as in the behavior of treated observations, and this variance entered into the total variance of the payment cost associated with work reduction for each treatment cell.

Wisconsin believed that allowance for variability in the behavior of control families substantially improved the model. This was probably true, except for the particular interpretation put on the model's result. In order to measure differential response at any design point (i.e., the difference between behavior at that point and control behavior) the model needed observations at both the design point and the relevant control point. Each new non-zero design point required its particular quota of control observations, but nothing in the nature of the optimization required that these be different observations from those used to control for other design points. Wisconsin chose to program the model to add up the control observations associated with each of the 27 design points, thereby effectively generating a separate control group for each separate treatment cell. This produced very large overall control groups, which in none of the various specifications fell below 60% of the total sample unless directly constrained.

At the end of his memo, in a presentation of sensitivity tests on the model, Conlisk developed a decision-theoretic approach to judging allocation strategies which was to be the basis for most later discussions. He considered six different "states of nature" — i.e., assumptions about the true response—and eight different "actions" — i.e., design specifications.

**States of Nature** $(q)$:

1. Wisconsin model with $Z$ a function of $U^2$ and $U^3$. [Basic model.]
2. Wisconsin model with $Z$ a function of $U$ and $U^2$. [$U, U^2$ model.]
3. Wisconsin model with $Z$ a function of $U^2$ alone. [$U^2$ model.]
(4) "Basic model" with response ceiling set at breakeven \( M(g, r) = g/r \).
(5) Basic model with modest unequal error variance. [Standard deviation of the Z function error term varying positively with expected negative tax payment from \( \sigma \) for the control group to 5/4 \( \sigma \) for the largest expected payment group.]
(6) Analysis of variance model similar to that in my January memo, but with equal error variance.

**Actions (p):**

(1) Equal number of families at each design point.
(2) Optimal allocation for the basic model.
(3) Optimal allocation for the \( U, U^2 \) model.
(4) Optimal allocation for the \( U^2 \) model.
(5) Optimal allocation for the \( M(g, r) = g/r \) model.
(6) Optimal allocation for the unequal error variance model.
(7) Optimal allocation for the analysis of variance model.
(8) Compromise design — Optimal allocation for the basic model subject to a minimum of 17 observations for each design point.

He then determined the value of the objective function which would obtain if action \( p \) were adopted when the true state of nature was actually \( q \), for all combinations of \( p \) and \( q \). For each combination, he constructed a standardized loss measure of the damage done by choosing the wrong action. It happened that each action had quite sizeable losses for some states of nature, and that no action dominated the others in the sense that it showed the smallest loss for all states of nature.

To choose the best action in this situation, he turned to two common rules in decision theory:

(i) the minimax criterion — choose the action whose maximum loss over all states of nature is smallest

(ii) the minimum expected loss criterion — assign subjective probabilities to the various states of nature, calculate expected losses for each action, and choose the action with the smallest expected loss.
On both criteria, the analysis of variance design and the compromise design performed considerably better than the other specifications. Under the minimax criterion, the analysis of variance design rated best with the compromise design close behind, and vice versa on the minimum expected loss criterion.

An exchange of memoranda ensued requesting and reporting results of tests with additional states of the world and actions. In particular, Harold Watts wrote two memoranda in March and April elaborating on the design theoretic approach. To a degree, the debate about the true response contours was renewed in differences of opinion about Bayesian priors to be attached to various states of the world. The role of these priors was important enough to produce very different minimum expected loss choices depending on the values assigned. Using probabilities representing his judgment, Watts concluded in his April 4, 1969 memorandum\(^\text{21}\) that discussion should be confined to the basic and the \(U, U^2\) models, making at most minor adjustments to these specifications.

Watts' analysis attached 90% of the true state of nature probabilities to three closely allied basic and \(U, U^2\) models, leaving 10% for the analysis of variance model. Conlisk's earlier work had also attached 10% probability to the analysis of variance case, but had spread the other 90% points over a wider range of design alternatives. The greater the spectrum of states of the world considered, the better the performance of the analysis of variance model which hedged completely over all states. This robustness to misspecification was the value of the analysis of variance model, not any belief that the model actually represented the true state of the world.

Even with the extensive design work done by Wisconsin, only a modest subset of plausible states of the world had been formally introduced into the

analysis. But the weight of even this evidence led me to conclude, as it did a number of others at MATHEMATICA, that the costs of misspecification substantially exceeded the costs of inefficiency in choosing an allocation model for the negative tax experiment. This judgment, plus a belief that attention to very important secondary objectives of the research required a more balanced allocation than those yielded by various regression models of transfer cost of work response, moved MATHEMATICA to recommend an analysis of variance approach or some compromise solution which would yield a comparably balanced allocation.

(7) With the date for Jersey City enrollment fast approaching, and still major differences of position on the optimal allocation strategy, MATHEMATICA and Wisconsin agreed to refer the allocation question to Professor James Tobin for an independent and binding judgment. Professor Tobin agreed to undertake the task and in late May, after extensive consideration of the design issues, recommended the following assignment of remaining families (Table 7A).

Tobin based his allocation on a variant of Wisconsin's design model incorporating the following features:

(i) a Z function expressed in terms of U and $U^2$.

(ii) a modified cost specification which weighted the zero work and zero income response hypotheses by $[1 - (1/2) r]$ and $1/2 r$, respectively, rather than simply averaging them. [This assumed a milder work disincentive response than previously and lowered the variable cost associated with each design point.]

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(iii) an additional fixed cost component of $240 per year for the experimental group and $100 per year for the control group to be paid out as deemed appropriate to prevent attrition from the sample.

(iv) a reduced rate of attrition in all cells as a result of the retention payments, reaching a maximum of 20 percent for observations receiving no negative tax payment.

(v) a budget ceiling of $1,450,000 per year, a sample size limit of 1,300 observations; and the standard policy weights, population frequencies, and constraints on stratum totals (.3, .3, .4) and budget share going to the 125 percent guarantee plan (1/3).

(vi) error variance of response twice as high in cells where initial income was (1) below the guarantee level, or (2) above the breakeven level as in other cells. This to allow for the

<table>
<thead>
<tr>
<th>Tax Plan</th>
<th>g</th>
<th>r</th>
<th>Income Stratum</th>
<th>Stratum I</th>
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Table 7A. Tobin Allocation of Outstanding Sample Households

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</tbody>
</table>

(iii) an additional fixed cost component of $240 per year for the experimental group and $100 per year for the control group to be paid out as deemed appropriate to prevent attrition from the sample.

(iv) a reduced rate of attrition in all cells as a result of the retention payments, reaching a maximum of 20 percent for observations receiving no negative tax payment.

(v) a budget ceiling of $1,450,000 per year, a sample size limit of 1,300 observations, and the standard policy weights, population frequencies, and constraints on stratum totals (.3, .3, .4) and budget share going to the 125 percent guarantee plan (1/3).

(vi) error variance of response twice as high in cells where initial income was (1) below the guarantee level, or (2) above the breakeven level as in other cells. This to allow for the
possibility of very large response from those who (1) cease work and accept the guarantee, or (2) make a drastic shift toward leisure by withdrawing the labor of one family member. (vii) assumption that exogenous non-experimental effects on earnings (but not differences among responses to different treatments) were city specific, so that the constant term in the Z function varied from city to city. This prompted a recommendation that survey work be reinstituted in Trenton, Paterson, and Passaic to identify and enroll 150 additional control households.

From the allocation calculated according to this model variant, Tobin made "a number of judgmental deviations." For one, he shifted families among cells "to be sure that there were enough observations of programs of high policy interest value so that estimates of their responses and costs would not depend too exclusively on interpolation." Secondly, he increased high payment observations above the number suggested by the model "to provide a sufficient spread of observations to meet the informational requirements of studies other than the central estimate of work response and associated costs, i.e., sociological variables and administrative experience." He figured the cost of these deviations from the calculated optimum to be about a 3 percent to 4 percent increase in the variance of the desired estimate, noting that this loss of accuracy was "a true loss of course only on the assumption that the model used in the calculation is correct and that the purposes of the experiment are completely captured in the objective function used in the calculation. A better way to put it is to say that these "losses" are in part insurance against the possibility that some other model specification is

25 Ibid., p. 10.
better and in part a trade-off in favor of some other objectives of the project.\footnote{Ibid., p. 11.} In the same vein, he noted further along that "there is a certain arbitrariness to any particular parametric specification. Among the available sample points it will pick at a minimum enough to match the number of parameters to be estimated, and at a maximum this number plus the number of effective constraints. Thus, there will, in general, be several cells to which no observations are assigned, 10 to 16 of the 27 cells with an 11-parameter function and the various constraints on costs and sample sizes. The trouble is that the identity of the empty cells changes with the specification, and we are not sure about which one is correct. That is why there is some safety in spreading observations more widely than any particular specification would."\footnote{Ibid., p. 18.}

While hedging in this way, Tobin specifically rejected an analysis of variance model saying he felt certain that there was continuity in behavior and that observations in one cell contained some information about behavior in adjacent cells. Nevertheless, his adjustments to the Wisconsin model moved substantially in the direction of recognizing MATHEMATICA's interests and concerns and although one might have perhaps preferred adoption of some un-tampered model like "anova" whose properties were well known, the compromise Tobin outcome was readily accepted by all parties.

Table 7B shows the final allocation of sample observations over all tax plans, all income strata and all cities. A prominent feature of this allocation was, of course, its large control group and the corresponding requirement that MATHEMATICA go back for more control families in the three already-assigned cities. In speaking to this feature, Tobin noted that control observations were exceptionally valuable in providing low-cost information about the function $Z$ that could contribute to estimating the value of $Z$ at positive levels of $g$ and $r$. Citing the importance of disentangling treatment effects from
underlying income changes, and the need for fully-comparable experimental data on both the latter and the former, he concluded that "nothing could be worse for the credibility of the conclusions of the project than vulnerability to the criticism that too much or too little response was estimated through failure to pin down other sources of income change"\textsuperscript{28} by use of an adequate control group. On such general statements, the argument rested.

\textsuperscript{28} Ibid., p. 13.
Table 7B. Final Allocation of Sample Observations in the Negative Tax Experiment

<table>
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<tr>
<th>Tax Plan</th>
<th>Stratum I</th>
<th>Stratum II</th>
<th>Stratum III</th>
<th>Total</th>
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<td>Jersey City</td>
<td>Scranton</td>
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<tr>
<td>g r</td>
<td></td>
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<td>18</td>
</tr>
<tr>
<td>Total</td>
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</tbody>
</table>
Chapter VI
SUMMARY: SPRING 1970

In order to summarize the design work on the New Jersey negative income tax experiment it will be useful to draw together the major issues of the preceding chapters in a logical rather than chronological ordering. To do this, I would like to draw first on some of the definitions and distinctions employed by Professor Jan Tinbergen in his model of policy planning.1

Tinbergen distinguishes three levels of what he calls economic means:

- (1) foundations — the most basic relationships and institutions in society
- (2) structures — fundamental elements of the organization of an economy and
- (3) instruments — quantitative variables whose values can be changed to effect relatively frequent adjustments of an economy.

As examples he gives:

- (1) the existence of a social security program,
- (2) the various types of taxes in use in an economy, and
- (3) the levels and structures of rates in existing taxes.

By economic means he means economic data that can be changed, with more or less difficulty, by policy makers. Changes in foundations he calls reform, changes in structures, qualitative policy, and changes in instruments, quantitative policy.

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It seems clear that the introduction of negative income taxation is not a
reform in Tinbergen's sense of the word. The United States has had a national
income maintenance program in operation for almost four decades since en-
actment of the original Social Security Act with its Public Assistance Titles.
Consideration of a negative tax comes closer to a qualitative policy decision
— that is, a basic reordering of the structure of income maintenance. At is-
sue are such fundamental questions as who benefits, in what form and amounts,
on what basis and under what conditions, who pays, and who administers and
how. Much more than quantitative policy — changing the values of existing
policy variables as, for instance, raising support levels or defining narrow
new eligibility categories — is involved.

Tinbergen goes on to distinguish three broad stages of policy planning.
The first is ascertaining the actual state of affairs. This stage in the field of
income maintenance — the documentation of the shortcomings of welfare in
terms of coverage, levels of support, incentives to work and save, indepen-
dence and dignity of recipients, and administrative propriety, uniformity, and
efficiency — has been going on for many years and has come into particularly
sharp focus since 1965. Some of this effort is reflected in my background
Chapter I.

The second stage is forecasting the future assuming no policy change
and measuring the divergence of the forecast from the desired state of affairs.
Projections of this sort in the field of income maintenance have taken place on
several fronts. Much attention has been given to the inevitable unmet needs
of poor families, particularly with respect to hunger, if the current welfare
system with its low benefits and limited eligibility continues unchanged. At-
tention has also been given to the self-defeating nature of many welfare regu-
lations and procedures which tend to work against people moving off the rolls.
But most of all, attention has been paid to the rapidly rising levels of welfare
costs, which fall heavily on hard-pressed states and localities, and to projec-
tions by many in government and elsewhere that the burden of these non-fed-
eral costs plus an ever-growing bureaucracy will almost certainly cause the
system to collapse if just the people who are now eligible for aid apply for it. Again, some of the prospects for continuing poverty and an upcoming crisis in welfare were mentioned in Chapter I.

Tinbergen's third stage is estimating the effects of alternative policies. Prior to 1967, a number of useful efforts had been made to anticipate the impact of negative tax programs by looking at the behavior of Social Security, Public Assistance and General Assistance recipients, and at the behavior of persons with varying amounts of property income. But the negative tax concept represented such a fundamental restructuring of income maintenance efforts that extrapolations from existing programs to both different types of programs and different types of beneficiaries were not adequate to make the necessary policy judgments. In order to carry out planning stage three reliably, an experimental pilot study was required, and, for perhaps the first time in the history of American social decision-making, such a study was approved and funded in June 1967.

Once an experimental study had been decided on, the various design issues discussed in Chapters II through V, remained to be identified and resolved. These many issues can be usefully grouped under two headings — policy and design. Policy issues concern what kind of program or kinds of programs should be tested, what type of information about these programs is desired, and in what detail. Design issues concern where and how to set up the study so as to test successfully the programs of interest and obtain reliably the desired information. In what follows, I will deal in turn with each of the several major policy and design issues, posing the decision which had to be made and evaluating the choice.

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1. **Policy Issues**

I will begin with the policy issues, which have a logical precedence (even though there must generally be some feedback from design considerations since not all desired policy information can be obtained experimentally). These policy issues can themselves be divided into two major groups which I shall call statutory and instrumental. Statutory elements are those which constitute fixed features of the negative tax structures being tested and are common to all experimental treatments. Instrumental elements are negative tax parameters on which experimental evidence over a range of values is desired in order to guide policy makers to the best set of values.

- (a) **Statute variables:** Statute variables are fixed either because a particular configuration is inherent in the structure of the policy being tested, or clearly superior a priori to any other formulation, or because the experiment cannot, given its limited scope, undertake experimentation with them, or expect to learn anything useful about variations in them even if it could accommodate such variations. The principal statutory variables in the negative tax study are the following:

- (i) **The definition of the tax unit:**

  The question here is who may file for negative tax benefits and who may such person claim as members of the unit for which he is filing. The objective in formulating a tax unit definition is to group people in such a way that negative tax benefits paid to them reflect their true economic needs and in such a way that application of the definition does not, in and of itself, prompt people to regroup or to claim to have regrouped in unwanted ways. The two basic approaches which appear in the literature are a dependency-based criterion which uses a support test to determine who may file and who may be claimed as dependents, and an income pooling criterion which requires all persons who combine their income to meet major living expenses to file a single negative tax claim together. The second approach was adopted in the study, since the income pooling criterion was thought to measure better the economic status of
various household groups. In particular, it gave tax unit membership to persons who made major contributions to household expenses but were not dependent on the household head, and it recognized the unavailability to the household, in some cases of income earned by a person who was dependent on the household head. Persons who were determined to be sharing a dwelling unit with an enrolled household but making only modest fixed cash contributions to the household were allowed to file separately, in which case those contributions were counted as income to the main unit. This had the principal effect of permitting teenage earners to remain in their original households without their earnings which they kept for their own use being used to reduce the main households' benefits. For ease of administration, tax units were confined to a single dwelling unit, and all members of that dwelling unit were presumed to be pooling income in the absence of specific evidence to the contrary. The predominance in the sample of intact nuclear families occupying their own homes and apartments made the choice of tax unit definition considerably less critical in determining the benefit status of participants than it might otherwise have been. To date the adopted definition appears to have worked satisfactorily, and although a great many family units in the sample have gone through reorganizations, particular loss of members, none of these alterations seem attributable to any destabilizing incentives from the tax unit definition.

- (ii) The definition of taxable income:

An integral part of any tax program is an exhaustive definition of the income base to which tax rates are applied, indicating all items to be included in taxable income and all eligible exemptions and deductions from that income. The basic goal in defining income is to measure accurately ability to pay taxes, although other objectives, such as encouraging certain kinds of expenditures, are often added on.

In the case of the negative tax, ability to pay tax is usually seen from the opposite perspective as relative need for support. Matching ability to pay, or alternatively need for support, to overall economic status requires a comprehensive definition of taxable income. Such a definition generally includes
all cash receipts plus net unrealized capital gains, and is often extended to include some imputed income flow from assets over a certain value. The imputation reflects society's belief that persons with inadequate money incomes should turn to their own asset holdings as well as to the public purse in order to meet living expenses, and also the artificiality of any accounting procedure which defines need on the basis of income received over some arbitrary period of time.

In the negative tax study, the fact that sample households had virtually no assets other than their homes and personal possessions allowed a neglect of asset imputations in the definition of income for experimental purposes. The only imputations included in the income base were those applied to owner-occupied homes and to rent-subsidized public housing units.

Net unrealized capital gains were included in taxable income, as were all cash receipts by all tax unit members, except for modest exclusions on such things as private gifts and death benefits, and complete exclusion of public need-based transfer payments and payments, such as tuition scholarship, required by the terms of their receipt to be used for other than living expenses. Deductions were limited to costs of self-employment, costs of home-ownership and costs of alimony and court-ordered support payments. The issues involved in selecting a particular definition of income were not questions of choice among competing income concepts. The need for a comprehensive definition of income, as well as the constituents of such a definition, had long been agreed upon by most negative tax analysts, including the project designers. The questions were rather how closely the ideal could be approached in a limited experimental setting, without encountering major practical problems of obtaining and verifying accurate income reports. Overall, the income definition was designed to measure economic wellbeing as precisely as necessary, given the limited asset holdings of sample families, and as precisely as possible, given the practical limitations on administration, compliance, and policing within the project. The specific definition chosen has worked well to date — members of the project staff have not had occasion to change it and
participating families have accepted it and found it relatively easy to comply with their filing responsibilities under it.

- (iii) **The tax treatment of families by size and composition:**

Two objectives are commonly cited in setting a schedule of income guarantees by family size and composition. The first is equity — the provision of an equal standard of support for different types and sizes of households. Economies of scale in living expenses by family size and differential costs of upkeep by age or other personal characteristic of family members are considered here. The second is neutrality — the avoidance of any incentives to alter family size or composition which might stem from the structure of guarantees. Two such alterations are of particular public concern — the separation of parents of minor children and the enlargement of families through continued births. Associated with the second objective is a further goal — the avoidance of fraudulent claims of changes in family size or composition.

With different weightings of these objectives in mind, plus a desire for simplicity, a number of proposals for structuring guarantees have been made. Some suggest flat per-capita payments for all family members. Other, such as the Nixon Family Assistance Plan, feature per-capita payments at two levels — higher for adult family heads than for dependents. There are a number of variants of economies-of-scale guarantees, some tied to exemptions and deductions in the personal income tax and some tied to Social Security Administration poverty lines or other poverty measures. Finally, some proposals base guarantees solely on the ages of family members.

The guarantee schedule chosen for the study explicitly recognized economies-of-scale in family size and accepted as a point of reference the Social Security Administration poverty line for a family of size four ($3,300/year). Basic guarantees for families larger than size four tapered off more rapidly than SSA poverty lines, which rested solely on a nutritional standard, in order to allow for further economies-of-scale in other expenditure areas, notably housing. On the lower end of the schedule, the basic guarantee for a family of
size two was reduced below the corresponding SSA poverty line, and this smaller amount was split evenly between the two family members. The purpose of this arrangement was to rule out any gain in guarantees from splitting up of the first two members (generally male-head and spouse) of a family. Guarantees for the eight negative tax variants incorporated in the experiment were fixed percentages of the basic guarantees.

Family members other than the first two who left an original household and set up a new filing unit were allotted the marginal guarantees of their former unit at the time they left. Thus, the total of outstanding guarantees for each original unit and for the sample as a whole could not rise through the regrouping of individual participants. This arrangement was acknowledged not to be of policy interest, and accompanied a judgment that formal analysis of the impact of negative taxes on family stability was beyond the scope of the project. Evaluation of the guarantee schedule, both its equity aspects and, to whatever extent possible, its neutrality aspects, must await more experience with the program.

There are, of course, a number of other program features which are common to all negative tax variants being tested. These include provisions for uniform administrative procedures (including a common four-week period for income reporting, bi-weekly benefit checks, and standardized forms and techniques of field office operation) and arrangements for coordination with other public programs (such as low-income housing, Public Assistance, Medical Assistance, Food Stamps, and the personal income tax). Although they are of considerable practical importance to the smooth functioning of the experimental program, they pose somewhat less important policy issues for a national system than do the three statutory variables.

- (b) Instrument variables: There are two primary instrumental variables in the negative tax project and one secondary one. The primary variables are the guarantee level and the tax rate — the two parameters which completely define any linear negative tax structure. (Nonlinear structures were ruled out of the design at the early planning stage as too
ambitious for the limited scope of the project.) The guarantee level is a measure of the amount or degree of support to be provided by the tax program. It may be defined in absolute dollar terms (set, for instance, on the basis of political or economic judgments of what the society can tolerate or afford) or in percentage terms relative to some standard of need, which standard may itself be fixed in dollar terms as the Social Security Administration poverty lines or the Bureau of Labor Statistics modest but adequate income figures, or may be linked to some moving value such as the national median income.

In the project, guarantees were set at various percentages of the Social Security poverty lines, modified as indicated in point (iii) above. The poverty lines suffer from the deficiencies which afflict any absolute measure of need. It is difficult to pick a particular dollar figure which has any better claim to being a subsistence level or threshold than any other nearby figure. Although more income is generally acknowledged to be better (more adequate) than less, it is hard to argue scientifically that $3300 is better than $3200 by a significantly greater margin than $3400 is better than $3300, or vice versa, except to note the usual assumption of continuously declining marginal utility of income. Also, absolute measures of need, whatever their initial validity, are continually being outdated over time in their own terms in a dynamic economy. Indeed, most analysts view need as a relative phenomenon linked to the continually changing distribution of income. Furthermore, the poverty lines suffer from their own particular limitation to nutritional requirements as the single basis for determining need. But they were accepted as the most widely known and used measures, and some effort at improvement was made through adjusting their economies-of-scale. Four percentage levels of the modified poverty lines were included in the experiment — 50%, 75%, 100%, and 125%. The lower bound was thought to be the lowest benefit plan of national policy interest (it turned out to closely approximate the Nixon Family Assistance Plan) and the lowest-benefit plan of sufficient economic importance to sample families to produce some earnings adjustment to it. The upper bound was set as high as OEO was willing to go, so that a wide range of future policy choices and potential disincentive outcomes might be investigated.
The negative tax rate represents a balance between efficiency and incentive. A high tax rate reduces benefits rapidly as other family income rises, thereby focusing money relatively efficiently on those most in need. However, the high rate may so reduce beneficiaries' net wages that, in connection with the guarantee, they decide to work less than before, as little as before, or not at all. Lowering the negative tax rate will mitigate the work disincentive effects by allowing a higher net wage, but will begin to extend eligibility for benefits up into less needy and more heavily populated income ranges. The policy question may be seen as determining, for any given guarantee level, what the gains in terms of lower transfer costs from making the program more efficient by raising the tax rate cost in terms of lost output and resulting increases in benefits.

The project incorporated three tax rates — 30%, 50%, and 70%, in order to cover a broad range of trade-offs between efficiency and incentive. However, only eight of the twelve possible rate and guarantee combinations were included in the study, the others being ruled out as lacking sufficient policy interest. The eight featured plans are shown in the following chart.

![Guarantee level vs. Negative tax rate chart]

The area covered by these eight plans was judged to constitute the policy space of interest for national legislation in the short and medium run, and was thought to cover a wide enough range of tax parameter values to elicit some differential in work response. The overall decision on the policy space appears to date to have been a good one, with the possible exception of the
exclusion of the 125% guarantee/70% tax rate plan, theoretically the maximum disincentive plan, since significantly different work behavior by guarantee level or tax rate has not been observed so far.

The final instrumental variable in the study is the accounting period over which income is averaged for negative tax purposes. This is a timing variable which determines the immediacy of the tax bite on income and the sensitivity of benefits to changing family needs. Negative tax plans with relatively long accounting periods — e.g., full-year averaging — provide a stable floor under income and rely on other transfer or insurance programs to meet shorter-term fluctuations in family needs where necessary. They may also have favorable incentive effects from spreading taxes on current income over a relatively long period of time. Tax plans with short accounting periods — e.g., monthly reporting with no averaging — respond quickly to changing family needs but may exacerbate work disincentives since any earnings increases will immediately feel the full brunt of higher taxes. Short term accounting plans often feature some longer-term, usually annual, adjustment procedure in order to avoid inducements to irregular income flows and to achieve horizontal equity over the longer period.

Variation in the accounting period was introduced into the study on a partial replication basis. The main body of 1309 families allocated by the design model was subject to a basic accounting period of twelve weeks — that is, income from one four-week report period was averaged with income from the preceding two periods to determine negative tax benefits in the subsequent four-week period. An annualizing adjustment was superimposed on this moving average by means of a carryover for up to thirteen periods of any excess of average income over breakeven in any period. As the result of a decision midway through the sampling and enrollment procedure, an extra 65 families in Jersey City and Scranton were enrolled in a set of lagged-accounting-period plans. For these families, income in one four-week report period was averaged with income in the previous twelve report periods to calculate benefits for the upcoming four-week period. No carryover provision was needed with
this lagged accounting system which was itself an annual moving average. The purpose in introducing the accounting variation was to see whether substantially different timing mechanisms produced any difference in work behavior or entailed any particular benefits or hardships for families. The outcome will not be known until more experience is gained with the lagged system which appears only in the last two cities.

2. Design Issues

Turning now to the design issues of how best to experiment with the tax programs defined to be of policy interest, there are two broad questions of sample design to be considered. The first question concerns the nature of the population to be sampled for participants in the study. The broader the population sampled, within the overall population of potentially eligible beneficiaries of a national program, the more representative the sample and the more widely valid the results. However, the narrower the population, the less pronounced the natural fluctuations in income and the less difficult the detection of response to experimental treatment in the presence of those underlying fluctuations.

The limited scale of the study required some restriction of the sample to a group of low-income households relatively homogeneous in the factors which determined their work behavior and income. Families headed by a male aged eighteen to fifty-eight were judged to be the sufficiently homogeneous group of greatest policy interest. This interest stemmed from the pronounced public controversy surrounding their status as transfer recipients, the particular uncertainty about their response to transfer programs from which they had traditionally been excluded, and the major role their response would play in determining program costs given their large aggregate contribution to GNP. Sample families were further restricted to live initially in low-income tracts in central cities of standard metropolitan statistical areas. This limitation focussed the study on urban predominately wage-earning families with few or no assets and little seasonal fluctuation in income, thereby increasing sample homogeneity and yielding a particularly tractable group of participants.
The final decision on sampling was the exact location of the sample — i.e., the particular cities from which families were to be drawn. Concentration upon a few cities was decided upon for ease and efficiency of sampling and administration. An a priori decision had been made at OEO to go to New Jersey because of its selection of large, close-set industrial cities, its relatively limited welfare system, and the general receptiveness of its public officials. Extension to Scranton, Pennsylvania was later undertaken to draw additional white families into the sample and to embrace an entirely different kind of urban poverty setting from that represented by New Jersey ghettos.

Looking back to the early days of the study, the eligibility standard for participation in the sample, particularly the male-head requirement, was hotly debated both from within and without. On the outside, notably in Congress and OEO, critics found the limitation discriminatory and unacceptably restrictive. On the inside, staff members worried about restrictiveness as initial sampling results showed eligible households to be a much smaller proportion of the overall population than had been projected from 1960 Census data. However, outside criticism subsided when other income-maintenance projects dealing with other population groups in other locations were funded and got underway. And inside concern abated when it became clear that a sample of the desired proportions could be drawn from the projected cities without a change in eligibility criteria. Over time, interest in the originally defined group has, if anything, increased with the introduction of Nixon’s Family Assistance proposal and the resulting attention to the working poor. Also over time, New Jersey and Pennsylvania sample sites have shown no hostility to the project and have not undergone any anomalous social or economic transformation which would make experimental results there suspect.

The second broad design issue is determination of sample size and allocation over negative tax treatments. This involves two separate questions of knowledge:

1. What knowledge about behavior is desired from the experiment?
2. What knowledge about relevant behavior is already known and, therefore available to inform the design process?
Considering question one first, the negative tax project is a wide ranging, multi-purpose, multi-disciplinary study. It seeks information about a variety of economic, sociological, psychological and political behavior in response to negative tax treatment. However, it was established early in the project that work behavior analysis was to be the overriding central focus of the study and the single basis for structuring the sample design. Other secondary objectives would then be fit as well as possible into the resulting framework. This was a reasonable simplification, since work behavior was far and away the major issue of policy interest in the study, and there was a well-formed body of theory on the subject which lent itself to model building. It would have been difficult even to express some of the secondary objectives in meaningful common units let alone group them, appropriately weighted, into one overall objective function. And, a priori, there was no reason to expect major conflict between the work incentive analysis and other objectives such as analysis of expenditure patterns, mobility, individual goals and expectations, family composition and functioning, community involvement, etc.

Within the focus on work behavior, there is still considerable leeway to define the specific objective or objectives of the study. An obvious and basic approach would be to look at some measure of behavior in response to experimental treatment — e.g., changes in earned income over one or more periods during the study — for all participants including those receiving the null treatment. This approach seeks to determine changes in the supply of labor at all points in the policy space plus the control point. It may be seen as estimating the contours of a response surface measured in terms of changes in earned income over all design points in the program. This was the approach adopted in my allocation proposal.

However, one may view the control point as not simply another design point for which readings on changes in earnings are desired, but rather as a baseline for measuring earnings changes at all other treatment points. In other words, the magnitude of policy interest for each point in the policy space becomes the difference between labor supply at that point and labor supply at
the control point. This difference can rightfully be called the response to experimental treatment, so that the design problem can be stated as measuring the contours of work response (in terms of differential changes in earnings) over the policy space.

Finally, one might choose to focus on the transfer costs associated with work response to experimental treatment (specifically, on the additional amounts of negative tax benefits resulting from income reductions in treatment groups over and above any income changes in the control group). This approach makes an explicit judgment that the reason one wants to determine behavior under negative income taxation is to estimate the cost, in terms of outpayments, of a national negative tax program. This cost of a national program has two components — the cost given the pre-existing distribution of income and the cost associated with changes in that distribution as a result of introduction of the program. It is the second component on which experimental data are required. The appropriate specification can be obtained by weighting the response magnitudes of the preceding paragraph by the relevant tax rates and by fractions which adjust for over-breakeven response (since tax units which reduce their earnings from above-breakeven levels will only be reimbursed for that portion of the reduction which lies below breakeven and then only at a rate determined by their assigned negative tax rate). These additional weights convert response (in terms of differential changes in earned income) to costs of response (in terms of additional dollar outlays from work reduction).

This last specification was the one incorporated in Wisconsin's design model and ultimately adopted for the experiment. The choice reflected an acceptance of the view that it was differential behavior between experimental and control groups which was of principal policy interest, and that such differential behavior was primarily of interest as a basis for estimating program costs.

One practical outcome of this choice is an emphasis on high tax rate treatments since earned income changes are translated into greater benefit changes in these plans than in lower tax rate plans. But the most important
result is a much larger than anticipated control group which encompasses a full half of the sample.

Even in a model which treated the control group as simply another design point and estimated behavior at all points on an equal footing, the control group would have received many more observations than any single treatment group due to the very low relative cost of control observations and the quite substantial variability in underlying income changes from year to year. A model which treated the control group as a baseline for measuring response in all treatment groups would have further increased the control group share, since control behavior would then enter into measurement of the response magnitude of interest for all treatments. But the particular model adopted for the experiment increased the control group share even further by determining the number of control observations needed to anchor response in each treatment group separately, and then adding up these numbers to obtain an overall control group.

It is important to keep in mind that control observations reveal nothing about (uncertain, controversial, and perhaps extremely variable) behavior under negative income taxation, but only about underlying (status quo) behavior with which treatment behavior is to be contrasted. Maximizing information about response, as defined in the experiment, means minimizing the variance of the difference between treatment behavior and control behavior. The variance of this difference is the sum of the variances of treatment behavior and control behavior, since the behavior of treated and untreated families is presumably uncorrelated. With the prevailing sample allocation, the precision of the estimate of control behavior will be greater by several orders of magnitude than the precision of the estimate of treatment behavior for any particular treatment group and, therefore, for the weighted average of all treatment groups as a whole. I believe that the precision of the estimate of response to negative tax treatment could have been, for equal cost, improved by shifting families out of estimating control behavior and into estimating treatment behavior.
Turning to question two, the issue becomes how much designers already know about response. If one knows something about response, one certainly wants to build that knowledge into the design so as to limit the area of ignorance over which experimental results are sought, and focus efficiently on the remaining information desired. However, there are risks involved in assuming more than is known with certainty, since allocating observations efficiently to test a restricted model will prove costly if the model turns out to be wrong. On the other hand, in an area of no certainty but perhaps strong presumption, one can be too cautious about making plausible assumptions and suffer needless losses from inefficiency — i.e., from diluting the experiment's overall strength by providing for tests of response patterns that are highly unlikely.

In the project, a number of efforts were made to evaluate the risks of misspecification versus the risks of inefficiency for a range of alternative design model formulations. There was some difference of opinion on the weight of the evidence developed, but in the end, a particular quadratic model was adopted, thereby opting for the possibility of severe misspecification losses rather than the virtual certainty of more modest efficiency losses from an unrestricted model.

However, in a compromise solution, a number of free-hand adjustments were superimposed on the model outcome. The adjustments were made to allow for secondary research objectives which received no recognition in the model (particularly the desire for administrative experience with high-benefit plans, and for sociological studies of families whose incomes were raised substantially by their tax treatment) and to hedge in case the model specification proved incorrect. In other words, it was acknowledged that the joint answers to questions one and two were in some degree unsatisfactory — that the combination of narrowly defining the problem and then assuming a great deal of knowledge about it held unacceptable risks.

I believe that the final adjustments, which shifted the sample toward a more balanced allocation, were a move in the right direction and improved the sample design. However, I am not sure they went far enough away from
testing with maximum efficiency a restricted design model of quite uncertain validity, and toward a more cautious allocation acknowledging what I consider to be substantial area of ignorance remaining on the subject of work incentives. It remains to be seen whether the complete allocation will permit the kinds of analyses which researchers will wish to do when the true patterns of response emerge. But several preliminary observations can be made.

The first is that the sheer length of the design discussions has complicated the project. Very different allocation guidelines, representing different stages of the design development, were applied in Trenton, in Paterson/Passaic, and in Jersey City and Scranton. This has resulted in quite different treatment packages by city as reflected in the following data on average payments.

**Average Negative Tax Benefits by City**

*(Annual rate as of April 1970)*

<table>
<thead>
<tr>
<th>City</th>
<th>Average benefit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Trenton</td>
<td>$ 728</td>
</tr>
<tr>
<td>Paterson</td>
<td>598</td>
</tr>
<tr>
<td>Passaic</td>
<td>884</td>
</tr>
<tr>
<td>Jersey City</td>
<td>1092</td>
</tr>
<tr>
<td>Scranton</td>
<td>936</td>
</tr>
<tr>
<td>Total sample</td>
<td>879</td>
</tr>
</tbody>
</table>

These links between treatment and city, which imply other correlations — e.g., between treatment and race — will require special attention in the analysis.

Secondly, as might be guessed from the quite modest benefit levels shown above, actual transfer costs are running considerably below the cost assumptions built into the design model. Total transfers were recently estimated to be running about $300,000 under budget per year, and this figure is consistently rising. Since all the competing design models had approximately
the same cost assumptions, this underflow would have occurred in any event. But it presents particular problems in the present case where payments by design were already low due to the particular workings of the model.

One problem is the relative importance of the $260 per year flat retention fee recommended by Tobin and paid in $10 bi-weekly installments as part of the regular benefit checks. This fee, which goes to all filing units regardless of tax treatment, is as much as one-third of the average total payment in one city, and the proportion is steadily rising everywhere. A flat payment of such relative magnitude may soon begin to contaminate carefully structured negative tax benefits, particularly for the large numbers of families around breakeven whose response is of critical importance to the model.

Another problem is the growing number of experimental families who are receiving nothing but the filing fee. These families may end up telling us little about behavior under negative income tax. Income data suggest that most will remain above breakeven for extended periods of time, and that those who do dip below will have appreciable carryovers to use up before becoming eligible for benefits. The low payments plus the failure to increase benefits quickly when incomes drop below breakeven have produced most of the experimental attrition to date. So, the problem with filing fee recipients is that they provide limited information, drop out in significant numbers, and leave a biased sample behind. The data for filing fee recipients and drop-outs are shown in the following table.

Discussion of families over breakeven and attrition leads to one final matter of interest — the problem of putting policy and design together to achieve a valid simulation. This matter is usually discussed under the heading of Hawthorne effects — behavior which occurs when experimental subjects respond not only to their intended treatment but to the fact of being in an experiment as well. A decision to go ahead with a study like the New Jersey project implies a judgment that potential Hawthorne effects are of manageable proportions. It does not imply a denial of their existence.

There are two different sources of possible difficulty in the negative tax study. One is inability to simulate completely a national program. This is
Experimental Group Status
(April 1970)

<table>
<thead>
<tr>
<th>City</th>
<th>Numbers of filing units</th>
<th>Attrition to date</th>
<th>Proportion of current sample receiving fees only</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Originally enrolled¹</td>
<td>Currently enrolled²</td>
<td>Currently receiving fees only</td>
</tr>
<tr>
<td>Trenton</td>
<td>87</td>
<td>76</td>
<td>40</td>
</tr>
<tr>
<td>Paterson</td>
<td>162</td>
<td>136</td>
<td>74</td>
</tr>
<tr>
<td>Passaic</td>
<td>113</td>
<td>90</td>
<td>55</td>
</tr>
<tr>
<td>Jersey City</td>
<td>166 (+ 30)</td>
<td>189</td>
<td>62</td>
</tr>
<tr>
<td>Scranton</td>
<td>131 (+ 35)</td>
<td>162</td>
<td>68</td>
</tr>
<tr>
<td>Total</td>
<td>659 (+ 65)</td>
<td>653</td>
<td>299</td>
</tr>
</tbody>
</table>

¹ Entries in parentheses for Jersey City and Scranton show additional families enrolled in lagged accounting variants without regard to the design model.

² These figures include new filing units formed by the splitting of originally enrolled households. There are ten such new units, half of them in Trenton.

manifested in such things as the limitation in time, the lack of government agency administration, the singling out of a limited set of people to participate, etc. The other is introduction of certain program features which serve only the research objectives of the study and are unique to the experimental setting. The principal example of this is periodic lengthy interviews with participants.

Coping with these purely experimental artifacts takes place on two levels — attempting to minimize their impact and attempting to measure or control for whatever impact may remain. Efforts on both fronts seem at present to be quite promising. The one growing dilemma, largely unexpected, concerns attrition and measures to counter it. It has been amply demonstrated that families who are not induced to stay in the program by a small amount, or in some cases any amount, of money can generally be inveigled to remain by the argument that they are playing a valuable role in an important national experiment. This argument has been used both to get families to enroll and, increasingly, to encourage their continued participation. In a national program,
there would be no determination to maintain contact with families who dropped out of the system, especially those families above breakeven and ineligible for benefits. But in the experiment, all attrition is costly and must be combatted as effectively as possible. With the low and declining levels of payments, and the large and growing proportion of families receiving no negative tax benefits at all, there is a real challenge in maintaining the sample reasonably intact without generating damaging Hawthorne effects by touting participants on their great experimental value.

Any overall evaluation of the design of negative tax experiment must, in the end, rest on a sum of individual evaluations of the many component design issues. Decisions on each issue must make sense in and of themselves and fit together with other decisions to form a coherent whole. In the preceding chapters, I have tried to present my judgment that basically sound decisions were made on the various issues of what to test, where, on whom, and how. Perhaps not all decisions were equally good — I have more reservations about some than others, particularly about the sample allocation outcome. But it is clear at this point that the project can operate smoothly according to design, and I believe that it will continue to operate smoothly to produce valuable experience and results both for income maintenance policy and for social-science experimentation.
APPENDIX X

RULES OF OPERATION

for the

BASIC INCOME PROGRAM

This booklet is designed to provide a summary of the rules of operation of the Basic Income Program being conducted by the Council for Grants to Families. It sets forth the definitions and procedures used by the Council in administering the program, and describes the rights and responsibilities of participating families.

COUNCIL FOR GRANTS TO FAMILIES
92A Nassau Street
Princeton, New Jersey 08540
RULES OF OPERATION for the BASIC INCOME PROGRAM

I. Auspices of the Program

The Basic Income Program is being conducted by the Council for
Grants to Families, an organization established jointly by MATHEMATICA
of 92A Nassau Street, Princeton, New Jersey 08540, and by the Institute
for Research on Poverty of the University of Wisconsin, Madison, Wisconsin
53706. Funds and overall guidance to the program are provided by the United
States Office of Economic Opportunity.

II. Confidentiality

All information obtained from families participating in the Basic In-
come Program will be used only to operate and to evaluate the program,
and will be kept in strictest confidence by the Council.

III. Family Participation

A. Initial Eligibility

The eligibility of a family to enter the Basic Income Program is estab-
lished by the Council after two separate interviews with the family. Families
are selected to be interviewed at random from among all families living in
certain neighborhoods of the following cities:

Trenton, New Jersey
Paterson, New Jersey
Passaic, New Jersey
Jersey City, New Jersey
Scranton, Pennsylvania

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Once preliminary interviews are completed in these cities, and families found to be eligible are enrolled, no new families may be added to the program.

B. Continued Participation

Eligible families may continue to participate in the program, which will last three years, as long as they maintain their residence within the United States and provide the Council with the correct information it needs to operate the program. (See Section IV, Obligations of Participants, and Section VII, Penalties.)

IV. Obligations of Participants

In order to continue in the program, families must report their income and the size of their family unit once every four weeks on Family Income Report Forms provided by the Council. All information requested on these forms must be filled in fully and accurately and filed promptly.

The Council utilizes a comprehensive definition of income which is described in detail in Appendix A, Definition of Income.

The family unit is defined as the head of the family and his or her dependents. A person is considered a dependent if he or she resides with and is supported by the head of the family.

A more detailed definition of dependency and provision for adding persons to the family unit after the start of the program, institutionalization of family members, and separation of family members into two or more households is given in Appendix B, Definition of Family Unit.

V. Payments to Participants

A. Calculation of Payments

1. The amount of each family's Basic Income Payment will depend on the number of people in the family unit, the family income, and the Basic Income Plan assigned to the family.
2. Two methods of income averaging will be used to determine Basic Income Payments.
   a. For some families, income reported to the Council during each four-week reporting period will be averaged with income reported in the previous two reporting periods. This three-period (12-week) moving average, plus or minus any imputation or deduction as described in Appendix A, Definition of Income, will become the income base for calculating Basic Income Payments in the next four-week period. If that income base in any four-week period is greater than the income level at which a family's Basic Income Payment for that period would drop to zero (the breakeven point) then the excess income over that level will be carried over and added to the income base in the next four-week period. Such excess income will be used to raise the subsequent income base or bases up to but not over the breakeven point, and will be carried over from period to period until exhausted or for 13 periods (one year), whichever is sooner.
   b. For other families, income reported to the Council during each four-week reporting period will be averaged with income reported in the previous twelve reporting periods. This thirteen-period (52-week) moving average plus or minus any deduction or imputation as described in Appendix A, Definition of Income, will become the income base for calculating Basic Income Payments in the next four-week period.

3. A family that files its four-week Family Income Report Form correctly will receive a flat bi-weekly filing fee from the Council. This fee will be in addition to any Basic Income Payment for which the family may be eligible.
B. Filing Requirements

In order to receive regular bi-weekly payments from the Council a family must submit its regular four-weekly Family Income Report Form correctly and on time to the Council. A more detailed definition of the filing requirements is given in Appendix C, Filing Periods and Procedures.

C. Interview Fees

Families will be interviewed once every three months for the full period of their participation in the program. These interviews are designed to evaluate the operation of the program, and have no bearing on the amount or timing of Basic Income Payments. Families will be paid a special fee for their participation in each of these interviews.

D. Reporting Payments to the Internal Revenue Service

1. The interview fees and the flat bi-weekly fees for filing Income Report Forms are taxable income and must, under Federal Law, be included in any income tax declaration submitted to the Federal Government.

2. The Internal Revenue Service has ruled that the bi-weekly Basic Income Payments are not taxable income. No Federal income taxes should be paid on these payments.

3. In addition, all families enrolled in the Basic Income Program and receiving more than the filing fee will be reimbursed, in whole or in part, for Federal income taxes paid during the time they are enrolled in the program. The amount due the family will be calculated at the end of each year, provided that the family has given the Council adequate evidence of its final Federal income tax liability for that year.
VI. Rights of Recipients

A. Disputing a Council Decision

1. A recipient may, at any time, dispute a decision by the Council with respect to his family's bi-weekly payment. The status of payments to families contesting a Council decision is given in Appendix C, Filing Periods and Procedures.

2. A recipient disputing the disposition of a matter by the Council must contact the local office, either orally or in writing, to initiate reconciliation procedures.

B. Administrative Remedies

1. The office manager will make arrangements to discuss the matter with the recipient and to achieve a mutually satisfactory disposition. The recipient may be accompanied by any representative he chooses to assist him in such discussion, and the office manager may be accompanied or assisted by other staff members of the Council.

2. In the event that the discussion does not result in a mutually satisfactory disposition of the matter, the office manager shall inform the recipient of his right to appeal. To exercise this right, the recipient shall make oral or written application for review to the office manager within five days. The office manager will then file a request for a hearing with the Review Board, and will make all other documents relating to the case available to the Board.

C. The Right of Appeal

The Council shall maintain a Review Board of three hearing officers whose sole function shall be to review the proposed decision of the Council. A more detailed description of the Review Board and its functions is given in Appendix D, Review Board.
VII. Penalties

Families who are otherwise eligible for full participation in the Basic Income Program may be penalized by the Council for any of the following activities:

1. Failure to provide the Council in a timely fashion with the information on family size, income, and address which it needs to operate the program.
2. Knowing supply of false information to the Council on family size, income, or residence.
3. Deliberately false statement of non-receipt of any Council payment.

Penalties for these actions will range from delay or forfeiture of particular payments (see Appendix C, Filing Periods) to recapture of unwarranted payments by direct assessment or deduction from later payments, to permanent exclusion from the program.

VIII. Policy Decisions

Policy decisions will be appended to and take precedence over this document.
Appendix A

DEFINITION OF INCOME
DEFINITION OF INCOME

For the purposes of the Basic Income Program, the term "income" is defined as it is for the Federal income tax, with the following exceptions and modifications.

A. **Additional Items Included in Income**

1. The entire amount of any payment received as an annuity or pension.
2. The amount or value of all prizes or awards.
3. The aggregate life insurance proceeds, in excess of $1,000, which a family unit received on the death of any one individual.
4. Gifts, support payments, inheritances, and trust distributions of capital, from sources outside the family unit, in excess of a total of $100 per year. But amounts received from a person who is living with a family unit but is not a member of the family unit, to the extent that such amounts represent reimbursements or contributions for the actual cost of maintaining such non-member person, are not income.
5. Interest on all governmental obligations.
6. Any amount received in the form of damages, insurance payments, workmen's compensation, or in any other form if it is paid as compensation for physical, mental, or any other personal injuries or sickness or for wage or income continuation.
7. The full amount of all dividends, including periodic payments that are in whole or in part a return to capital.
8. The amount of any scholarship or fellowship, including the value of
room and board supplied without charge, to the extent that such scholarship or fellowship exceeds the costs of tuition, fees, and books.

9. The amount of current or accumulated income that could, within the discretion of any person, be paid to a family member from a trust or estate, except that any such amount that is in fact paid to some other person shall not be so included.

10. Alimony and court-ordered support payments whether periodic, lump sum, or installment.

11. The rental value of public housing to the extent that such value exceeds the amount paid as rent, and the rental value of owner-occupied housing. Such value will be allocated across household members on a per-capita basis and will be attributed as income to the head of the family if the house belongs to him or to one of his dependents. Housing value allocated to members of the household who are not also part of the family unit will not be included in family income.

12. Any direct or indirect cash payments (other than payments that are required by the terms of payment to be used for purposes other than meeting general living expenses), and the value of lodging received in kind, from any job or public or private agency, including, but not limited to payments or transfers made pursuant to the following programs or plans, regardless whether received as a lump sum or as periodic payments:

(a) Unemployment compensation.
(b) Strike benefits or unemployment benefits paid to any person by any union or any other organization or agency.
(c) Social Security (Old Age, Survivors, Disability, and Health Insurance) benefits.
(d) Veterans Disability benefits.
(e) Training stipends.
B. Deductions and Exclusions

No deductions or exclusions shall be allowed except for the following:

1. Businessmen and independent contractors may deduct the direct costs incurred in earning income.

2. Mortgage interest and real property taxes plus a fixed amount for property maintenance may be deducted in computing the income from owner-occupied housing.

3. Alimony and court-ordered support payments to persons outside the family unit, whether periodic, lump sum, or installment, shall be fully deductible. Also, a fixed amount of $30 per month shall be deductible for each person whom the family supports outside the home, providing such person was being supported outside the home or was a member of the family unit at the beginning of the program. A family will be considered to be supporting a person outside the home if they make cash payments of $30 per month or more toward the upkeep of that person.

C. Capital Gains and Losses

Capital gains shall be taken into income at 100% and capital losses shall be deductible to the extent of capital gains realized at any time during the course of the program.

D. Public Assistance

1. The following payments and benefits and all others in which the size of the payment or benefit is based on demonstrated need, shall not be included in income:
   (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 Assistance.
2. Except for families living in Trenton, New Jersey at time of enrollment, no family in the program may receive any of the above listed payments, or any other in which the size of the payment or benefit is based on demonstrated need, at the same time the family is receiving more than a bi-weekly filing fee from the Council.

(a) Families receiving other payments or benefits at the time of enrollment may enroll in the Basic Income Program and will receive a bi-weekly filing fee from the Council as long as they continue to get these other payments or benefits and continue to report their income and family size to the Council.

(b) A family may choose to receive Council payments instead of other payments at any time during the experiment. Following the family's declaration of intent to receive Council payments and a suitable demonstration that it is not receiving other payments, regular Council payments will begin.

(c) Similarly, a family may choose to receive other payments instead of the Basic Income Program payments at any time during the experiment. Following the family's notification to the Council that it wishes to receive other payments, Basic Income Payments will cease and the family will receive only the bi-weekly filing fee.

(d) There is no limit set by the Council to the number of times a family may choose to change from one to the other form of payment.
Appendix B

DEFINITION OF FAMILY UNIT
DEFINITION OF FAMILY UNIT

A. Family Members

1. A person will be considered to be a member of the family unit if he or she is the head of the family or one of the dependents of the head.

2. A person is considered a dependent of the head of the family if he or she meets one of the following two considerations:
   
   (a) If the person is the wife, child, or stepchild of the head or any descendant of any child or stepchild, and lives with the head of the family.

   (b) If the person is not so related to the head, but lives and receives no more than thirty (30) dollars income per month from sources other than the head of the family or his dependents described in (a).

B. Adding Members to the Original Family Unit

A person who joins an eligible household after the start of the program will not be eligible for payments by the Council for Grants to Families unless one of the following conditions applies:

1. The new person is a child born to a female member of the initial family unit.

2. The new person is under 18 years of age and has been living in the household for more than six months.

C. Institutionalization of Family Members

A member of the original family unit will not be an eligible recipient of Council payments during any period in which he or she is serving in the U. S.
Armed Forces or institutionalized, unless such institutionalization is voluntary and the family unit makes payments for his or her care.

D. Separation of Family Members

An original family unit whose members separate into two or more units, i.e., whose members form or become part of two or more households maintaining separate and independent residences, will divide its original Basic Income Payments between or among those new units. This division will be achieved by splitting the total payment of the original unit into its component parts attributable to various adult and dependent members of the unit as follows:

1. A family head or spouse who leaves the original unit will take with him or her one of the two adult payments initially assigned to that unit. Such head or spouse will carry over the basic tax rate (the rate at which Basic Income Payments are reduced as other family income rises) assigned to the original unit, which rate will be applied to the pooled income of his or her new unit, in determining payment. If this new unit is already enrolled in the Basic Income Program, its basic tax rate will take precedence. The remainder of the original family unit will lose the payment attributable to the head or spouse who has left.

2. A dependent 18 years of age or older who leaves the original unit will take with him or her the marginal dependent's payment, that is, the payment attributed to the last member of the original unit. Such person will carry over the tax rate of the original unit, which will be applied to the pooled income of his or her new unit, unless that new unit is already enrolled in the Basic Income Program, in which case its tax rate will take precedence. The original unit from which the dependent has departed will lose the marginal payment, that is, the payment attributed to the dependent that left. There is no provision for independent filing by persons under 18 years of age.
3. A family which separates so that head and spouse live apart, each with at least one of the original unit dependents, will be treated as two families with only one adult member. These new families will carry one tax rate and pool income as described above.
Appendix C

FILING PERIODS AND PROCEDURES
FILING PERIODS AND PROCEDURES

It is necessary that a family submit a four-weekly Family Income Report Form to the Council correctly and on time in order to receive regular bi-weekly payments. Failure to submit a complete and correct form to the Council on time will result in late payments or, in the case of excessively late filing or unclear Income Report Forms, forfeiture of particular payments. Families have the right, however, to dispute the Council's decision with regard to the timing of any particular payment.

A. Filing Periods

1. A Family Income Report Form will be considered on time if it is received in the Princeton office within two weeks of the date on which it was mailed to the family. These two weeks will be known as the Regular Filing Period.

2. A Family Income Report Form which is not received in Princeton during the Regular Filing Period will be considered late. A family whose Income Report Form is late will be notified of this fact by a letter mailed from Princeton on the final Friday of the Regular Filing Period.

3. Payment will be made on a late report if it is received within 14 days of the end of the Regular Filing Period. These 14 days will be known as the Late Filing Period. Such payment will be made to the family one week after the regular bi-weekly payment date. The second check for that period will be mailed at its usual time.
4. A Family Income Report Form which is not received in Princeton by the end of the Late Filing Period will be considered void and neither of the payments based on that report will be made. The examiner in the Princeton office, in consultation with local Council representatives, may in special circumstances determine that payment will be made on a report which would otherwise be declared void. In order for families who have had an Income Report declared void to resume receiving Council payments in a later period, they must provide satisfactory evidence of their previously unreported income to a local Council representative.

5. If a Family Income Report Form is received on time but requires clarification before it can be used as a basis for payment, the family will be notified of this fact by a letter mailed from Princeton on the final Friday of the Regular Filing Period. In order to receive payment at the regular time, the family must call or come to the nearest Council office and clear up the problem within five days of the end of the Regular Filing Period. These five days will be known as the Adjustment Period.

6. A Family Income Report Form with problems which remain unclarified at the end of the Adjustment Period will be considered late. If clarification is subsequently made within 14 days of the end of the Regular Filing Period, the family will receive a late payment as described in (3) above.

7. An insufficient report which is not clarified within 14 days of the Regular Filing Period will be said to have lapsed and both of the payments based on that report will be set equal to the flat filing fee. In special circumstances, the examiner in Princeton, in consultation with local Council representatives, may decide to make regular payment on a report which has lapsed. Families must provide the necessary clarification of a lapsed report before they can resume regular bi-weekly payments in any amount greater than the filing fee.
8. Families who fail to file or clarify an Income Report Form in any period in order to conceal unusually large income receipts in that period will immediately, upon discovery, have that income included in their current moving average.

B. Payments Status of Families Contesting a Council Decision

1. At any time, a family may contest a Council decision with respect to the amount or timing of a Basic Income Payment.

2. A family shall be considered to be contesting a Council decision at the time of receipt, in the local office, of written or oral notice by the family stating its wish to challenge a decision and describing the disputed item(s).

3. Immediately upon receipt of such notice from the local office, the Princeton office shall accept the family's statement on the item(s) in dispute, and shall calculate payments accordingly. The family shall continue to receive payments calculated according to the statement initially sent to the Princeton office until the matter is resolved.

4. A family in the process of discussing a disputed item with the Council will retain its regular obligation to report all sources of income and changes in family size. Aside from the disputed items, all other procedures will be followed as set forth in these rules.

5. Any additional dispute(s), either related or unrelated to the first, will be considered separately.

6. If the matter is settled in favor of the family, payments will continue without adjustment. If settled in favor of the Council, an adjustment in the family payments will be made over a reasonable amount of time to correct any over- or under-payment which may have occurred before or during the discussion of the matter.
Appendix D
REVIEW BOARD
REVIEW BOARD

1. The Review Board shall consist of three hearing officers whose function will be to review disputed issues brought before it by the office manager on request of a recipient.

2. The Review Board shall maintain a docket of all proceedings. Upon receipt of the request for hearing, the proceeding shall be assigned a docket number in chronological order. Each proceeding shall be considered in the order in which docketed.

3. Hearings shall be conducted as soon as possible after receipt of the hearing request, in the Council office located in the community in which the recipient resides.

4. Upon receipt of the hearing request, the Review Board shall set the time and the date on which the hearing will be held; the date shall be no later than two weeks after receipt of the hearing request.

5. The Review Board shall assign one of its members to conduct the proceedings as hearing officer.

6. Immediately after the Review Board has set the date and the time of the hearing, the hearing officer shall mail to the recipient a notice of the time, the date, and the place of the hearing. This notice shall also be included:

   (a) A full description of the procedure and rights of the recipient as outlined in these rules.

   (b) A statement of the recipient's right to be assisted by a representative of his own choice.

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(c) A statement of the availability of copies of the substantive and procedural regulations and of past decisions of the Review Board, the place where such copies are kept, and the hours during which the recipient and his representative may examine them.

(d) A statement of the recipient or his representative's right prior to hearing to examine the documents to be presented at the hearing upon two days' notice to the hearing officer.

(e) A statement of the recipient's right to object to the time and the date of the hearing within three days' receipt of the notice, by mailing to the hearing officer a written statement of the reasons for the recipient's objections and his choice of dates and times for the hearing.

7. The hearing officer may for good cause shown by the recipient set a new date and time for the hearing.

8. Immediately after receipt of the recipient's objection to the time and the date of the hearing, the hearing officer shall notify the recipient of his decision to accept or reject the recipient's objection, and of the new date and time of the hearing if one is set.

9. The hearing shall be conducted by the hearing officer and shall be attended by the recipient, his representative, and any witnesses the recipient decides have information relevant to the issues.

10. If, prior to or during the hearing, the recipient or his representative decides that cross-examination of the local office manager or other Council personnel is necessary for full exploration of the recipient's case, he shall ask the hearing officer to ensure the presence of the local office manager and such other personnel during the hearing; he may grant a continuance of the hearing if necessary.

11. The procedure at the hearing will be informal. Legal rules of evidence shall not apply, but the hearing officer shall have discretion to refuse to hear obviously irrelevant evidence. The hearing officer shall
determine the order in which issues are to be explored and evidence presented. He shall ensure that the recipient and his representative have a full opportunity to present the recipient's case. He shall inquire fully into the matter at issue and shall receive in evidence the testimony of witnesses and any document which are relevant to such matters, including affidavits of witnesses unable or unwilling to attend. The hearing officer may examine the witnesses. He shall give the recipient or his representative full opportunity to cross-examine all witnesses. He shall allow the recipient or his representative to present an oral or written statement of the recipient's reasons for disputing the local office manager's decisions on the contested issues, including arguments against the agency's interpretation of substantive regulations.

12. As soon as possible after the close of a hearing, the hearing officer shall make his decision on each of the contested issues. The decision shall be based on the evidence and arguments presented at the hearing and on the files of the case.

13. The hearing officer's decision shall be in writing and shall contain the following:

(a) His statement of the issues and his resolution of each.
(b) His explanations and interpretations of the substantive rules and past Review Board decisions deemed controlling.
(c) Findings of fact together with a statement of the reasons for each finding, the evidence relied upon and a summary of the evidence rejected or considered insufficient and the reasons for such judgment.
(d) Where appropriate, an outline of the action necessary to implement the decision.

14. The hearing officer shall submit his written decision, together with the file of the case and the documentary evidence, to his colleagues on the Review Board for confirmation. On approval of one other Board member, the decision shall be final. If both the hearing officer's colleagues
disapprove the decision in any part, the entire Board shall review the files and the documentary evidence and shall resolve the issues involved. The Review Board's final decision shall conform to the standards set out in Rule 12.

15. Immediately after deciding, the Review Board shall return the completed file, together with any record of the hearing, and a copy of the Board's decision to the local office manager for action appropriate to implement the decision.

16. Immediately after deciding, the Review Board shall mail a copy of its opinion to the recipient at his home address, together with an explanation of the effect of this decision on future payments to the recipient. If a representative appeared with or on behalf of the recipient, a copy shall be mailed to such representative.

17. The Review Board shall have its decision rephrased to eliminate any references to the identity of the recipient. It shall transmit copies of the rephrased decision to the agency's central office for indexing and inclusion in the information made available to the public.

18. The Review Board's decision shall be final and shall not be subject to any administrative or judicial review or attack.

19. (a) Until the Review Board's decision, payments to the recipient as to the issues contested shall be based on the recipient's statement.

(b) After the Review Board's decision, payments to the recipient as to the issues contested shall be based on the Review Board's disposition thereof.

20. As its central office, the agency shall maintain copies of all of the agency's substantive and procedural regulations and copies of all decisions of the Review Board. Each decision of the Review Board shall be indexed according to subject matter and according to substantive or procedural regulations interpreted therein. The files shall be open to the public. The agency shall allow a recipient or his representative to obtain copies of any rules or decisions.
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