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INCOME MAINTENANCE EXPERIMENTS

MATERIAL SUBMITTED BY THE DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

TO THE

COMMITTEE ON FINANCE

UNITED STATES SENATE

RUSSELL B. LONG, *Chairman*

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FOREWORD

During the course of the Committee's deliberations on welfare proposals, it has come to our attention that the Department of Health, Education, and Welfare and the Office of Economic Opportunity are currently supporting a number of experiments directly relevant to the legislation under consideration. Materials on the experiments were submitted to the Committee and are printed so that they will be available to the interested public.

It should be noted that all of the experiments involving payments to individuals are guaranteed minimum income experiments; no attempt has been made to experiment with work-related proposals to improve our welfare programs. It should also be noted that results are not yet available on the effects of any of these experiments.

The justification for undertaking these experiments is succinctly summarized in the Department of Health, Education, and Welfare's "Background Paper on Income Maintenance Experimentation":

> The concept of Income Maintenance Experimentation had its origins three or four years ago in the growing interest, both within government and without, in the defects of our current welfare system and the design of alternative methods of income supplementation. Analysis of the various alternatives most widely promoted—ranging from universal children allowances to negative income taxes and wage subsidies—quickly focused on the fact that there was little, if any, hard data on behavioral responses to the various incentives, both favorable and perverse, implicit in any of these schemes. Since many of these so-called "inducted" effects of income maintenance policy have potentially huge fiscal or societal effects, it would seem that a sensible long-term program of income maintenance reform would require a sound program of research as its basis. Further, it appeared that given the subtlety of individual behavioral responses to varying incentives, it was impossible to accurately assess many of the most important potential consequence of income maintenance reform either by extrapolation from static cross-sectional data or from information gathered from the type of relatively uncontrolled demonstrations projects previously attempted.

The Department is to be commended for its forthright statement of the need for thorough and careful experimentation and analysis of the effect on motivation and work incentives before any guaranteed minimum income program is adopted by the Congress.

It is the Committee's hope that publication of this material will be useful to persons interested in proposals to provide a guaranteed minimum income.

RUSSELL B. LONG,
Chairman.

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BACKGROUND PAPER AND REPORTS ON INCOME MAINTENANCE EXPERIMENTATION

Background Paper on Income Maintenance Experimentation May 1971. Division of Income Maintenance Research, Family Assistance Planning

1. THE NATURE OF INCOME MAINTENANCE EXPERIMENTATION

An income maintenance experiment is a project which seeks to provide information on the effects of a given financial "treatment" which can be generalized not only to populations other than the particular one covered by the experiment, but also to variations in the treatment itself. For example, an income maintenance experiment will seek not only to show if the work effort or recipients will increase or decrease given a certain standard and tax rate, but to develop a statistical description of this relation from which one can infer what the labor force response will be to variations in the particular standard and tax rate chosen. Similarly an experiment might focus on effects on the birth rate which may occur as the cost of children is changed by income maintenance. The intent would be to show not just that the birth rate did or didn't change but how much it would be expected to change at various levels of payment.

To achieve this objective in an experiment an attempt is made to simulate laboratory conditions. Care is taken to gather adequate information on a non-treatment group which would provide some control comparisons. Other characteristics of the subject and their environment are continuously measured so that nonexperimental changes in the subjects' responses attributable to differences in personal characteristics can be isolated. Further, steps are taken to insure that the number of subject observations in both the group receiving the "treatment" and those in the "non-treatment" group, plus the range of variation in variables of interest, are sufficiently large to allow the application of the principles of statistical inference to determine the "significance" of any observed differences in response. The importance of these controls in the experimental situation is the following: they allow one to draw conclusions with a far higher degree of confidence in the probability of those conclusions being correct.

The definition of an experimental project contrasts sharply with that of a demonstration project in that the intent of a demonstration project is simply to show that a particular "treatment" can be administered to a given population and that, when it is, the status of this particular population will be altered in some discernible fashion. No attempt is made to control for the effect of nontreatment variables on the chosen population so that no rigorous generalization of the results to other populations or times, or to slightly altered treatment variables, is possible. A demonstration can be useful in terms of working out the "bugs" in the administration of a particular program or in generating public awareness or acceptance of such a program. An experiment on the other hand is conceptually far more difficult and often more costly but it yields more "powerful information." In short, an income maintenance experiment seeks to provide information which will help the policy-maker choose among the numerous options available to him by providing reliable estimates of the individual and social consequences of any particular choice.

2. THE NEED FOR EXPERIMENTATION

The concept of Income Maintenance Experimentation had its origins three or four years ago in the growing interest, both within government and without, in the defects of our current welfare system and the design of alternative methods of income supplementation. Analysis of the various alternatives most widely promoted—ranging from universal children allowances to negative income taxes and wage subsidies—quickly focused on the fact that there was little, if any, hard data on behavioral responses to the various incentives, both favorable and perverse, implicit in any of these schemes. Since many of these so-called "induced" effects of income maintenance policy have potentially huge fiscal or societal effects, it would seem that a sensible long-term program of income maintenance reform would require a sound program of research as its basis. Further, it appeared that given the subtlety of individual behavioral responses to varying incentives, it was impossible to accurately assess many of the most important potential consequence of income maintenance reform either by extropolation from static cross-sectional data or from information gathered from the type of relatively uncontrolled demonstration projects previously attempted. The idea of initiating carefully controlled experimental projects designed to yield statistically reliable data on stated hypotheses was thus conceived.

Among the most important questions requiring exploration through the experimental method are the following:

(*a*) How will proposed programs affect the incentive to work? If standards are raised to the point where some persons on welfare might be almost as well off as persons in unattractive low-paying jobs, will this encourage persons to drop out of the labor force and go on public assistance? On the other hand, if more liberal provisions are made for the retention of earned income while still retaining part of assistance payments, will some persons currently on public assistance be encouraged to enter the labor force or increase their work effort?

(*b*) What interactions will occur with manpower and work-related programs and services including jobs creation and training, day care and transportation services? Will the benefits achieved through a combination of income maintenance and job-related programs be "multiplicative" in the sense that they will be greater than what we would expect from adding together the observed effects of each type of program operating alone?

(*c*) How will proposed programs affect mobility—in particular, will it tend to accelerate, decelerate or reverse the current rural-urban migration pattern?

(*d*) Will family stability be enhanced by changes in income maintenance policies and if so what types of plans and variations within them will serve this purpose best? For instance, if male-headed families are included in the program, will this help to reduce family break-ups and illegitimacy?

(*e*) Will certain types of programs produce an adverse effect on family size—particularly child-oriented allowance systems?

(*f*) Will demand for social services, both public and private, be affected? Will the injection of additional money into a community of itself promote spontaneous development of private medical, legal and educational services for government-provided services; or will the latter still be required?

(*g*) Will consumption patterns change among low-income families? How high must payments be before families will budget significant amounts of money for more longrun investments in the health, education and general well-being of their families?

(*h*) What will be the general effects on the social and economic life of a community, particularly a small community? Will prices of goods and services change; will community cohesiveness be enhanced; will the location of businesses shift?

A parallel set of questions relates to how the same sort of policies might impact differently on different population groups and different areas—urban *vs.* rural; white *vs.* non-white; female-headed families *vs.* male-headed; aged *vs.* non-aged; persons in families *vs.* unrelated individuals and childless couples.

It is clearly not possible to obtain reliable answers to all these questions in a single experiment. At the same time, experiments are costly and difficult to design and implement. Consequently, the HEW–OEO intention has been to try to keep the number of experiments as small as possible, limiting such projects to a series of well-controlled and carefully designed experiments each of which will be a necessary and integral part of an overall research strategy. Thus each of the experiments discussed below focus on one or more issues of importance, these issues being determined both by their priority in policy making and by their suitability for exploration through the experimental method.

3. RELATIONSHIP OF THE EXPERIMENTS TO THE FAMILY ASSISTANCE PROGRAM

Each of the income maintenance experiments sponsored by HEW and OEO test programs that are consistent with the basic concepts of the Family Assistance Plan. That is, they test programs providing basic income allowances to families (including working poor families) through a work incentive structure (i.e. a tax rate or reduction in benefits less than 100 percent per dollar of earnings). However they all differ substantially from FAP in terms of specific details. For example, all of the experiments test more than one support level and most of those levels are substantially higher than the $2400 support level under FAP. The work incentive feature (or tax rate) is also varied in each of these programs.

Although the experiments have already provided some limited data supportive of the FAP concept, they are not necessary to justify the basic welfare reform proposed by FAP. The FAP proposal responds to the breakdown of the existing welfare system and is based on a simple analysis of the type of problems that caused this breakdown (e.g., the incentives in the current program for family breakup, the current work disincentive of excluding aid to the working poor and the widely divergent benefit levels across the States which produce gross inequities of treatment among equally needy families). No experiment is needed to demonstrate that these inequities should be minimized. The FAP program builds upon analysis of these problems and offers immediate and workable solutions to them.

While the FAP program is the appropriate answer to the current welfare crisis, it is inevitable that as time goes on, changes to the basic FAP legislation will be proposed by this or subsequent Administrations or by the Congress. Thus the experiments look to the future in the sense that they are designed to provide useful information to the policymakers who will be concerned with such questions as the impact of raising the basic support level, changing the marginal tax rates, expanding program coverage, integration with other in-kind and cash programs, and so forth. The central concern of the experiments, that of work incentives, does not arise in the current version of the Family Assistance Plan given its more modest support level and its work requirement provisions. FAP responds to a set of problems whose immediacy has been well documented. The experiments will be crucial in providing basic information for future changes to FAP.

4. DESCRIPTION AND STATUS OF THE EXPERIMENTS

There are four income maintenance experiments currently funded by HEW and OEO. The New Jersey and rural experiments are sponsored by OEO and the Seattle/Denver and Gary experiments are sponsored by HEW. All of these experiments focus on the controversial problem of work incentive in an income maintenance system. The first two OEO experiments deal almost exclusively with this crucial question and are designed to determine the effects of financial treatments on the work response of male-headed families in both rural and urban areas. Male-headed families are of particular interest since they constitute a large portion of the working poor population that was not covered under previous welfare programs. Thus, if there is indeed any disincentive to work in an income maintenance system, it will be most discernible in the work effort of those working already (i.e., primarily male-headed low income families) who may choose to reduce their work effort if offered a minimum annual income support that may approach their previous net income.

The more complex HEW experiments will focus on different issues of major policy concern in addition to the work incentive question. The Gary Income Maintenance Experiment will test the effects of a negative income tax plan combined with day care and social services on urban black families with particular emphasis on female-headed families. The Seattle Income Maintenance Experiment is designed to test the combined effects of a negative income tax plan with a manpower program, serving both white, black, and Mexican-American, male- and female-headed urban families.

In addition, HEW is funding a limited administrative test of some of the FAP program in Vermont. The Vermont project differs substantially from the four other projects in that it is aimed at solving operational problems of administering the program rather than in measuring the behaviorial response of program recipients. A brief description and status report on each project follows below:

A. The New Jersey Graduated Work Incentive Program

The Office of Economic Opportunity took the lead in the field of income maintenance experimentation in 1968 when it initiated work on the New Jersey Income Maintenance experiment. This experiment focuses on the question of the work response of male-headed families to a negative income tax type income maintenance program. The project concentrates on the urban poor in five communities. Design of the project was carried out under contract by the Institute of Research on Poverty at the University of Wisconsin with assistance from the Mathematica Corporation of Princeton, New Jersey. The first group of experimental families was enrolled in the project in August of 1968. A preliminary report of the results of this experiment, based on the first year of operation, was made by OEO in February 1970. A second preliminary is expected about April 1972.

B. The Rural Income Maintenance Experiment

The Institute of Research on Poverty (University of Wisconsin) under the sponsorship of OEO is also currently conducting an experiment in two rural areas (in North Carolina and Iowa) to test the work incentive effects of a negative income tax plan on predominantly rural populations. The population in this test will also consist primarily of male-headed families. Families were enrolled into the program in November and December of 1969. A preliminary report on the findings of this project is planned for July 1972, and it will be based on the first two years of operation of the project. A final report is expected a year later.

C. The Gary Income Maintenance Experiment

This experiment sponsored by DHEW will test the effects of a negative income tax plan, combined with day care and social services on black, urban families with particular emphasis on female-headed families who will comprise about 60% of the sample; this particular group is not covered by either of the two OEO experiments. This experiment, like the Seattle experiment will be generally compatible with the New Jersey and rural experiments in terms of the type of income maintenance program to be tested, definitions of family units and income and other basic design criteria. However, each of the HEW experiments will focus on a different issue of major policy concern, in addition to income maintenance financial treatments.

The principal focus of the Gary experiment is on the family work decision and how it is affected by an income maintenance transfer system. The experiment will attempt to measure economic responses, such as labor supply, consumption patterns and investment in human capital, as well as sociological variables such as family functioning, motivation, and asperations. In addition, the project will test the impact of separately administered social services (such as day care, homemaker services, and counseling) in combination with direct cash transfers in order to measure the demand for such services when their provision and acceptance is no longer conditioned upon the receipt of assistance payments. It has been argued that even if a secure basic income floor could be established, there would remain a need for specialized problem-solving services. The magnitude of need has not yet been established, nor has the cost-effectiveness of various service types been determined.

This project is funded by an HEW contract with the State of Indiana Department of Public Welfare. The design and operation of the project is carried by the University of Indiana via a subcontract with the State Welfare Department. Design of the project began in the fall of 1969. Enrollment of families into the project began in March 1971 and should be completed by the end of June. A preliminary report on project results is planned for the fall of 1973 with a final report to be submitted approximately one year later.

D. The Seattle-Denver Income Maintenance Experiment

This experiment is the most comprehensive of all the urban experiments, serving both white and black families, having either one or two parents present. The experiment is intended to test the combined effect of a negative income tax scheme with a manpower program. Thus, in this particular experiment, the income transfer program itself will be supplemented by one or more manpower programs including (a) job training (b) counseling and vocational guidance services; and (3) day care services for working mothers. The Seattle-Denver experiment includes a population not served to any substantial degree by any of the other experiments, namely one-parent white families, and will uniquely test the interactive effects of income maintenance and manpower programs.

The primary hypothesis to be tested in the Seattle-Denver experiment is that manpower training in combination with a rational system of cash transfers will yield a policy payoff exceeding the sum of the outcomes of the two separate components. The experiment will provide vital information concerning the proper mix of manpower and cash, thereby suggesting the most efficient allocation of scarce government funds in the future. For example, answers shall be sought to such questions as "how much will an additional $400 a year in basic financial support change the work effort of the family, if (a) there is no change in investment in manpower or (b) there is a simultaneous increase in the manpower investment in a family by $200?" The experiment will measure the effects of different combinations of income maintenance support levels and manpower programs by looking at the:

(*a*) Work effort of the household.

(*b*) Productivity of the household as measured by changes in earnings.

(*c*) Investment of the household in training or other education.

(*d*) Changes in attitudes toward the future.

(*e*) Changes in household stability.

While unemployment in Seattle was well below the national average when HEW first negotiated with the State of Washington for the design of the experiment in 1969, the unemployment rate has since risen precipitously to a current level over twice that of the national average.

This situation posed serious problems for the experiment, which is designed to measure labor suply response both singly and in conjunction with manpower counseling and training.

Ideally, one would wish in such an experiment to have a virtually unlimited demand for the services for the experimental population so that any differences in the work effort of these receiving financial and/or manpower treatments, as compared with the control or null treatment group, could be attributed to the incentive effects of these programs. In a situation of low or declining job opportunities, it would be hard to filter out the differential effects of changes in labor supply and demand unless some adequate control were provided through comparable information gained in a more favorable labor market situation. It therefore became necessary to divide the planned sample between the city of Seattle and another city, as life as possible in terms of the demographic characteristics of its population, but with a relatively high and stable level of labor demand. Denver, Colorado, has been selected as the control for the labor market situation. While this change has caused some disruptions to the project, the overall advantages of this move will be considerable. It will be possible to fulfill the objectives of the original Seattle design, and, at the same time, gain valuable information on the potential effects of income maintenance programs on normal adjustment to the business cycle.

This project is funded by an HEW contract with the State of Washington Department of Public Assistance. The design and operation of the project is carried out by the Stanford Research Institute via a subcontract with the State Department of Public Assistance. Design of the project began in the fall of 1969. Enrollment of families into the project in Seattle began in November 1970 and is expected to be completed by April 1971. Enrollment at the Denver site is anticipated to begin in August 1971. The Seattle/Denver Experiment, like the other three, is designed to run for three years. However, a small portion of the sample (approximately 20%) will continue on the program for two additional years. This extension will serve to verify that the experimental results from the total sample as well as the other three experiments are not unduly biased by the effects of a transitory change in income. A preliminary report of findings of the full sample is expected in the fall of 1973 and a final report approximately one year later.

E. Vermont Pretest Project

Although this project has frequently been referred to as an income maintenance experiment, its focus is actually on planning the implementation of the FAP program rather than on testing how the system works or how it affects the behavior of individuals. While the project was originally conceived as a full scale pretest of the FAP program, its scope is now limited to (a) the development of a detailed plan for Federal administration of the Family Assistance Plan and State supplemental and adult programs, and (b) the development of a model plan for day care under FAP and expansion of day care families throughout the State. A sample survey of potential FAP recipients to obtain baseline information will be conducted to support these planning efforts.

This project is carried out by means of a contract with the State of Vermont. The project began in July 1970. The six projected analytical volumes have been completed and have been submitted to DHEW. These analyses will be used in implementing the FAP program nationwide and are as follows:

Volume I Administrative Structure and Procedures.
Volume II Regulations.
Volume III Accounting Period Implications and Options.
Volume IV Development of the FAP Pretest in Vermont.
Volume V Report on the Baseline Survey and Cost Projections.
Volume VI Evaluation and Experimentation in Child Care.

The data from the baseline survey will provide us with detailed information about the impact of the FAP program upon a very significant portion of the FAP population (rural white working poor families, which constitute the largest single group of the newly eligible population under FAP).

The child care component of the Vermont project involves development of a plan for a model FAP child care system and subsequent implementation of the approved plan which will involve an expansion of existing facilities and services throughout the State. This plan has been completed and the implementation phase has begun. The Vermont 4-C has already taken significant steps toward resource development in conjunction with these planning activities.

5. STATUS REPORT ON FINDINGS FROM THE INCOME MAINTENANCE EXPERIMENTS

To date only one of the experiments has been in operation long enough to report any preliminary results, the New Jersey project. This is because the experiments must operate for at least a two or three year period before one can say with a high degree of certainty that the results observed were not simply distortions in the behavior of the experimental population which resulted from the newness of the project. Since the objective of the experiments is to measure the long-run response of families to an income maintenance program, the families must be able to regard the experimental payment as being secure for a reasonable length of time. Because these projects are intended to be carefully controlled experiments, it is important to limit as much as possible the perception in the minds of the experimental population that they are a special group, since this could very well bias the results. Therefore, it is not in the best interests of the overall experimental effort to make any partial findings generally available before the end of the project.

A. Findings from the New Jersey Experiment

OEO issued a brief initial report of findings from the New Jersey experiment in February 1970, and subsequently a more extensive report of those findings was issued by the Institute for Research on Poverty of the University of Wisconsin in June. Further preliminary results concerning the work effort of participants in the experiment were released by OEO in May 1971. However even the latest findings must be qualified as preliminary in the sense that they are based on only the first year's experience of the total population and 18 months for ½ of the sample. Thus some allowance must be made for the possibility of distortions in behavior of the experiment population produced during the start up phase. A brief summary of the New Jersey findings are:

(*a*) There is no evidence indicating a significant decline in weekly family earnings as a result of the income assistance program.

(*b*) Low income families receiving supplementary benefits tend to reduce borrowing, buy fewer items on credit, and purchase more of such consumer goods as furniture and appliances.

(*c*) The Family Assistance Program, excluding the Day Care Program and Work Training provisions, can be administered at an annual cost per familiy of between $72 and $96. Similar costs for the current welfare system run between $200 and $300 annually per family.

The more extensive analysis of work effort response released in May 1971, supports the earlier preliminary findings and further refines the data.

The only statistically significant difference in earning that was found between the experimental and control groups was a reduction in the earnings of wives in the yearly sample. However this difference does seem to disappear at the end of the 18 month period. As a result of the average number of workers per family declining, the total number of hours worked per experimental family is slightly less than for the control group.

However, since there are no significant earnings differences between these two groups, the results imply that the experimental families have significantly inceased their average hourly earnings compared to the control group. Indeed, the average family hourly earnings appear to have increased by 20% for experimental subjects as compared to only 8% for the controls.

It is important to note also, that there was no significant differential in the number of hours worked per family among the various income maintenance plans, indicating that the various combinations of tax rates and guarantee levels have not yet affected the number of hours a family works.

There are several plausible explanations for these observations. The availability of a "cushion" in the form of experimental benefits may allow the prime worker the freedom not to accept the first job he can find, but rather to seek one that is more appropriate to his skills and interests and pays a higher wage.

Another view suggests that when a family initially experiences an abrupt increase in income, there will be a tendency to "invest," rather than consume a substantial portion of the increase. Thus we may see an increase in the purchase of durable goods and/or an increase in "human capital" investment in the form of training and/or increased time spent searching for better jobs. Such behavior may account for part of the reduction in hours observed, as well as increased hourly earnings. This approach suggests that labor force participation and hours of work would return toward normal, and hourly earnings would stabilize at a new (higher) level. The hypothesis can only be tested as data covering a longer time span becomes available.

B. Findings from the Other Income Maintenance Experiments

In addition to the New Jersey experiment, there are three other income maintenance experiments, the Rural experiment funded by OEO and the Seattle-Denver and Gary experiments which are funded by DHEW. Since these experiments have been in operation for either just one year or are just beginning, research findings on individual behavioral response will not be available for at least two years. However, several important lessons have been learned in developing designs and administrative structure for these experiments.

The first and most important lesson arises from the fact that HEW experiments explicitly cover the current welfare population and, in so doing, attempt to replace the current layering of welfare and other in-kind benefits by a single integrated income maintenance program which preserves work incentives and eliminates horizontal inequities and vertical "notches". One lesson of this attempt is that it is impossible to achieve such integration without making some current recipients worse off unless fairly high guarantee levels are established for experimental purposes. For example, in Seattle it was necessary to modify the design structure by allowing rather generous day care allowances for all single-parent families since these are currently available to such families from the welfare department. In Gary, despite the existence of a maximum AFDC payment of $2,100 for a family of four, it was necessary to raise the minimum experimental guarantee to $3,300, and even at that level it will not provide superior benefits to some 30 percent of current welfare families. The anomaly occurs because Indiana welfare payments are at a minimum, not reduced at all for earnings below $2,560. Furthermore, given virtually unlimited work expense allowances, payments are in practice not reduced for some considerable distance beyond that earning level.

Another equally important finding is that certain administrative details can be among the most important determinants of the character and impact of an income maintenance program. Chief among these is the definition of an accounting period for determining eligibility for benefits. For example, the use of an annual accounting period will result in an income maintenance system far different from that which employs a monthly accounting period (which is similar to that being employed in the current welfare system) both in terms of cost, equity and work incentives. A brief analysis of the data obtained from the Seattle Income Maintenance Experiment showed that caseloads may be doubled when one uses a monthly accounting period rather than an annual accounting period.[1] Of a random sample of 100 male-headed families in Seattle with incomes below $15,000 annually, only 19% were eligible for payments on the basis of an annual accounting period, whereas with a monthly accounting period another 23% became eligible.

Furthermore, families that are similarly situated in terms of income over a short period (such as a month) may have quite disparate incomes over a long period (such as a year) and vice versa. Take for example two four person families with total annual earned income of $4,320 (the FAP breakeven point) but one family earns it over an entire 12 months period while the other earns all of it during a six month period. Under an annual accounting period neither family would receive any benefit payments since both are over the FAP breakeven point. However under a monthly accounting system the former family would still receive no payments but the latter family would receive $800 worth of benefits as a result of the way in which its earned income was distributed. Thus the monthly accounting system will not treat families who earned the same annual income in an equitable manner, if their incomes are unevenly distributed.

The significance of the choice of an accounting period on cost, caseload, and equity, as illustrated above, was brought out during the technical development of the income maintenance projects. This pre-

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[1] The accounting period systems noted here are but two of a number of differing accounting period systems which can be varied to achieve different program objectives.

liminary information has already been useful to the Ways and Means Committee in their selection of an accounting period system for the welfare reform bill recently reported out by the Committee.

Another almost as important lesson learned both in New Jersey and from analysis of the three-year baseline data collected in Seattle is that given the variability of income flows among the poor, regular reporting of income and prompt adjustment of payments is essential to keep program costs within tolerable bounds.

6. PROGRAM CHARACTERISTICS OF THE 4 INCOME MAINTENANCE EXPERIMENTS

	New Jersey	Rural (Iowa, North Carolina)	Seattle/Denver	Gary
Guarantee levels (1971) (family of 4).	$1,830 [1] $2,903 $3,889 $4,839	$1,938 [1] $2,907 $3,876 $4,844	$3,800 $4,800 $5,600	$3,300. $4,300.
Offset tax rates	30 percent 50 percent 70 percent	30 percent 50 percent 70 percent	50 percent 70 percent 70 percent decline [2]. 80 percent decline [3].	40 percent. 60 percent.
Sample size by experimental treatments:				
Experimental	724—60 percent	374—46 percent	3,850—76 percent	1,287—76 percent.
Financial only	624—60 percent	374—46 percent	1,000—20 percent	466—26 percent
Financial and manpower.	NA	NA	1,850—36 percent	NA.
Manpower only	NA	NA	1,000—20 percent	NA.
Financial and social services.	NA	NA	NA	466—26 percent.
Social services only	NA	NA	NA	355—20 percent.
Control	489—40 percent	435—54 percent	1,250—24 percent	495—28 percent
Original sample size by site	Trenton 197 Patterson-Passaic 452. Jersey City 390 Scranton 318	Iowa 308 North Carolina 501	Seattle 2,100 Denver 3,000	1,782.
Sample characteristics	Nonaged male-headed families and couples, black, white, and Puerto Rican.	Predominantly non-aged male-headed families, and couples and unrelated individuals some female-headed and aged families, couples and unrelated individuals, black and white.	Nonaged male- and female-headed families and couples, black, white and Mexican-American.	Nonaged male- and female-headed families, black.
Sex of family head	Male, 1,359—100 percent.	Male, 587—73 percent. Female, 108—13 percent. Over 114—14	Male (approximate) —60 percent. Female-headed families—40 percent.	Male (approximate) 792—40 percent. Female, 1,190—60 percent.
Special treatments	Accounting period variation.	Accounting period variation.	Manpower services.	Day care and social services.

[1] These are the projected guarantee levels for 1971. Actual levels will be set in July 1971 on the basis of the National Consumer Index's cost of living increase. Original levels for New Jersey (1968), $1,650, $2,475, $3,300, and $4,125 Original levels for rural (1969), $1,741, $2,611, $3,482, and $4,352.

[2] Tax rate declines by 5 percent for each additional $1,000 of earned income (e.g. the 1st $1,000 of earned income is taxed at a 70 percent rate the 2d $1,000 of earned income is taxed at a 65 percent rate, and so on.)

[3] Similar to procedure identified in footnote 2 above.

OEO PAMPHLET 3400-4
MAY 1971

Further Preliminary Results of the NEW JERSEY GRADUATED WORK INCENTIVE EXPERIMENT

Office of Economic Opportunity

Further Preliminary Results:

THE NEW JERSEY GRADUATED WORK INCENTIVE EXPERIMENT

Conducted by

The Office of Economic Opportunity

May 1971

NOTE

The experiment discussed in this pamphlet is a continuing one: Final results will not be available until June of 1973. Because of the current Congressional discussion of Welfare Reform, it was felt that preliminary data should be publicly disseminated, although the data are not fully analyzed.

The Office of Economic Opportunity discussed earlier preliminary findings in a February, 1970, pamphlet. Those findings were adjusted and extended in a June, 1970, discussion paper published by the Institute for Research on Poverty at the University of Wisconsin. This current analysis will be followed by further reports as future data merit.

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INTRODUCTION

It is abundantly clear that the present welfare system is failing to meet national goals:

-- Welfare recipients frequently receive more income from their welfare benefits than nonwelfare families who are working full time.

-- Benefit levels vary greatly from state to state.

-- In 26 states, male-headed families generally are ineligible for benefits, even if their total family income remains far below the welfare program's income eligibility criteria.

-- The rates by which welfare benefits are reduced as earned income increases are frequently so high that a family is discouraged from attempts to supplement welfare benefits by working.

In an attempt to rectify these inequities and inconsistencies, President Nixon in August, 1969, introduced a bold new plan for Welfare Reform. Designed to provide income assistance to all poor families with children, the Welfare Reform Program would move toward equalization of benefits among states; ensure that work effort is encouraged, not discouraged; and, for the first time, provide assistance to the working poor.

Policymakers have been concerned, however, that any such assistance program would encourage families to rely on the income assistance and withdraw from the labor force. If, it has been argued, benefits are increased as a family's own work effort decreases (and conversely, decreased as work effort increases), we could expect to see a substantial reduction in that family's incentive to work and a dramatic escalation in the cost of providing benefits.

Thus, results from an Office of Economic Opportunity experiment launched in 1968 are of particular interest to researchers and policymakers as they consider Welfare Reform. The experiment is testing the impacts of an assistance system, in many ways similar to the President's program, on a broad variety of issues: work incentive, cost of benefits, administrative costs, and a number of corollary issues such as the impact on health, borrowing and spending behavior, family stability, general attitudes toward work, children's school performance and social behavior, and leisure time activities. The central objective of the experiment, however, is to determine the relationship of labor supply to the level of benefits and the tax rate on earned income.

Like Welfare Reform, the experiment, which is being conducted by the Institute for Research on Poverty and MATHEMATICA, Inc., a Princeton, New Jersey, research firm is structured to provide assistance that increases as earned income declines and decreases as earned income increases. But unlike the President's Welfare Reform Program, this experiment does not include a work requirement. Nor does it provide the extensive day care services that are an integral part of the President's program.

The addition of these two provisions as proposed in the Welfare Reform Program would be expected to have a positive effect on work incentive. Moreover, many of those in the experiment can receive higher benefits from it than the proposed Welfare Reform Program would provide. Therefore, the proposed Welfare Reform would minimize any possible reduction of work effort that might be observed in the experiment.

The experiment was not designed to include a representative sample of the entire low-income population, but rather a portion of it that is of particular interest to those concerned with Welfare Reform: the urban, working poor. An experiment launched a year and a half after this experiment began is concerned exclusively with the rural poor (and is described in Appendix II). The urban experiment is limited to a random sample of poor and near-poor families in Trenton, Paterson, Passaic, and Jersey City, New Jersey, and Scranton, Pennsylvania, with:

-- At least one man (usually the family head) between the ages of 18 and 58 who is neither disabled nor in school.

-- At least one other person in addition to the family head; i.e., a child, a wife, or an aged relative.

-- Income in the year before the experiment started not in excess of 150 percent of the poverty line. (At the start of the experiment, this poverty line was $3,300 for a family of four.)

This group is highly significant for policymakers, since the urban, working poor represent about 45 percent of the families who would be eligible for the Welfare Reform Program. Furthermore, it is among this group that any work disincentive precipitated by an income assistance program would be most likely to be observed.

After screening and pre-enrollment interviews to determine eligibility, families in the experiment were randomly assigned to a control group or to an experimental group. Those in the experimental group were further randomly assigned to one of eight "treatments," which differ as to the guarantee level (level of benefits when income is zero), tax rate (rate at which benefits are reduced as other income increases), and, hence, breakeven point (level of earnings at which benefits stop).

The guarantee is 50, 75, 100, or 125 percent of the poverty level, which is annually adjusted as the Consumer Price Index changes. The automatic cost-of-living adjustment increased the level for a family of four from the $3,300 level at the start of the experiment to $3,482 and subsequently to the current level of $3,686. As other income increases, it is "taxed" at the rate of 30, 50, or 70 percent. The eight combinations of benefit levels and tax rates are as shown below:

Tax Rates	Guarantee Levels			
	50%	75%	100%	125%
30%	A	C		
50%	B	D	F	H
70%		E	G	

Thus, for example, for a family in Treatment A, benefits are computed by taking the difference between the actual guarantee (50 percent of $3,686, or $1,843) and 30 percent of the family's earned income. If the family has four members and an earned income of $2,000, then the benefit is the difference between $1,843 and $600 (30 percent of $2,000), or $1,243. Benefits for four-person families in each of the treatments with various earned incomes are shown below:

Treatment	$0	$2,000	$3,000	$4,000
A	$1,843	$1,243	$ 943	$ 643
B	1,843	843	343	0
C	2,765	2,165	1,865	1,565
D	2,765	1,765	1,265	765
E	2,765	1,365	665	0
F	3,686	2,686	2,186	1,686
G	3,686	2,286	1,586	886
H	4,606	3,606	3,108	2,608

It is not now possible to predict differential changes in work effort among families in the various treatments because data are available from less than half the total time span of the experiment. It is possible, however, to examine the aggregate impact of an income assistance program and to predict some trends in that impact with regard to recipients' labor market behavior. This analysis, although not as useful as later analyses will be, is unquestionably relevant to current considerations of Welfare Reform.

SIZE AND NATURE OF THE EXPERIMENT SAMPLE

A total of 1,213* families were selected for the experiment, with 724 being assigned to the experimental treatments and 489 to the control group. Payments began in Trenton in August, 1968, for a relatively small sample. In many ways, Trenton has served as a pilot for the other cities, with administrative procedures being tested there before being applied to the other four cities. Payments began in Paterson and Passaic in January, 1969, in Jersey City in June, 1969, and in Scranton in September, 1969. Because data collection and processing lags about four months behind the payments, the analysis presented here is based on the first 18 months' experience in Trenton, Paterson and Passaic and the first 12 months' experience in Jersey City and Scranton.

Although no attempt (other than the use of random selection and assignment processes for both groups) was made to match the experimentals with the controls as to ethnicity, pre-enrollment income, family size, or other characteristics, detailed analysis has shown that differences between the two groups at the start and at present are statistically significant only with regard to ethnicity. A part of this disparity will be corrected as a result of the enrollment of 141 new control families in Trenton, Paterson and Passaic. These new controls, who bring the total number of control families to 632, are not included in this analysis, however, because of the shortness of their time in the program.

* Not counting new controls added later, as discussed below.

A detailed breakdown of the ethnicity, pre-enrollment income, and size of the families in both the experimental and control groups is included in the tables in Appendix I; their assignment by city at the start of the experiment and the number still in the experiment at the end of the first 12 and 18 months follows:

	Experimentals			Controls		
City	Start	12 Months	18 Months	Start	12 Months	18 Months
Trenton	86	80	72	37	29	28
Paterson-Passaic	276	236	226	106	83	82
Jersey City	198	189	NA	192	171	NA
Scranton	164	163	NA	154	148	NA

As indicated above, 64 families in the experimental group and 33 in the control group dropped out during the first 18 months in Trenton, Paterson and Passaic. At the end of 12 months, a total of 56 experimental group families and 58 control families had dropped out of the whole experiment. This attrition rate does not appear to be unacceptably high, however. Based on previous experience with panel studies the sample design had anticipated a 10 percent attrition for those in the experimental treatments receiving high benefits. Higher loss rates were anticipated for controls and families receiving small benefits or no benefits because they are at or above their breakeven points. (The final design was based on a 20 percent loss rate for families who went over their breakeven points. Because early experience in Trenton and Paterson-Passaic indicated that attrition might ultimately exceed these allowances, payments made to families for reporting income were increased in order to make keeping contact with field workers more attractive.)

It is, of course, impossible to specify how much attrition is acceptable without knowing how the attrition ultimately will be distributed. If attrition is concentrated in a few experimental cells or among one or two types of families, as does not appear to be indicated, a 15 to 20 percent attrition rate would be quite serious. Randomly distributed attrition as high as 40 or 50 percent, on the other hand, would not seriously jeopardize data interpretation. Attrition does affect the precision of any analysis. For example, the statistical precision of the estimate would increase 12 percent if one-sixth of the sample drops out instead of one-third.

The experiment differs significantly from the Welfare Reform Program in that it coexists with the welfare system that the program seeks to replace. This does raise problems that will not exist if Welfare Reform is enacted. When the experiment began, New Jersey did not have AFDC-UP (Aid to Families with Dependent Children-Unemployed Parent), although it did have an AFDC program for female-headed families. At the start of the experiment in Trenton, those in the experimental group were allowed to receive AFDC benefits and payments from the experiment. They were required to report AFDC payments as well as any other income to the experiment field workers, but their benefits from the experiment were not reduced because of the AFDC payments. At the same time, they

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were instructed to report their experiment benefits to the state welfare office, and it was expected that the experiment benefits would be taken into account when the welfare benefits were determined.

In January, 1969, the AFDC-UP program was initiated in New Jersey, where it was extended to both those who were unemployed and those who were under-employed. Thus, all families in the experiment were theoretically eligible to receive AFDC-UP payments, should they become unemployed. This program provided a maximum guarantee of $4,160 per year for a family of four (a level higher than the breakeven point for several types of families in the experiment's lower benefit treatments).

In Paterson and Passaic, where payments were just beginning, and later in Jersey City, when payments began in June, families were told they could not receive both AFDC-UP and experiment benefits, but that they could choose between the two benefits and change from one to another at any time. These same regulations applied to Scranton, where an AFDC-UP program was in effect before the experiment started.

Because of some confusion on the part of families reporting benefits, the rule permitting Trenton families to receive experiment and AFDC-UP payments simultaneously was revised in January, 1970. Families in this city must now also choose between the two programs.

BACKGROUND OF THIS REPORT

When the House Ways and Means Committee was in the final stages of considering Welfare Reform in January 1970, it became clear that data from the experiment would be useful in its deliberations. At that point, however, procedures for recording, checking, correcting, and analyzing the data were in only the early stages of development. Thus, information from the first, second, and third quarterly interviews in Paterson and Passaic and the first, second, third, and fourth quarterly interviews with families in Trenton was retrieved by hand from the data files and coded by hand on punch cards. In addition, some earlier tabulations of data from the screening and pre-enrollment questionnaires were used, as were the income reports submitted every four weeks by the experimental families only. Minor errors of punching and coding were encountered, but time constraints prevented tracing them down and correcting them.

Despite these deficiencies, it was clear that the data base was broad enough and the analysis procedures sufficiently careful that preliminary trends could be predicted. A report suggesting those trends was therefore issued on February 18, 1970. It was also evident, however, that further analysis was needed.

Thus, in June, 1970, Dr. Harold Watts of the Institute for Research on Poverty issued a discussion paper, "Adjusted and Extended Results From the Urban Work Incentive Experiment." This paper was based on an analysis which corrected the coding and punching errors of the February report, and which utilized full-year data from

Paterson, Passaic and Trenton.

The June report confirmed the findings of the February report:

> "The main impression left after a review of these crude analyses is that the experimental treatment has induced no dramatic or remarkable responses on the part of the families. The data are weak at this point, and so we can only expect to detect large effects with any confidence. Consequently, the only prudent conclusion at this point is that no convincing evidence has been found of differences between control and experimental families. This is a remarkable finding in itself, since there is a wide-spread belief that such payments will induce substantial withdrawal from work and increases in other forms of dependence........
>
> "No evidence has been found in the urban experiment to support the belief that negative-tax-type income maintenance programs will produce large disincentives and consequent reductions in earnings."

This present report utilizes a computerized data base, which has permitted a much more sophisticated and refined analysis than either of the earlier reports. Data from Jersey City and Scranton are available for the first time; additional data are available from Trenton, Paterson, and Passaic. As noted, we now have data for a full year or four quarterly interviews, from all five sites. These five cities, for the sake of convenience in this report will be called the full sample. Data from six quarterly interviews, or 18 months, are also available for Trenton, Paterson, and Passaic, which will be called the half sample.

This analysis is based entirely on data from the lengthy, in-person interviews which are conducted once each three months with families in both the experimental and control groups and cover a broad spectrum of issues. The analysis is limited to data on work effort, however, because time constraints prohibit a more comprehensive analysis and because impact on work effort is the issue of primary interest to those considering welfare reform.

This analysis is further limited to data from the 1,075 families who have been interviewed continuously during the experiment; i.e., those who have not missed more than one interview and whose missing interview is neither the pre-enrollment interview nor the fourth quarterly interview for the full sample or the sixth quarterly interview for the half sample.

Finally, no attempt has yet been made to consider those receiving AFDC-UP payments as a separate treatment in the experiment. As noted earlier, these families must report their AFDC-UP payments and may not receive both AFDC-UP benefits and benefits from the experiment. The analysis reported here utilizes a sample that includes welfare recipients in both experimental and control groups. It was repeated excluding welfare families in both groups, and no significant differences in results were found.

Ultimately, of course, more sophisticated and refined analyses of the work behavior of welfare families will be made.

CURRENT FINDINGS

The new analysis of data from the urban experiment confirms the findings of the previous two analyses with respect to work effort as indicated by family earnings: There is no evidence indicating a significant decline in weekly family earnings as a result of the income assistance program.

As shown in Table 1, about 31 percent of the families in the experimental group in the full sample showed earnings increases of more than $25 a week during the first year, as compared to about 33 percent of the controls. Also in the full sample, about 25 percent showed earnings declines of more than $25, compared to 23 percent of the controls. About 35 percent of both the experimental and the control families in the half sample showed earnings increases of more than $25 during the first 18 months, while about 29 percent of the experimental group families and 23 percent of the control group families showed declines of more than $25.

Statistical analyses indicate that these differences are too small to be considered statistically significant--that they could easily have occurred by random chance.

Several other comparisons of control and experimental group behavior were made. The one statistically significant difference that was found was a reduction in the earnings of wives in the full sample (12 months' observation in all five cities). But as shown in Table 2, this difference does not exist at the end of the 18 months' observation of the half sample.

TABLE 1

TOTAL FAMILY EARNINGS CHANGES:
COMPARISON OF EXPERIMENTAL AND CONTROL GROUP EXPERIENCE

	Experimentals		Controls	
Full Sample a/	Number	Percent	Number	Percent
+	202	30.9	139	32.9
=	258	39.5	171	40.5
-	163	25.0	97	23.0
NA	30	4.6	15	3.6
Total	653	100.0	422	100.0
Half Sample b/				
+	102	34.6	35	35.0
=	90	30.5	35	35.0
-	86	29.2	23	23.0
NA	17	5.8	7	7.0
Total	295	100.0	100	100.0

\+ increase of more than $25 per week.
= change of $25 or less.
\- decrease of more than $25.
NA undetermined because at least one earnings observation is missing.

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a/ All five cities at the end of 12 months.
b/ Trenton, Paterson, and Passaic at end of 18 months.

TABLE 2

WIFE'S EARNINGS CHANGES:
COMPARISON OF EXPERIMENTAL AND CONTROL GROUP EXPERIENCE

	Experimentals		Controls	
Full Sample a/	Number	Percent	Number	Percent
+	43	7.5	40	10.8
=	480	84.1	317	85.2
-	38	6.7	14	3.8
NA	10	1.8	1	0.3
Total	571	100.0	372	100.0
Half Sample b/				
+	20	8.4	8	10.3
=	201	84.5	63	80.8
-	14	5.9	6	7.7
NA	3	1.3	4	1.3
Total	238	100.0	78	100.0

\+ increase of $15 or more per week.
= change of less than $15.
\- decrease of $15 or more.
NA undetermined because at least one earnings observation is missing.

a/ All five cities at the end of 12 months.
b/ Trenton, Paterson, and Passaic at end of 18 months.

Table 3 shows earnings changes for heads of households; again differences between the experimental and control groups were found to be statistically insignificant.

Statistical analysis also showed that the difference in earnings changes were insignificant:

-- Between the control and experimental group families where both the husband and wife are present.

-- Among those families assigned to high* benefit plans, those assigned to low benefit plans, and those in the control group.

In addition, when regression analyses were used to control for the effects of differences in ethnicity and location of the samples, no significant earnings differentials in total family earnings were found between experimental and control subjects.

The development and refinement of the computerized data base permitted measures of work effort in addition to earnings to be considered in this analysis. In particular, measures of hours worked by the family as a whole and by individual members, as well as the number of workers per family, were examined, using regression analysis to control for possible effects of ethnicity, city, age of family head, etc., in a test of whether experimental subjects behaved differently from control subjects as a result of the experimental

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*Because the families, on average, earn very close to the poverty line, the high and low benefit plans have been classified by size of benefits paid to families whose income is <u>at the poverty line</u>. "High" designates those plans that pay 45 percent or more of the poverty level at that income level and "low" designates those that pay 30 percent or less.

TABLE 3

HEAD'S EARNINGS CHANGES:
COMPARISON OF EXPERIMENTAL AND CONTROL GROUP EXPERIENCE

	Experimentals		Controls	
Full Sample a/	Number	Percent	Number	Percent
+	187	32.1	101	26.5
=	278	47.7	203	53.3
-	106	18.2	73	19.2
NA	12	2.1	4	1.1
Total	583	100.0	381	100.0
Half Sample b/				
+	97	39.9	22	27.5
=	91	37.5	38	47.5
-	50	20.6	19	23.8
NA	5	2.1	1	1.3
Total	243	100.0	80	100.0

+ increase of more than $25 per week.
= change of $25 or less.
- decrease of more than $25.
NA undetermined because at least one earnings observation is missing.

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a/ All five cities at the end of 12 months.
b/ Trenton, Paterson, and Passaic at end of 18 months.

treatment.

In the full sample of husband-wife families, a statistically significant difference in the number of hours worked appears between the control and experimental groups The differential between the hours worked by those in the experimental group and the hours worked by those in the control group is about 12 percent, with the experimental group working about five hours less a week than the control group. This difference, which did not exist at the beginning of the experiment, is largely accounted for by a difference in the average number of workers per family in the experimental group. Like the difference in the number of hours worked, the differential in the number of family workers is statistically significant. Since there are no significant earnings differences between the experimental and control groups, these results imply that the experimental families have significantly increased their average hourly earnings. Indeed, this did occur: For the full sample in the first year, average family hourly earnings increased by 20 percent for experimental subjects compared with 8 percent for the controls.

It is important to note, however, that there was no significant differential in the number of hours worked per family among the various income maintenance plans. Again, these data are too tentative to permit generalizations, but this lack of a significant differential does indicate that the various combinations of tax rates and guarantee levels have not yet affected the number of hours a family works. The differentials in average hours, employment, and earnings between experimental and control groups are detailed in Table III-1 in Appendix III.

These results are recent. While the differential in work effort (as measured by number of hours worked) was certainly anticipated by everyone associated with the experiment, the differential effects on hourly earnings seems not to have been expected. Hence, substantial analysis must be undertaken to try to clarify the reasons for this effect. The bulk of this analysis has not yet been done; indeed, much of it cannot be done until further data are collected.

Some further indications of how this differential is arising can be gleaned, however, from an examination of the behavior of separate members of the family. This examination suggests that about 40 percent of the differential in family hours is attributable to the heads of families in the experimental group working less than those in the control group. This differential is 6 percent of the average hours worked by the heads of families in the control group at the end of one year in the experiment. There is no evidence that this is associated with a few family heads totally withdrawing from the labor force and living only on the assistance payments. Rather, the effect seems to arise from the small differences in the amount of overtime worked, the length of periods of unemployment, or the time worked on a second job.

The remaining 60 percent of the hours differential is attributable to spouses and other adult workers. Here the effect seems to be related to the rate at which these secondary workers entered the labor force as the labor market softened over the course of the experiment. In other words, the effect observed appears not to be a reduction in work effort by secondary workers in the experimental group, but rather less of an increase in this effort than appears in the control group.

For all three groups of workers--heads, spouses, and other adults--a differential increase in average hourly earnings of 7 to 8 percent appears to favor the experimental groups.

There are several plausible (and partial) explanations for these observations that can be advanced. With respect to the differential in average hourly earnings, it is quite possible that the effect of the experimental treatment is to raise simultaneously the aspiration levels of the families with respect to wages and their capability to find work at these levels. The availability of a "cushion" in the form of the experiment benefits may allow the prime worker the freedom not to accept the first job he can find, but rather to seek one more appropriate to his skills and interests and one which also pays a higher wage. In the case of spouses and other secondary workers, the same type of behavior may be appearing. Secondary workers enter a slackening labor market generally to make up for decreases in the prime worker's earnings. Income assistance payments may lead to a delay in the entry of such workers, or provide them an opportunity to search for higher paying jobs.

Another explanation is that what we are viewing is the process of adjustment to a new source of income. Economic theory suggests that when a family experiences an abrupt increase in income, there initially will be a tendency to "invest," rather than consume, a substantial portion of the increase. This investment may take the form of purchase of durable goods, such as appliances or housing, or it may take the form of outlays to increase the family's "human capital," its skills and employment opportunities. If the latter is occurring, we would expect to see increased participation in training programs and/or increased time spent searching for better jobs. (In both cases, the "investment" takes the form primarily of foregone income which could have been earned during the training or search period.) Such behavior might account for part of the reduction in hours observed, as well as the increased hourly earnings on the part of families in the experimental group.

Over time, as families adjust to their new income source, this hypothesis would suggest a diminution in "investment" type behavior. Labor force participation and hours of work would return toward normal, and hourly earnings would stabilize at a new (higher) level. We hope to be able to test this hypothesis as more complete experimental data, covering a longer time span, become available.

The foregoing hypotheses relating to the hours and hourly earnings findings and their applicability to any national income assistance program must remain somewhat speculative on the basis of information now available. It is possible, of course, that some of the differences observed are due to aspects of family behavior that have not as yet been adequately measured or specified in the preliminary analyses of experimental data undertaken so far. It must be emphasized that what has been done to date is essentially descriptive. More powerful analytical tools can be applied, once all of the data are in, to provide much greater insight into the behavioral mechanisms behind the experiment findings. It should also be recognized that the results of an experimental program may differ somewhat from the results of a similar (or even identical) national program. For example, the results from a job search for an experimental subject may be different from those he could expect if all other job-seekers in his area were part of a national income assistance program. The explication of how, and to what degree, the experimental setting affects the results we will obtain is a matter of high priority on the analytical agenda of this experiment.

IMPLICATIONS FOR WELFARE REFORM

In essence, these new results do not significantly alter the conclusions drawn from the earlier analyses of the experimental data. There is still no indication of a precipitous withdrawal from the labor force by families who receive income maintenance payments. Moreover, as noted earlier, this experiment does not have any work requirement or day care programs. Both of these provisions could be expected to reduce any possible reduction in the hours of the prime wage earner.

It must be remembered that under the Welfare Reform Program, the benefit received by a given family will depend on total earnings of that family. The evidence available thus far indicates that family earnings of the experimental group have not fallen relative to those of the control group. Thus, this evidence continues to suggest that the labor force withdrawal phenomenon will not increase the costs of Welfare Reform.

These results may also suggest an additional reason for supporting the Welfare Reform Program. It appears that an income assistance system may give poor people, particularly the working poor, the ability to seek out better jobs. Their dependence on the vicissitudes of low-wage labor markets will be reduced because when faced with unemployment, they will be better able to search for higher paying, more permanent employment. If this is true, it should be viewed as a significant step forward in our policies for dealing with poverty. But again, we emphasize, we still do not have an adequate understanding of these results. Seeking that understanding is clearly our next order of business.

APPENDIX I

CHARACTERISTICS OF SAMPLE

TABLE I-1

ETHNICITY OF SAMPLE

	Experimentals		Controls	
Full Sample [a]	Number	Percent	Number	Percent
White	220	32.9%	174	40.4%
Black	259	38.8	134	31.1
Spanish-speaking	173	25.9	107	24.8
Other	16	2.4	16	3.7
Total	668	100.0	431	100.0
Half Sample [b]				
White	33	11.1	8	7.3
Black	136	45.6	54	49.1
Spanish-speaking	115	38.6	43	39.1
Other	14	4.7	5	4.5
Total	298	100.0	110	100.0

a/ All five cities at the end of 12 months.

b/ Trenton, Paterson, and Passaic at end of 18 months.

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TABLE I-2

SIZES OF FAMILIES IN SAMPLE

	Experimentals		Controls	
Full Sample [a]	Number	Percent	Number	Percent
1 - 2	17	2.5%	14	3.2%
3 - 4	152	22.8	116	26.9
5 - 7	345	51.6	223	51.7
8 - 10	129	19.3	65	15.1
11+	35	3.7	13	3.0
Total	668	100.0	431	100.0
Half Sample [b]				
1 - 2	12	4.0	4	3.6
3 - 4	71	23.8	32	29.1
5 - 7	147	49.3	57	51.8
8 - 10	57	19.1	13	11.8
11+	11	3.7	4	3.6
Total	298	100.0	110	100.0

[a] All five cities at end of 12 months.

[b] Trenton, Paterson and Passaic at end of 18 months.

TABLE I-3

FAMILY EARNINGS WEEK BEFORE ENROLLMENT

	Experimentals		Controls	
Full Sample a/	Number	Percent	Number	Percent
$0 - 25	76	11.4%	55	12.8%
26 - 50	30	4.5	11	2.6
51 - 75	98	14.7	78	18.1
76 - 100	217	32.5	142	32.9
101 - 125	128	19.2	73	16.9
126 - 150	58	8.7	32	7.4
151+	39	5.8	30	7.0
NA	22	3.3	10	2.3
Total	668	100.0	431	100.0
Half Sample b/				
$0 - 25	48	16.1	22	20.0
26 - 50	18	6.0	4	3.6
51 - 75	46	15.4	20	18.2
76 - 100	89	29.9	37	33.6
101 - 125	46	15.4	13	11.8
126 - 150	27	9.1	3	2.7
151+	12	4.1	7	6.4
NA	12	4.1	4	3.6
Total	298	100.0	110	100.0

a/ All five cities at end of 12 months.

b/ Trenton, Paterson and Passaic at end of 18 months.

APPENDIX II

RURAL EXPERIMENT

The rural experiment is being conducted among a dispersed sample of 810 farm and rural nonfarm families in Duplin County, North Carolina, and Pocahantas and Calhoun counties in Iowa. A total of 502 of these families are in North Carolina, and 308 in Iowa. Of the total, 54 percent are in the control group, and are receiving no income assistance payments; i.e., they are used as a basis against which to measure the behavioral responses of the 46 percent who are receiving payments. The total sample of 810 families includes 587 headed by a male between the ages of 18 and 58, 109 headed by a female in the same age range, and 114 headed by either a male or a female over 58.

Overall design and direction of the experiment, as well as all funding, comes from the Office of Economic Opportunity and the Institute for Research on Poverty at the University of Wisconsin. Like the urban experiment, the rural experiment is designed to continue for three years.

The primary objective of the rural experiment is to measure the effects of alternative tax rates, minimum guarantees, and accounting periods upon the work incentive of rural residents, and to compare and contrast these findings with those of the urban experiment. Again as with the urban experiment, a wide range of other objectives is included: determining the effect of payments on children of the poor (their health, school performance, vocational aspirations, etc.); changes in expenditure patterns, effects on credit versus cash buying; involvement in social business, and political organizations; the effects on family stability (separation and divorce rates); family nutrition and health; and on the rate and nature of rural-to-urban migration.

Families in the rural experiment have been receiving payments for 16 months. No preliminary analyses have yet been performed.

APPENDIX III

ESTIMATES OF DIFFERENTIALS IN EMPLOYMENT, HOURS, AND EARNINGS

TABLE III-1

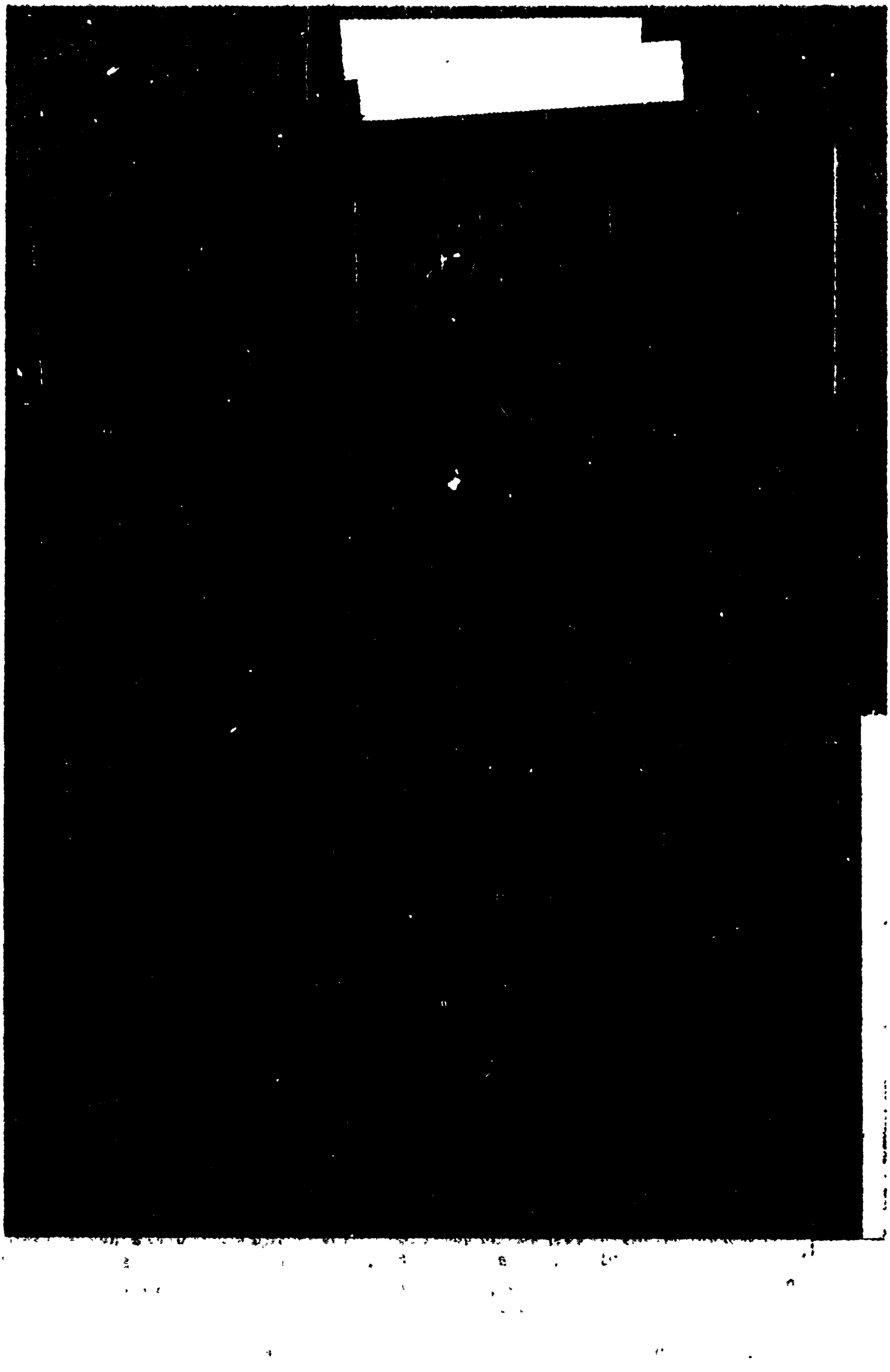
ADJUSTED MEAN ESTIMATES DERIVED FROM REGRESSION ESTIMATES OF DIFFERENTIALS IN EMPLOYMENT, HOURS, AND EARNINGS

(Husband-Wife Families)

	(1) # employed per family	(2) hours per employee (3)/(1)	(3) # hours per family	(4) earnings per hour (5)/(3)	(5) earnings per family
Family Total:					
Control mean	1.242	34.4	42.73	2.45	104.59
Absolute diff.	- .151**	+ .1	- 5.06**	+ .24	- 3.32
Exper. mean	1.091	34.5	37.67	2.69	101.27
% differential	-12.2%	+ .3%	-11.8%	+9.8%	- 3.2%
Male Head:					
Control mean	.885	37.9	33.55	2.61	87.52
Absolute diff.	- .032	- 1.0	- 2.09	+ .20	+ .75
Exper. mean	.853	36.9	31.46	2.81	88.27
% differential	- 3.6%	- 2.6%	- 6.2%	+7.7%	+ .9%
Female Spouse:					
Control mean	.176	28.6	5.03	1.92	9.66
Absolute diff.	- .044	- .1	- 1.27	+ .14	- 1.93
Exper. mean	.132	28.5	3.76	2.06	7.73
% differential	-25.0%	- .4%	-25.2%	+7.3%	- 20.0%
Other Earner:					
Control mean	.180	23.0	4.14	1.79	7.40
Absolute diff.	- .075**	+ 3.1	- 1.70*	+ .13	- 2.14
Exper. mean	.105	26.1	2.74	1.92	5.26
% differential	-41.7%	+13.5%	-41.1%	+7.3%	- 28.9%

NOTE: The fourth quarterly means cited above have been adjusted, by use of regression analysis, for the differing composition of the control and experimental groups in terms of location, ethnicity, age, family size, and pre-enrollment value of the variable in question. These means, and the associated control-experimental differentials, may therefore be interpreted as applicable to control and experimental groups with identical composition in terms of these variables. Percent differentials are computed using the mean of the control as the base.

*Significant at the .95 level. **Significant at the .99 level.



Revised June 10, 1970

ADJUSTED AND EXTENDED PRELIMINARY RESULTS FROM THE URBAN GRADUATED WORK INCENTIVE EXPERIMENT

by Harold W. Watts*

General Description and Orientation of the Experiment

It is useful to review the objectives and structure of the Urban Graduated Work Incentive Experiment, before going on to present and interpret early results. The relevance of this experiment to the on-going discussion of Nixon's Family Assistance Plan and welfare reform in general is genuine enough; but because it was planned and initiated well before the introduction of legislation, and because it is still a long way from completion, these results must inevitably be both less comprehensive and less powerful than many people would like them to be, or think they should be.

The impact of welfare reform on the labor supply is both crucial and poorly understood. First, if earned income goes down, the actual benefit paid out will increase. This will raise the cost of the program above the levels projected on the assumption of no change in income, though not by the full amount of the drop in earnings. Secondly, because any such income loss is only partially made up, the increase in spendable income for the recipient of the benefit will be less than that intended—i.e., less than the total amount of income before the program plus the benefit. Consider the following example: of a given dollar paid out in benefits (at a fifty percent rate) ten cents

*Extensive credit for efforts underlying this report is due to David Kershaw, Robinson Hollister, Jeri Fair, Felicity Skidmore and Nancy Williamson.

may in fact be offsetting a twenty-cent reduction in earnings, leaving the family only 80 cents better off than before. This would compare with an expected benefit of 90 cents, all of which would have represented increased spending power had there been no change in earnings. Hence, reductions in income induced by the transfer system cut two ways; costs are 10 cents higher than expected and the net impact on family income turns out to be 10 cents lower than expected. This double-edged effect of disincentives on costs and benefits makes accurate estimation of the earnings response crucial.

For many of the groups currently receiving the conventional welfare programs, large amounts of work and earnings have been neither expected nor realized. An improved incentive structure for these groups may elicit some small amount of additional effort; but for precisely the reason that they were originally allowed to receive transfers, it is unrealistic to expect that improvements beyond the $33 1/3 of income they can now keep in most states will produce a quantitatively significant increase in self support. The effect on labor supply of the group that has <u>not</u> traditionally been eligible for transfer payments (those working poor with appreciable if inadequate incomes) may turn out to be significant, however. This group represents proposed new beneficiaries who at present perform a substantial amount of work. Their gainful work could well be discouraged but we have no idea by how much. Therefore, the first priority in an experiment that aims at ascertaining the labor supply response to a major change in our transfer mechanism (and the consequent impact on costs and benefits of such a program) must be to examine this group.

This is the reasoning that led us to restrict our first experiment to families (i) which include at least one dependent person and one male between 18 and 58 where the male is neither disabled nor going to school at the time of initial enrollment, and (ii) whose total family income is less than 150 percent of the "poverty line."* People have expressed concern that other important beneficiaries of public assistance (the female-headed families, the aged, single persons, etc.) were not included. In large part, this concern reflects lack of appreciation of the difference between an experiment focussed on a specific and pivotal issue and a demonstration or pilot program aimed at a more holistic (and superficial) assessment of a proposed program. It is not because the excluded groups are regarded as unimportant in general, nor that the kinds of reforms being proposed would not provide major improvements in terms of dignity, equity, and even

*The actual income levels used for determining eligibility are not the same as the official poverty lines, but they are close. Our "poverty lines" are shown below along with the eligibility ceiling in terms of 1968 prices.

Family Size	"Poverty Line"	150% of"Poverty Line"
2	2,000	3,000
3	2,750	4,125
4	3,300	4,950
5	3,700	5,550
6	4,050	6,075
7	4,350	6,525
8 or more	4,600	6,900

incentives for these groups. It is rather that one important, well-specified and as yet unclarified issue can be most appropriately explored by confining the study to the working--largely male-headed--poor.

People have also been surprised to find the experiment not limited entirely to families below the poverty line. But it must be clear that any scheme that raises families up to or even close to the poverty line and provides incentives for recipients to augment their benefits must make partial payments to families well above the poverty line. The Family Assistance Plan, for example, pays minimum benefits of $1,600 for a family of four, but continues to pay fractional benefits up to an earned income of $3,920.* If the minimum benefit were raised to, say, $2,400 the benefits schedule would extend up to earnings of $5,520. There are many more working families in the $3,000-$5,000 range than there are below $3,000; and since these "near poor" are directly affected by such a program it would be very foolish to evaluate it on the basis of a minority among those who will be affected.

These restrictions on the eligible population do, certainly, limit the value of the experiment for any holistic kind of evaluation. The urban experiment in New Jersey and Pennsylvania is further limited by concentrating on families in those parts of specific Eastern industrial cities where poverty is most concentrated. Much of America's

*This amount is equal to twice the minimum benefit (because of a 50 percent tax rate) plus the $720 "set aside" of initial earnings that does not reduce the benefit at all.

poverty population is in rural areas and smaller non-industrial towns. And again much of it is scattered in parts of our metropolitan areas outside the most ghetto-like environments.

This one experiment was simply not designed to provide direct evidence on a random sample of poor families. It was designed to concentrate on an important but more manageable group within which the non-experimental variation was both less extreme and along fewer dimensions. Other experiments are underway--in rural areas and in urban areas with less exclusive sub-populations. But these are less far along, with some, indeed, just getting underway. Bearing all these limitations in mind, then, we may now consider a few key details of the structure of the experiment.

The sample of households includes (a) control households who receive no experimental transfers no matter how low their income goes and (b) experimental households who are similar to the control households in every way except that they are also eligible for payments related to their income, under one of eight different variants of a negative income tax. These eight differ as to (a) the maximum benefit paid when income is zero, and (b) the rate at which benefits are reduced as income increases; consequently they will also differ in terms of (c) the break-even point, i.e., the level at which benefits finally disappear. Some families at any time have incomes above the break-even point for their particular program variant, and will therefore be receiving no benefit. The control families, as well as the experimental families, can avail themselves of ordinary welfare and other benefits provided by state or federal programs, although the experimental families are required to forego benefits from the experimental program if they receive cash welfare payments.

Every four weeks the experimental families (and not the controls) are required to report their income and any changes in family size. The benefit calculation is made at the central office; and if a benefit is due, it is mailed to the family in two bi-weekly installments. All of the families, however, are interviewed every three months; and the data collected in this way (being comparable between control and experimental families) is the basis for all controlled and scientific comparisons. There are four experimental sites: Trenton, Paterson-Passaic, Jersey City, and Scranton Pa. The magnitude of work involved in finding and enrolling these households required that the experimental sites be started up one at a time. Payments were begun for the small (almost pilot) group in Trenton in August 1968. Paterson-Passaic did not come into operation until January 1969 followed by Jersey City in July and Scranton in October of the same year.

The families have been promised anonymity; they have also been promised that, so long as they report their income to us accurately and on time, they will remain eligible for payments based on their income for a three-year period. It has been expected that families will only gradually become adjusted to the program and the options it provides. Moreover, it seems possible that their behavior will be affected by the approach of the end of the experiment to the extent that they anticipate it. Thus, it may be that only a stretch of data from the middle part of the experiment will reflect "normal" behavior under a negative income tax program.

Selection and Assignment of Control and Experimental Treatments

The basis for measuring the effects of the eight negative tax treatments on experimental families lies in the comparison between the experimental group and the control group (null treatment) over time. The extent to which these two groups exhibit different characteristics at the time of enrollment on important variables such as income, employment and family size may therefore be important in interpreting the preliminary results. Using data from the screening interview, we found no significant differences between these groups in the urban experiment, allowing us to eliminate the possibility that variations in response could be caused by the mismatch of control and experimental groups on the basis of initial characteristics.

Experimental and control observations were selected randomly from a stratified* "pool" of families who were judged eligible on the basis of a screening and pre-enrollment interview (for eligibility criteria see above). No attempt was made to "match" the experimental and control observations on the basis of any of the characteristics; observations assigned to each of the three income strata were randomly allocated (using the RAND Corporation Table of Random Digits) to the control group or to one of the eight negative tax plans. In Trenton, Paterson and Passaic, 364 families were assigned to the experimental treatments and 145 families to the control group.

*The three strata are: (i) family income below $3300/year for a family of four; (ii) $3301/year to $4125/year for a family of four; (iii) $4126 to $4950 for a family of four. These levels are based on revisions in the 1965 Social Security Administration poverty lines.

Tables 1 through 5 below compare the two groups for several critical variables, including summaries of initial characteristics both for the 509 families from Trenton, Paterson and Passaic on which the OEO report and the present study are based, and for the full sample of 1218* (adding Jersey City and Scranton).

History of the February 18 Document

When the House Ways and Means Committee was in the final stages of consideration of Nixon's Family Assistance Plan, the Office of Economic Opportunity asked the Institute for a report on the first indications from the urban experiment. At that point analysis of the first returns had not yet been planned, let alone carried out. Only a fraction of the eventual data base was available, and attempts to draw conclusions from such a slim base would have been premature--at least from the viewpoint of conventional scientific research. Because of this opinion indeed, the development of a system for recording, checking correcting, and finally analyzing the data had been allowed to proceed slowly, and was only in an early stage of development.

As soon as we began to consider how to respond to OEO (at the very end of January), it became clear that a special crash effort was required simply because the data and processing system being developed for "normal" use would have taken at least two months to produce the

*This group does not include additional control families selected subsequently to bring the total sample to 1359.

Table 1: *Racial Distribution*

(Percentage)

	Experimental	Control
Trenton, Paterson and Passaic:		
Black	44.6	47.5
White	13.0	12.0
Spanish	42.0	40.0
Full sample:		
Black	38.6	30.9
White	32.8	41.0
Spanish	28.6	28.0

Table 2: *Mean Years of School Completed*

Trenton, Paterson and Passaic:	
Experimental	7.96
Control	7.46
Full sample:	
Experimental	8.63
Control	8.69

Table 3: *Family Head Employed at Enrollment*.*

(Percentage)

	Experimental	Control
Trenton, Paterson and Passaic:		
Yes	89.0	93.7
No	11.0	6.3
Full sample:		
Yes	93.1	94.1
No	6.9	5.9

*The difference in proportion unemployed at start is not large enough to be significant at the .90 level (two-tailed test), although the "t" value is just short of the critical value in the Trenton, Paterson and Passaic subsample.

Table 4: *Mean Family Size at Enrollment*

Trenton, Paterson and Passaic:	
Experimental	5.92
Control	5.54
Full sample:	
Experimental	6.00
Control	5.69

Table 5: *Mean Family Earnings*

(Year Preceding Enrollment)

Trenton, Paterson and Passaic:	
Experimental	$4,001
Control	4,008
Full sample:	
Experimental	4,103
Control	3,959

data instead of the two weeks we had. Consequently, quick decisions had to be made as to which variables would be of greatest interest and also from which of the available interview waves these variables could best be measured. It was possible to get observations spanning a full year for Trenton; for Paterson-Passaic the available observations were for nine months; and for the other cities the available time span was felt to be too short to provide useful indications of any impact the program might be having. Concentrating on the first two sites, then, we chose to use the 9-month income changes in Paterson-Passaic and to pool them with the 12-month changes for Trenton. Some information, of course, was drawn from first, second, and third quarterlies for both sites, as well as several items which were taken from the baseline or pre-enrollment survey. These items were coded from the several surveys by recruiting a large number of people over one very busy week-end. The coded data was punched in another rush operation, and then carried from Princeton to Madison for tabulation and analysis. Machine tabulations proceeded through the first week of February. We encountered, in the process, minor errors of punching and coding; but simply had no time to trace them down and correct them if we were to meet the deadline forced upon us.

The following week personnel from Wisconsin and MATHEMATICA took the raw tabulations to Washington, where a first draft of the report was put together. In addition to the coded and processed data from the questionnaires, two other sources of information were drawn upon for the report: (i) some earlier tabulations of data from the screening and pre-enrollment questionnaires that covered the entire sample (i.e., not just for Trenton and Paterson-Passaic), and (ii) income reports submitted every four weeks by the experimental families only. These income reports are valuable because they provide a more continuous and comprehensive record of income than can be obtained from the questionnaires, which are only administered quarterly. They do not (of course) provide any comparisons between experimental and control families.

Issues Concerning the Original Data Base

The most important and interesting issue about the experiment is, as was stated above, the effect of the transfer treatment on labor supply: i.e., the response of family earnings to the receipt of benefits. It was imperative that the OEO report address itself directly to this problem. Table IV of that report, showing income changes for control and experimental groups, represented our best efforts at that point to answer the question: What do negative income tax payments do to earnings of recipient family members?

The data behind that table were weekly incomes, measured by identical interviewing procedures for both control and experimental households, at two points in time. We were concerned to make the interval between measurements as long as possible, and to that end we used data from the pre-enrollment and fourth quarterly interviews in Trenton (which were administered in August of 1968 and 1969 respectively) and data from the pre-enrollment and third quarterlies for Paterson and Passaic (administered in January and October 1969). This involved the pooling of 9-month income changes for Paterson-Passaic with full-year changes for Trenton. Given that there are controls in both places it was not unreasonable to pool income changes for unequal intervals.

Longer intervals are, of course, better than shorter ones, and it is now possible to incorporate data from the Paterson-Passaic fourth quarterly and consider all the changes as referring to a one-year interval. The new income change tables in this report are all based on one-year changes.

A second problem is presented by the fact that a substantial number of the families initially enrolled had been lost for a variety of reasons, leading to the absence of any third or fourth quarterly to provide income information for them. Eighty of the original 509 households were in this category for Trenton. An additional 18 were lost in Paterson-Passaic between the third and fourth quarterlies.

This attrition is very troublesome—amounting to 19.3 percent of the 509 original sample points during the first year of operation. The attrition is understandably higher for controls than experimentals (27 percent versus 16 percent); and it has been around 21 percent for families with incomes too high to get benefits at the start. The rate is also higher for the Spanish-speaking part of the sample (28 percent) than for blacks and other whites (13 percent). It is, not surprisingly, lowest among families that have started and remained eligible for benefits above the minimum payments (8.3 percent).

In Trenton it is 22 percent and in Paterson-Passaic 18 percent. The better experience in Paterson-Passaic may be attributable to (i) higher base payments and also (ii) special efforts (introduced after our initial experience in Trenton) to reduce attrition. Since most of the Spanish-speaking people are in Paterson-Passaic, it seems likely that the added efforts to cut down on attrition have been successful, but partially offset by the inclusion of more Puerto Ricans. Within the experimental group, the high tax rate plans show slightly more attrition than the low ones; and the lower two income strata also have a slightly higher rate of attrition than the upper one. Some of the currently missing cases will be recovered, in the course of following cases that have moved, etc., but there will be other new attrition cases made up of those we can't find at the next interview. We shall only know the final extent of the attrition when the interviewing program is completed.

Besides cases of attrition, partially incomplete questionnaires made it impossible to secure usable information on income change for some families. Ideally, the income concept used for OEO's original Table IV required that there be complete income information for the husband and the wife from both the pre-enrollment and the subsequent quarterly interview. Two different practices were used when any of this information was missing:

1) If, on a given interview, income was reported properly for one spouse but not for the other, the latter was assumed to be zero. If neither spouse reported income, either at the early or later interview, the family was excluded from the analysis of income change. On this basis, 316 families provided usable income changes--84 control families and 232 experimental ones. Hence, in addition to the 80 families for whom the later interview was not available at all, another 103 were deleted because of incomplete income answers. These are the data that underlie variation I in the next section.

2) A second convention was used that permitted recovery of most of the 183 observations. If a spouse reported working in the previous week (on a separate question) but did not provide earnings information, that component of income change was considered unusable; and the logic outlined in (1) above was followed. If, however, the previous question was either unanswered or answered with a non-work response, the earnings item was assumed to be zero. On this basis, 484 out of the 509 were usable--i.e., another 168 observations were salvaged. But these would necessarily have a zero income total either for the earlier or

the later reading or both (40 in fact were zero both times). Nine more observations were rendered usable after corrections of original card punching, and this produced the data base underlying variation III in the next section.

Neither of these two conventions are entirely satisfactory. The first one excludes too many observations--cases where a zero income is a reasonable guess. The second includes too many--namely the attrition cases, for which no information at all was obtained from the fourth quarterly. A middle ground has subsequently been adopted, which excludes cases with no fourth quarterly data and assigns zero for other non-responses (except where there is evidence that one or both spouses are working). This process yielded a total of 401 observations on income change, which provide the data used for variations IV and V below, and for the income change tables in the following section.

Updating and Extending Charts IV and V of the OEO Preliminary Results Report of February 18, 1970.

As a previous section indicated, the OEO Report was compiled under considerable time pressure. Interviews used for analysis included the pre-enrollment through the fourth quarterly in Trenton and pre-enrollment through the third quarterly in Paterson and Passaic. We have subsequently had the opportunity both to correct coding and punching errors in the original data cards and to increase the length of the Paterson and Passaic experience by including the fourth quarterly interview.

The two most important entities in OEO's Report are clearly Charts IV and V: Chart IV specifying comparisons between experimental and control groups in Trenton and Paterson-Passaic with regard to changes in family incomes over time, and Chart V specifying the monthly mean incomes of experimental families in Paterson and Passaic only.*

In Table VI, we shall present OEO's Chart IV in its original form (variation I) along with four close substitutes (only one of which was available when the OEO Report was compiled).

Variation I (the one published in the report) was, as indicated earlier, based on the 316 families which reported earnings (possibly zero) on both the earlier and later interviews for at least one spouse. It was also based on data cards that contained minor coding and punching errors; and the 12-month changes from Trenton were pooled with the 9-month changes for Paterson-Passaic.

Families were cross-classified jointly by their pre-experiment earnings and their later earnings using intervals (of weekly earnings) as follows: 0; $1-25; $26-50; $51-65; $66-80; $81-95; $96-110; $111-125; $126-150; $151 or more.

Income was regarded as having changed for any family found in a different earnings interval for the later interview than for the earlier one. On average this required a change of at least $15/week in the middle of the observed earning distribution.

Variation II is based on the same procedure as Variation I, the only difference being that the errors in the data cards were corrected.

*Trenton was not included in Chart V primarily because of difficulties in handling the different time periods covered in the two sites. It could be included, however, without changing the direction of the trend shown in the chart.

Table 6: *Comparisons between Experimental and Control Groups with Regard to Changes in Family Earnings Over Time*

Variation I --Original Report: Trenton Fourth Quarterly and Paterson and Passaic Third Quarterly (Coding errors uncorrected; all non-responses eliminated; N = 316)

	Control	Experimental
Percent of families whose:		
Earnings increased	43%	53%
Earnings did not change	26	18
Earnings declined	31	29

Variation II --Trenton Fourth Quarterly and Paterson and Passaic Third Quarterly (Data cards corrected; all non-responses eliminated, N = 318)

	Control	Experimental
Percent of families whose:		
Earnings increased	44%	55%
Earnings did not change	24	18
Earnings declined	32	27

Variation III --Trenton Fourth Quarterly and Paterson and Passaic Third Quarterly (Data cards corrected; non-responses analyzed to add in zero incomes; significant at 95 percent level of confidence; N = 493)

	Control	Experimental
Percent of Families whose:		
Earnings increased	31%	43%
Earnings did not change	25	19
Earnings declined	44	38

Variation IV --Trenton Fourth Quarterly and Paterson and Passaic Fourth Quarterly (Data cards corrected; families required to move out of interval $25 wide to show increase or decrease; attrition cases eliminated, other non-responses set equal to zero; N = 401)

	Control	Experimental
Percent of families whose:		
Earnings increased	34%	33%
Earnings did not change	37	39
Earnings decreased	29	28

Variation V --Trenton Fourth Quarterly and Paterson and Passaic Fourth Quarterly (Data cards corrected; families required to move out of interval $15 wide to show increase or decrease; attrition cases eliminated, other non-responses set equal to zero; N = 400)

	Control	Experimental
Percent of families whose:		
Earnings increased	41%	43%
Earnings did not change	29	28
Earnings decreased	30	29

Two additional cases were usable and the percentage distribution was changed only slightly.

Variation III shows the corrected data; but the alternative procedure outlined above was used to assign zero incomes to most of the non-response cases. This made a total of 493 "usable" records (97 percent). With the larger sample, the greater percentage of increases among experimentals become significant at the .05 level. A comparable table was computed while preparing the OEO Report using the uncorrected cards and was, again, only trivially different from this one.

Variations IV and V are based on a more drastically improved data base. Fourth quarterly earnings have replaced third quarterly earnings for the Paterson-Passaic families (these were not available earlier), and all (98) that have not (as yet) completed the fourth quarterly interview have been eliminated. The 410 observations remaining (one family that had split was entered twice in the original 509 cases), were then processed using the zero-assignment procedure when a head or spouse was not known to have been working. This process eliminated 9 families in which someone was working but no earnings were reported. For each of the remaining 401 families, the change in earnings over the year since the experiment started was explicitly calculated rather than inferred from a cross-tabulation. From the distribution of changes so calculated, Variation IV displays the percentage breakdowns for (a) increases of $25/week or more, (b) no change (plus or minus) as great as $25/week and (c) decreases of $25/week or more. Using these procedures there is virtually no difference between the experience of control and experimental families—

the latter had one percent fewer increases but also one percent fewer decreases.

For variation V, a narrower interval was used for "no change." Changes of $15/week or more were counted. This produced a substantial increase in positive changes and very little change (one percent) in the number of decreases counted. Here the experimental families had more increases and fewer decreases--but even so, the differences do not approach statistical significance.

Of the first three variations, which relate to the data used for the original report (except for correcting minor errors) variation III provides the strongest indication of greater effort, as reflected in earnings, for experimental families. Almost all families are represented, and the data have been purged of minor errors. The resulting differences in that table are significant at the .05 level (i.e., would happen only one time out of 20 purely by chance if there were no real difference between controls and experimental changes in earnings).

Variation I was used in the original report rather than (the uncorrected version of) Variation III for two reasons. First, it involved no assignment of values to non-response--and was "conservative" in that sense. Second, since there were, and are, ample reasons for being cautious in interpreting these early data, a non-significant and less marked contrast between control and experimental families, as provided in Variation I (or II), was preferred for immediate release--again with the aim of making a conservative choice.

The last two variations, which are based on a third approach to the non-response problem and use data for full-year changes in earnings in both cities show no significant differences. There is some effect that depends on the required size of earnings changes however. For reference it may be useful to note that a $15/week change of earnings of head and spouse corresponds to about 20 percent of average earnings at enrollment, and a $25/week change corresponds to 33 percent of average earnings at enrollment. The actual average increase over the year was around 7 percent, broken down as shown below in Table 7.

Table 7: *Weekly Mean Average Family Earnings*

	Control	Experimental
Enrollment	$74.87	$77.74
Fourth Quarterly	79.84	83.52
Percentage Change	6.6%	7.4%

The original report's Chart V has now been updated, thereby eliminating two small problems with the original data. There were minor entry errors in the raw data tables used to calculate the chart, and the final summation of Paterson and Passaic mean family incomes was not weighted correctly (data went to OEO for Chart V in six parts—mean incomes for each of the three incomes strata in each city—which were not properly weighted when added to get a total monthly mean income). Neither of these errors had any appreciable impact on the Chart and the conclusions obviously remain the same. In addition to the above corrections, the chart has now been extended to include two additional months, bringing it to a full year. Comparisons between the original and the updated and extended monthly means are shown below (Table 8):

Table 8: *Corrected and Updated Versions of the Chart V in OEO's Original Report*

	Original	Updated and Extended
Month 1	$340	332
Month 2	361	361
Month 3	388	379
Month 4	383	383
Month 5	381	372
Month 6	380	386
Month 7	363	355
Month 8	358	356
Month 9	385	370
Month 10	381	375
Month 11		383
Month 12		391

The Paterson-Passaic experimental group was used alone in OEO's original chart, because their report had to be short and easy to understand and Trenton could not be "added in" in any simple or obvious way.* Trenton started five months earlier and had been running longer, but there were many more observations in the Paterson-Passaic group, making the trend, though shorter, more reliable. In any case, since there are no comparable measures for the control group, Chart V can only be interpreted relative to general knowledge of income experience of the poor. Within this frame of reference, both Chart V and the comparable diagram for Trenton (or even for subgroups such as income strata) are equally emphatic in showing that there is no pronounced reduction in incomes following enrollment in our benefit program.

Further Tests and Analysis

This section presents more complete results for the subgroup of 410 families from Trenton, Paterson and Passaic from whom we have usable fourth quarterly questionnaires.

As regards the analysis of change in earnings, the discussion here will concentrate on earnings changes greater than $25/week. Similar tests and tables were compiled using the $15/week criterion but, since they generally gave the same indications (and were similarly non-significant) they are deleted here. In addition to changes in total earnings of head and spouse (called family earnings above) changes in

*For instance, should one combine the same calendar month, as closely as possible, or months that are equidistant from the beginning of benefit payments?

earnings of the head alone have been analysed and are presented below, again considering only changes greater than $25/week.

The earnings changes of families or heads were classified according to treatment--both the gross control/experimental contrast and distinguishing among treatments within the experimental groups (Tables 9 and 10). Chi-square contingency tests were carried out, and in no case was the null hypothesis of no difference in earnings change among groups rejected at the .10 level (a less stringent requirement than the .05 level typically used for such tests). Classifications by city, ethnic group and stratum were also made (Tables 11 and 12). The only instance of significance at the .10 level was for husband's earnings change for the contrast between Trenton and Paterson-Passaic; here most of the difference between the two cities was found in the experimental groups.

When control/experimental comparisons were made within black and Spanish subgroups, there was a nearly-significant relation between the treatment and city classification and change in total family earnings. Most of this was due to a sharp difference between the two experimental subgroups (Table 13).

In Table 14 the control/experiment comparisons are shown within the two cities for earnings of the head alone. As will be noted, most of the favorable evidence for the experimental group comes from a disproportionate number of earnings increases in Paterson-Passaic.

Even though the other differences are not statistically significant, it will be useful to discuss further the patterns of income change shown in Tables 9-14.

Table 9 shows the distribution of earnings changes for the control and experimental groups and, within the experimentals, for tax rate and guarantee level. With the change to a more satisfactory data base, the distribution of changes in earnings is virtually identical between controls and experimentals when one considers the earnings of head and spouse combined. Higher tax rates appear to elicit more earnings increases in this table, as do high guarantees. But it must be emphasized that these differences are <u>not</u> significant. Table 10 shows the same comparison for earnings of the head only. Here heads of experimental families show up slightly better than controls; but otherwise the picture is much the same, and again not significant.

Tables 11 and 12 show the change distributions for the total sample and for the two different cities (or experimental sites) for ethnic groups and for income strata. Paterson-Passaic shows a greater prevalence of earnings increases and fewer decreases both for the earnings of head and for head and spouse combined. The differences are significant at the 90 percent level for head's earnings. The sample is then split into two parts--black and non-black--and a separate column is shown for the Spanish-speaking (overwhelmingly Puerto Rican) portion of the non-black group. For changes in head's earnings (Table 12) there is scarcely any discernible difference. What little there is shows the blacks having fewer decreases in earnings. Considering combined income of head and spouse, the experience of the black families shows a more pronounced (but not yet significant at the 90 percent level) tendency toward earnings gains as compared to the rest of the sample. The contrasts by stratum mainly show a tendency for the higher strata to have

Table 9

Earnings Changes within Treatment Categories: Distribution of the Changes in Weekly Earnings of Head and Spouse between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings	Control	Experimental	Tax Rate 30%	Tax Rate 50%	Tax Rate 70%	Guarantee Low	Guarantee High
Increased by more than $25/week	34	33	30	31	41	32	37
Stayed within $25 of first enrollment	37	39	38	38	42	37	40
Decreased by more than $25/week	29	28	32	31	17	31	23
No. of Families	105	296	63	157	76	190	106

Table 10

Earnings Changes within Treatment Categories: Distribution of Changes in Weekly Earnings of Head Only between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings of Head	Control	Experimental	Tax Rate 30%	Tax Rate 50%	Tax Rate 70%	Guarantee Low	Guarantee High
Increased by more than $25/week	24	30	27	28	37	28	33
Stayed within $25 of first enrollment	49	44	43	45	45	44	45
Decreased by more than $25/week	27	26	30	27	18	28	22
No. of Families	105	296	63	157	76	190	106

Table 11

Earnings Changes within Cities, Ethnic Groups, and Income Strata: Distribution of the Changes in Weekly Earnings of Head and Spouse between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings	All	City: Trenton	City: Paterson-Passaic	Ethnic Group: Black	Ethnic Group: Non-Black	Ethnic Group: Spanish	Stratum: I	Stratum: II	Stratum: III
Increased by more than $25/week	34	27	36	35	32	27	28	35	37
Stayed within $25 of first enrollment	38	39	38	39	38	41	45	41	31
Decreased by more than $25/week	28	34	26	26	30	32	27	24	32
No. of Families	401	93	308	191	210	153	127	113	161

Table 12

Earnings Changes within Cities, Ethnic Groups, and Income Strata: Distribution of the Changes in Weekly Earnings of Head Only between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings	All	City		Ethnic Group			Stratum		
		Trenton	Paterson-Passaic	Black	Non-Black	Spanish	I	II	III
Increased by more than $25/week	28	20	31	28	29	26	22	31	32
Stayed within $25 of first enrollment	46	46	46	47	44	46	53	45	40
Decreased by more than $25/week	26	34	23	25	27	28	25	24	28
No. of Families	401	93	308	191	210	153	127	113	161

Table 13

Earnings Changes for Treatment Contrasts within Ethnic Groups: Distribution of the Changes in Weekly Earnings of Head and Spouse Between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings	Black		Spanish	
	Control	Experimental	Control	Experimental
Increased by more than $25/week	36	35	27	28
Stayed within $25 of first enrollment	34	41	46	38
Decreased by more than $25/week	30	24	27	34
No. of Families	53	138	41	112

more earnings changes (i.e., fewer that stay within 25 dollars of the initial value). There is also some tendency for the larger number of changes to be on the plus side. This pattern holds up both for the income of head only and for the combined income of head and spouse, although there is an understandable higher prevalence of "no change" for earnings of the head alone.

Table 13 displays the different patterns of response to experimental treatment for the black and Spanish-speaking sub-samples. In the case of blacks, a larger fraction of the experimental families showed no change in combined earnings and most of the offsetting reduction was provided by fewer decreases. In the case of the Spanish groups, the experimental families experienced more changes than controls and most of these appeared as decreases.

Table 14 compares the treatment effects on changes in head's earnings in the two experimental sites. There is virtually no treatment effect apparent in Trenton, while in Paterson-Passaic there is a substantially higher prevalence of income increases for experimental families. (But there is a more favorable income change experience overall in Paterson-Passaic, as was noted above, which combines to make a significant difference in pattern between the two cities).

Tables 15 and 16 show the answer distribution for two attitudinal questions. These data come from the pre-enrollment interview--i.e., before the treatments started. The answers are only tabulated for the control and experimental families which have remained in the Trenton-Paterson-Passaic sample through the fourth quarterly.* Table 15 indicates that two-thirds of the

*The total adds up to less than 410 because of non-responses.

Table 14

Earnings Changes for Treatment Contrasts within Cities: Distribution of the Change in Weekly Earnings of Head Only between Preenrollment Interview and Fourth Quarterly (i.e., One Year Later)

(Percentage)

Change in Earnings, of Head	Trenton		Paterson-Passaic	
	Control	Experimental	Control	Experimental
Increased by more than $25/week	19	21	25	33
Stayed within $25 of first enrollment	46	45	51	45
Decreased by more than $25/week	35	34	24	23
No. of Families	26	67	79	229

Table 15

If Someone Gave You Enough Money for your Family to Live Comfortably, What Would You Do? (Alternative Answers in Percent)

	Total	Control	Experimental
Work less or quit	16	12	17
Work about the same	67	64	67
Work more	12	16	11
Other	5	8	5
No. of families	384	99	295

Table 16

What Things Do (Did) You Like Most About Your (Last) Job? (Alternative Answers in Percent)

	Total	Control	Experimental
Pay or wages	13	14	13
Co-workers	17	26	14
Treatment by boss	9	7	10
Steady work, security	34	28	36
Other	26	25	27
No. of families	360	93	267

families felt that they would work about the same amount even if they were guaranteed enought to live comfortably. Nearly as many indicated they might work more as indicated they might work less. Table 16 indicates that steady work and a secure job are prized substantially more than any other aspect of employment, and further substantiates the quite conventional work orientation held by families in the sample. Parenthetically, it is interesting to notice that such a finding was also strongly substantiated by the Heineman Commission.

Table 17 shows the prevalence of different major purchases in the first six months after enrollment of the families. The experimental families appear to buy substantially more furniture and more TV sets than the control families. Otherwise their buying habits are about the same. Twice as many families in Paterson-Passaic bought appliances and furniture as in Trenton.

Table 18 shows the status of the same basic groups of households at enrollment and one year later regarding the presence in the household of a husband, an employed head, and an employed spouse. It should be noted that the five percent reduction in families having an employed head is smaller than the nine percent reduction in families that have a husband present.

Table 19 provides further exploration of the reduction in the number of husbands present. Since the fraction experiencing a change is so small, it is quite frivolous to attempt any generalization from this evidence. But it is worth noting that any excess in the reduction of husbands present for the experimental families is accounted for by the fact that five experimentals and no controls have died--which serves as a lesson in the problems of data significance.

Table 17

Percentage of Families making Major Purchases During

First Six Months of Experiment

	TV	Appliances	Furniture	Other over $50
All families (410)	14.4	8.8	10.0	11.5
Control families (106)	12.3	8.5	6.6	13.2
Experimental families (304)	15.1	8.9	11.2	10.9
Trenton (98)	16.3	5.1	4.1	12.2
Paterson-Passaic (312)	13.8	9.9	11.9	11.2

Table 18

Percent of Families Including an Employed Head, an Employed Spouse, and a Husband Present

	At Time of Enrollment			At Time of 4th Quarterly (One Year Later)		
	Head Empl.	Spouse Empl.	Husband Present	Head Empl.	Spouse Empl.	Husband Present
All families (410)*	74	14	92	69	18	83
Control families (106)*	74	10	90	71	23	82
Experimental families (304)*	74	15	93	68	16	84
Trenton (98)*	79	20	93	65	26	82
Paterson-Passaic (312)*	72	12	92	70	16	84

*Number of families used as the base for the percentage.

Table 19

Change in Family Status During First Year

	Control		Experimental		Total	
	Number	Percent	Number	Percent	Number	Percent
Husband present at start	95	100.0	284	100.0	379	100.0
Deserted, separated, or divorced	6	6.3	18	6.3	24	6.3
Institutionalized	2	2.1	5	1.8	7	1.8
Died	0	0	5	1.8	5	1.3
Present at end of first year	87	91.6	256	90.2	343	90.5

In addition, a crude regression analysis was carried out to determine whether a significant experimental effect could be shown when a variety of other variables were held constant--such as experimental city, race, initial income status, etc. The dependent variable was either the combined (algebraic) earnings change of head and spouse, or the change for head alone. The conventional expectation would be that the experimental treatments would provide some (perhaps small) net disincentive when other things are held constant.

The results of these regressions, however, showed no reliable and significant effect of the experimental treatments even when other variables were held constant. These results are consistent with the general impressions gained from the review of the tabular analysis above. Most importantly, they suggest that there are not large and dramatic effects appearing in this experiment, and that much more data and more refined analytic work will be needed before any smaller effects there may be can be isolated and measured.

Conclusion

The main impression left after a review of these crude analyses is that the experimental treatment has induced no dramatic or remarkable responses on the part of the families. The data are weak at this point, and so we can only expect to detect large effects with any confidence. Consequently, the only prudent conclusion at this point is that no convincing evidence of differences between control and experimental families has been found. This is a remarkable finding in itself, since there is a widespread belief that such payments will induce substantial withdrawal from work and increases in other forms of dependence.

The crucial issue that relates to the effect on earnings is unresolved in the sense that no significant changes have been found. But to the extent that differences appear between control and experimental families they are generally in favor of greater work effort for experimentals. Hence, anyone who seeks to support an argument of drastic disincentive effects cannot expect to find even weak support in the data so far.

A word should be said about the nearly 20 percent of families originally enrolled that were not available for the fourth quarterly. These families have quit the program, moved and left no forwarding address, refused to be interviewed further, and so on. While efforts have been made that promise to cut such losses in the last two cities, this is already a large attrition rate, and must be expected to get somewhat larger in the two years that remain before the experiment is completed. Careful study of the characteristics of the lost families will, of course, be needed to assess the likelihood and possible direction of any bias thereby introduced. But it is worth speculating briefly whether such attrition is likely to have obscured an otherwise strong disincentive. For such to be the case, for instance, the experimental families missing from the fourth quarterly would have to have experienced more income reverses than those that remained and would, therefore, have received higher benefits as a result of their reduced earnings had they stayed in the experiment. It seems unlikely that large numbers of such people would have abandoned the payments which would otherwise have induced them to reduce their earnings.

As for the attrition in the control group, there may be some increased likelihood that families who enjoy a large income increase may be lost, but probably only if this is associated with a change in residence. At the same time, there is a large amount of mobility at the very lowest income levels and involuntary movement may well be induced by income reverses. The lowest attrition rates appear to be in those groups with unusually high or low income (compared with the bulk of our sample) to start with. Hence, there does not seem to be any reason to expect that attrition has masked a predominance of income increases among the control families.

In a number of very important respects the evidence from this preliminary and crude analysis of the earliest results is less than ideal. If there were other evidence, approaching the relevance of these data but having fewer problems, it would be highly questionable whether an attempt to interpret and use the New Jersey data currently available should be made. Such is not the case, however, and as a consequence (at risk of being premature) we have tried to be responsibly responsive to a pressing public need for information. That response is simple: No evidence has been found in the urban experiment to support the belief that negative-tax-type income maintenance programs will produce large disincentives and consequent reductions in earnings.

98-71

INSTITUTE FOR RESEARCH ON POVERTY

MID-EXPERIMENT REPORT ON BASIC LABOR-SUPPLY RESPONSE

by

Harold W. Watts

DISCUSSION PAPERS

THE UNIVERSITY OF WISCONSIN, MADISON, WISCONSIN

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May 1971

ABSTRACT

This paper reports basic descriptive summaries of the New Jersey/ Pennsylvania Graduated Work Incentives Experiment at the point where the full sample data for the first year of operation could be processed. Because of lags in enrollment, it is also possible to report here data for the first year and a half for the "half sample," i.e., Trenton, Paterson, and Passaic. Improvements in the data base enable the present report to concentrate on means and regression results instead of on the cruder tabular analysis of discrete change categories.

In summary, the results indicate a continuation of the earlier findings on earnings change i.e., no significant difference between control and experimental families. There are significant differences, however, in two alternative indicators of labor supply: (1) persons employed per family, and (2) hours worked per family. These differences indicate fewer workers or hours for the experimental families as static labor-supply theory would predict. There is also a differential in average hourly earnings tht reconciles the different indications given by earnings and hours. At this point there have appeared no obvious patterns within the experimental group but that question has not yet been sufficiently explored to warrant rejection of any hypothesis.

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May 1971

Mid-experiment Report on Basic Labor-Supply Response

by

Harold W. Watts*

The data in this progress report represent the first descriptive summaries obtained from system-produced longitudinal extracts of the basic (core) segment of the data file. A bibliography listing papers describing the experiment's origin, purpose, and basic design is provided in Appendix I.

Last spring the first results, covering less than a year's experience for only the first half of the sample (sites were phased in over a one-year period) were released. It is now possible to go beyond that to cover the entire sample for the first experimental year, and that same half for the first 18 months. In addition to the increased coverage we can say the data are more complete (more variables and all intervening quarterly values) and more thoroughly checked, edited, and "cleaned."

This improvement in the data base has enabled us to begin to use mean values and regressions (used primarily as descriptive devices for

*I cannot acknowledge all those who have contributed to the production of the data and analysis reported herein without a footnote longer than the report itself. But, without prejudice to the larger number, I here acknowledge with gratitude the following persons who have contributed extra effort to make this report possible. From MATHEMATICA I must thank David Kershaw, Jeri Fair, Marsha Shore, Frank Mason, Regina Pasche, Albert Rees, Glen Cain (on leave from Wisconsin), Robinson Hollister, Audrey Macdonald; from OEO, Thomas Glennan; and from Wisconsin, Nancy Williamson, Michael Watts, Claudio Frischtak, Felicity Skidmore and Margaret Witte.

obtaining conditional means) instead of the cruder tabulations of discrete changes that of necessity formed the principal evidence in the earlier report. The results discussed in the body of the current report will therefore be presented as means, adjusted means, and regression coefficients. (For comparison with the earlier results, Appendix II presents and discusses "change" tables of the kind used in the first preliminary report. In general, however, the two methods produce the same view of the basic outcomes that will be discussed below.)

DESCRIPTION OF THE DATA BASE

This analysis has been limited to a small number of the most basic indicators of labor supply and earnings. We have worked with the labor force status, hours worked, and earnings (all for the week preceding every interview) for each head and spouse, and an aggregate of any other adults (persons over 16) that are in the household unit. These data, in addition to family size, number of adults, number of children, and welfare status, are available from each successive quarterly interview, as well as the pre-enrollment interview (which was administered before families were notified of the experiment's existence). Besides the panel data on the above variables, the following static variables are available: city, ethnicity, age of head and spouse, average earnings, and weeks worked in year before enrollment, and finally, the family's designation either as a control family or as an experimental family assigned to one of the eight experimental treatments.

Any longitudinal study loses some of the families originally enrolled along the way. Most of the results cited here are for families that either completed a full year with no more than one missed interview (not the last one) for the "full sample" or completed the sixth quarterly and missed no more than one other for the "half sample." These will be termed the "continuous families." The excluded families represent the loss from panel attrition, and one important part of further work must be the analysis of possible biases produced by this loss. So far the losses have produced no significant changes from the distribution of the sample at the start, although the attrition rates are not quite constant over all subgroups of the sample. The loss so far amounts to 138 families from the original 1213, leaving 1075 "continuous" families to be analyzed (395 out of 505 for the half sample). While a cursory review of the nature of the losses has not uncovered any "drastic" disparity that would overturn the findings cited, neither can it be said that the losses have been analyzed as much as should or can be done. This is the first of a number of cautionary statements in this paper, warning against overinterpreting these early looks at the data. We do have a large amount of partial information on all these families and eventually can expect to remove much of the uncertainty they cause.

Within the continuous family sample two subgroups have been analyzed separately: the nonwelfare subgroup and the husband-wife subgroup. The nonwelfare subsample is defined as those families who reported welfare benefits for at most one of the quarters not including the last one. This subgrouping excludes 250 of the 1075 (23 percent) continuous families. It is not offered as a satisfactory means of purifying the results of the effect of the various public-assistance programs. That will require a much more highly structured analytic model than is attempted here. But the subsample is useful in that it provides some confirmation that tendencies do not disappear when the welfare group is excluded and may perhaps also provide some empirical guidance to the badly-needed development of more satisfactory analyses. Here then is an additional reason for caution in generalizing from any tendencies discussed below. The interference coming from the sample families' behavior toward the alternatives provided by welfare has not been well specified theoretically and has therefore not been partialled out of any results obtained so far.

The husband-wife family is of interest because this most typical and most numerous type of family is easier for most of us to reason about introspectively. It is also the group for which continuous individual persons (the head and spouse) can be most readily identified for meaningful disaggregation of the family aggregates used for analyzing the other subgroups. The 943 families in this subsample, then, are an important group in themselves. Since they also dominate the total sample, we can use them as the most logical and the easiest place to begin looking at individual behavior in a family (or household) setting.

CRUDE TIME SERIES

Tables 1-7 display means of the primary indicators of labor supply and earnings for the various samples described above. Tables 1 and 3 contain the control/experimental contrasts and a three-way ethnic breakdown for the total full and half samples respectively. The ethnic categorization is not exhaustive (a small group of unclassified cases are left out). The white group is surely heterogeneous ethnically and is best described as the non-black and non-Puerto Rican group. It should also be noted that the bulk of the "white" group is from the Scranton, Pennsylvania, site; virtually none of the other two groups are represented in Scranton; and "whites" are underrepresented in all the other sites. This imbalance is discussed further on pp. 31-34 below.

Tables 2 and 4 show breakdowns (for the full sample and half sample respectively) within the experimental group by generosity of plan. Because the families, on average, earn very close to the poverty line,[1] the plans have been classified by size of benefits paid when family income is <u>at</u> the poverty line. The lowest category pays no more than 5 percent of the poverty line in benefits at that income level, and the highest pays 75 percent.

Tables 5 and 6 show the control/experimental contrast for the non-welfare subsample only, and for the head and spouse of the husband-wife subsample. Table 7 shows the movement of the same variables in the four separate experimental sites.

The mean values in these tables have been calculated from the "usable" responses only, and as a consequence the number of families included in each mean will vary slightly from quarter to quarter. The loss from such scattered unusable responses rarely exceeds 3 percent.

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[1]The poverty line equaled $3300 for a family of 4 at the start of the experiment, subsequently inflated in pace with the consumer price index.

TABLE 1

LABOR SUPPLY AND EARNINGS MEANS FOR CONTROL AND EXPERIMENTAL GROUPS AND FOR ETHNIC GROUPS

First Year--Full Sample--Continuous Families

	Qtr.	Control (422)	Experimental (653)	White (387)	Black (386)	Spanish (272)
No. of employed persons/ family	0	1.08	1.14	1.15	1.11	1.10
	1	1.15	1.05	1.14	1.09	1.01
	2	1.16	1.09	1.16	1.12	1.00
	3	1.16	1.04	1.14	1.09	1.02
	4	1.18	1.02	1.11	1.12	0.96
Total hours/family	0	39.4	39.8	41.7	37.8	39.4
	1	40.8	36.7	39.7	36.7	37.8
	2	37.0	34.9	38.5	35.8	31.9
	3	39.6	36.9	39.0	37.1	37.0
	4	40.3	35.0	39.6	36.3	33.6
Total earnings/ family	0	87.74	88.84	94.89	86.13	81.99
	1	94.28	91.81	96.82	90.01	88.81
	2	88.90	88.88	94.94	91.73	76.58
	3	96.13	96.98	98.78	95.63	92.35
	4	96.65	94.03	100.92	94.91	82.96
Average earnings/ hour	0	2.23	2.23	2.28	2.28	2.08
	1	2.31	2.50	2.43	2.45	2.35
	2	2.40	2.55	2.47	2.56	2.40
	3	2.43	2.63	2.53	2.58	2.50
	4	2.40	2.69	2.55	2.61	2.47

TABLE 2

LABOR SUPPLY AND EARNINGS MEANS WITHIN EXPERIMENTAL GROUP--CLASSED BY POVERTY-LEVEL BENEFIT (B_p)

First Year--Full Sample--Continuous Families

	Qtr	B_p= 0,5 (139)	B_p=20,25,30 (224)	B_p=45,50 (162)	B_p=75 (128)
No. of employed persons/ family	0	1.09	1.14	1.16	1.17
	1	1.08	1.04	1.07	1.02
	2	1.06	1.08	1.11	1.09
	3	0.99	1.05	1.08	1.01
	4	0.99	1.03	1.04	1.02
Total hours/ family	0	38.2	39.7	41.1	39.9
	1	36.5	36.9	36.0	37.5
	2	33.0	34.3	35.4	37.3
	3	33.1	38.4	38.5	36.3
	4	34.4	35.1	35.8	34.5
Total earnings/ family	0	83.06	89.27	91.14	91.33
	1	91.69	92.28	90.47	92.80
	2	82.72	88.47	87.12	98.76
	3	90.29	100.43	100.22	94.06
	4	95.12	92.02	95.70	94.32
Average earnings/ hour	0	2.17	2.25	2.22	2.29
	1	2.51	2.50	2.51	2.47
	2	2.51	2.58	2.46	2.65
	3	2.73	2.62	2.60	2.59
	4	2.76	2.62	2.67	2.73

TABLE 3

LABOR SUPPLY AND EARNINGS MEANS FOR CONTROL AND EXPERIMENTAL GROUPS AND FOR ETHNIC GROUPS

First 6 Quarters--Half Sample--Continuous Families

	Qtr.	Control (100)	Experi-mental (295)	White (40)	Black (185)	Spanish (151)
No. of employed persons/ family	0	0.99	1.13	1.05	1.10	1.11
	1	1.08	1.07	1.07	1.10	1.03
	2	1.09	1.06	1.02	1.09	0.99
	3	1.16	1.01	1.17	1.06	1.00
	4	1.16	0.99	1.10	1.09	0.92
	5	1.16	1.00	1.00	1.11	0.94
	6	1.07	0.97	0.95	1.03	0.91
Total hours/family	0	34.7	37.7	38.1	35.9	38.0
	1	38.2	35.1	35.9	35.4	35.8
	2	30.3	31.8	34.7	33.1	28.3
	3	38.7	35.7	44.7	34.5	35.4
	4	38.8	33.6	37.9	34.7	33.3
	5	39.0	34.4	31.3	36.8	33.8
	6	35.6	32.7	33.8	34.1	30.9
Total earnings/ family	0	74.66	81.74	95.79	75.73	79.96
	1	88.68	84.52	88.20	83.80	84.99
	2	73.07	78.69	93.58	82.30	56.68
	3	93.60	94.37	120.08	86.57	92.59
	4	92.71	89.96	105.25	88.55	84.87
	5	94.18	94.03	93.72	97.25	86.19
	6	87.35	89.46	97.92	90.79	78.98
Average earnings/ hour	0	2.15	2.17	2.51	2.11	2.10
	1	2.32	2.41	2.50	2.37	2.37
	2	2.41	2.47	2.70	2.49	2.36
	3	2.42	2.64	2.69	2.51	2.62
	4	2.39	2.68	2.78	2.55	2.55
	5	2.41	2.74	2.99	2.64	2.55
	6	2.46	2.73	2.90	2.66	2.56

TABLE 4

LABOR SUPPLY AND EARNINGS MEANS WITHIN EXPERIMENTAL GROUP--CLASSED BY POVERTY-LEVEL BENEFIT (B_p)

First 6 Quarters--Half Sample--Continuous Families

	Qtr	B_p = 0,5 (72)	B_p = 20,25,30 (117)	B_p = 45,50 (77)	B_p = 75 (29)
No. of employed persons/family	0	1.17	1.09	1.12	1.21
	1	1.12	1.10	1.00	1.00
	2	1.07	1.08	1.04	1.00
	3	0.94	1.05	0.99	1.03
	4	0.94	1.01	0.99	1.03
	5	0.94	1.02	1.01	1.03
	6	1.01	0.97	0.95	0.90
Total hours/family	0	41.1	36.9	36.7	35.4
	1	36.1	37.6	30.8	33.2
	2	32.7	32.7	30.1	30.8
	3	33.6	38.5	34.2	33.3
	4	31.9	34.4	34.0	33.3
	5	31.9	36.3	35.1	30.7
	6	35.2	32.2	32.2	30.4
Total earnings/family	0	88.61	83.27	77.28	69.73
	1	86.39	91.17	73.50	81.38
	2	80.28	82.12	70.19	83.84
	3	92.34	100.53	87.56	92.07
	4	88.67	93.18	85.65	90.89
	5	86.03	101.11	94.28	84.57
	6	93.18	87.73	89.26	87.64
Average earnings/hour	0	2.16	2.26	2.10	1.97
	1	2.39	2.42	2.39	2.45
	2	2.46	2.51	2.33	2.72
	3	2.74	2.61	2.56	2.76
	4	2.78	2.71	2.52	2.72
	5	2.70	2.78	2.69	2.75
	6	2.65	2.72	2.77	2.88

TABLE 5

LABOR SUPPLY AND EARNINGS MEANS FOR CONTROL AND EXPERIMENTAL GROUPS IN NON-WELFARE AND HUSBAND-WIFE SUBGROUPS

First Year, Full Sample

		Non-Welfare		Husband-Wife Families			
				Husbands		Wives	
	Qtr	Ctl (324)	Exp (501)	Ctl (372)	Exp (571)	Ctl (372)	Exp (571)
No. employed/ family (or % employed)	0	1.10	1.16	0.90	0.89	0.10	0.15
	1	1.20	1.11	0.85	0.87	0.15	0.13
	2	1.20	1.16	0.88	0.90	0.14	0.12
	3	1.24	1.12	0.90	0.89	0.17	0.12
	4	1.27	1.11	0.88	0.86	0.16	0.14
Total hours/family (or per head or per spouse)	0	40.2	41.2	34.5	33.6	2.8	4.0
	1	42.5	39.3	33.8	33.0	4.5	3.4
	2	39.3	38.2	31.3	31.3	3.8	3.0
	3	43.1	40.6	33.8	33.9	4.7	3.2
	4	44.7	39.2	33.6	31.5	4.7	3.9
Total earnings/ family (or per head or spouse)	0	91.92	92.63	81.01	79.69	5.39	6.65
	1	100.80	99.03	82.40	88.34	8.40	6.79
	2	96.71	99.35	79.57	84.52	7.75	6.09
	3	106.50	108.53	86.61	93.87	9.46	6.48
	4	108.60	106.55	86.60	89.06	9.05	8.04
Average earnings/ hour	0	2.29	2.25	2.35	2.37	1.92	1.66
	1	2.37	2.52	2.44	2.68	1.87	2.00
	2	2.46	2.60	2.54	2.70	2.04	2.03
	3	2.47	2.67	2.56	2.77	2.01	2.02
	4	2.43	2.72	2.58	2.83	1.93	2.06

TABLE 6

LABOR SUPPLY AND EARNINGS MEANS FOR CONTROL AND

EXPERIMENTAL GROUPS IN NON-WELFARE AND HUSBAND-WIFE SUBGROUPS

First 6 Quarters, Half Sample

		Non-Welfare		Husband-Wife Families			
				Husbands		Wives	
	Qtr	Ctl (75)	Exp (200)	Ctl (78)	Exp (238)	Ctl (78)	Exp (238)
No. employed/ family (or % employed)	0	1.05	1.18	0.85	0.89	0.12	0.15
	1	1.17	1.18	0.85	0.89	0.18	0.15
	2	1.23	1.18	0.87	0.89	0.12	0.13
	3	1.31	1.13	0.85	0.84	0.23	0.13
	4	1.33	1.13	0.86	0.82	0.21	0.16
	5	1.31	1.12	0.86	0.85	0.23	0.16
	6	1.29	1.08	0.83	0.83	0.14	0.16
Total hours/ family (or per head or spouse)	0	36.8	39.5	31.1	32.4	3.7	3.8
	1	42.0	39.2	33.0	31.6	5.7	4.3
	2	35.6	36.6	27.5	29.1	3.7	2.9
	3	44.4	41.4	34.2	33.3	6.6	3.8
	4	44.7	39.5	33.0	30.8	5.9	4.8
	5	45.6	39.3	33.7	32.1	5.7	4.2
	6	42.1	36.8	29.8	30.0	5.0	4.9
Total earnings/ family (or per head or spouse)	0	79.06	86.40	72.96	75.21	5.47	6.22
	1	96.41	94.51	82.22	82.17	8.65	7.99
	2	85.01	93.49	70.61	76.70	7.00	5.73
	3	107.66	111.43	89.42	93.88	12.56	7.78
	4	107.81	107.20	85.63	87.73	11.64	9.93
	5	112.62	109.64	88.30	95.15	10.45	7.71
	6	104.27	102.66	80.77	87.85	8.38	8.78
Average earnings/ hour	0	2.15	2.19	2.34	2.32	1.48	1.66
	1	2.30	2.41	2.49	2.60	1.52	1.88
	2	2.39	2.55	2.57	2.63	1.89	1.99
	3	2.42	2.69	2.61	2.82	1.90	2.06
	4	2.41	2.71	2.59	2.85	1.97	2.06
	5	2.47	2.79	2.62	2.97	1.83	1.84
	6	2.48	2.79	2.71	2.93	1.68	1.81

TABLE 7

LABOR SUPPLY AND EARNINGS MEANS
FOR EXPERIMENTAL SITES

		Continuous 0-6 Families		Continuous 0-4 Families	
	Qtr	Tren-ton (96)	Pat.-Pass. (299)	Jersey City (355)	Scran-ton (307)
No. of employed persons/family	0	1.19	1.06	1.09	1.17
	1	1.09	1.07	1.01	1.17
	2	1.00	1.09	1.10	1.17
	3	1.06	1.04	1.09	1.13
	4	1.07	1.02	1.10	1.12
	5	0.98	1.06	-	-
	6	0.92	1.02	-	-
Total hours/family	0	41.4	35.5	39.7	42.7
	1	37.1	35.4	38.8	40.6
	2	35.6	30.1	38.5	38.5
	3	38.5	35.7	39.4	38.3
	4	34.4	35.1	37.1	39.8
	5	35.0	35.7	-	-
	6	32.8	33.7	-	-
Total earnings/family	0	87.00	77.62	92.58	94.35
	1	85.28	85.65	98.88	94.98
	2	83.38	75.30	100.64	90.99
	3	92.14	94.85	103.26	93.69
	4	81.19	93.44	97.64	98.23
	5	89.99	95.38	-	-
	6	85.80	89.95	-	-
Average earnings/hour	0	2.10	2.19	2.33	2.21
	1	2.30	2.42	2.55	2.34
	2	2.34	2.50	2.61	2.36
	3	2.39	2.66	2.62	2.45
	4	2.36	2.66	2.63	2.47
	5	2.57	2.67	-	-
	6	2.62	2.67		

The average hourly earnings have been calculated as the ratio of mean earnings to mean hours from the corresponding table entries. They should not be regarded as simple wage rates.

In these tables we can note first of all a divergent trend between controls and experimentals in the number of persons employed per family. This is evident in the first table and is substantiated in the different subsamples that follow. The tendency does not appear to be very strong for the husbands in husband-wife families, but it is prominent for the wives. No very obvious differences within the experimental group show up, however. In terms of the entire sample, whites appear to have the largest number of employed persons per family, with black families next and Spanish-speaking families third. This result appears to be produced by the Scranton families in the white subsample, because in the half sample which excludes them the whites appear to be generally below the blacks in terms of this variable. There is also some indication of a loosening in the labor market evidenced by control husbands' decline in employment. This is supported, on the "added worker" hypothesis, by the opposite behavior of the control wives.

Total hours worked by all family members show very similar patterns of movement. Again a differential appears between controls and experimentals. The Puerto Rican families manage to get in the fewest number of hours, but all groups appear to be affected by unemployment and/or short weeks.

Turning to total earnings the picture does seem to be different. Here we have a generally increasing overall trend in earnings per family but we do not find any divergence between control and experimental families. The inevitable consequence of this must be that average hourly earnings move in such a way as to offset the divergence of total family hours. The average hourly earnings figures in the last segment of several of the tables confirm this. Once again readily discernible patterns have not appeared within the experimental group. Earnings levels in general are lower for Puerto Ricans. Hourly earnings are also higher in the full sample for blacks than for (non-Puerto Rican) whites, but since this more than vanishes in the half sample it is again due to Scranton--where generally lower wage levels prevail (see Table 7).

ANALYSIS OF ADJUSTED MEANS

The tendencies visible in the sequence of mean values in the first six tables are more precisely estimated in Tables 8 and 9. These tables are based on simple control/experimental differentials estimated in "dummy" variable regressions which control for experimental site, ethnicity, and pre-enrollment value of the variable in question.[2]

These regressions were fitted for number of persons employed, total hours worked, and total earnings for the family aggregates; and within husband-wife families separately for the husband, wife, and other earners.

The adjusted means for control and experimental families shown in the tables are adjusted in the sense that each represents the regression value for the variable for a control (or experimental) family

[2]See Appendix III (Technical Notes) for a full discussion.

TABLE 8

ADJUSTED MEAN ESTIMATES DERIVED FROM REGRESSION ESTIMATES OF

DIFFERENTIALS IN EMPLOYMENT, HOURS, AND EARNINGS

Husband-Wife Families

	(1) # employed per family	(2) Hours per employee	(3) Hours per family	(4) Earnings per hour	(5) Earnings per family
Family total:					
Control mean	1.242	34.4	42.67	2.45	104.36
Abso. diff.	- .151**	+ .1	- 5.02**	+ .22	- 3.76
Exper. mean	1.091	34.5	37.65	2.67	100.60
% differ.	-12.2%	+ .3%	-11.8%	+9.0%	- 3.6%
Husband:					
Control mean	.885	37.9	33.55	2.61	87.52
Abso. diff.	- .032	- 1.0	- 2.09	+ .20	+ .75
Exper. mean	.853	36.9	31.46	2.81	88.27
% differ.	- 3.6%	- 2.6%	- 6.2%	+7.7%	+ .9%
Wife:					
Control mean	.176	28.6	5.03	1.92	9.66
Abso. diff.	- .044	- .1	- 1.27	+ .14	- 1.93
Exper. mean	.132	28.5	3.76	2.06	7.73
% differ.	-25.0%	- .4%	-25.2%	+7.3%	- 20.0%
Other earner:					
Control mean	.180	23.0	4.08	1.76	7.17
Abso. diff.	- .075**	+ 3.1	- 1.66*	+ .14	- 2.58
Exper. mean	.105	26.1	2.42	1.90	4.59
% differ.	-41.7%	+13.5%	-40.7%	+8.0%	- 36.0%

NOTE: The fourth quarterly means cited above have been adjusted, by use of regression analysis, for the differing composition of the control and experimental groups in terms of location, ethnicity, age, family size, and pre-enrollment value of the variable in question. These means, and the associated control-experimental differentials, may therefore be interpreted as applicable to control and experimental groups with identical composition in terms of these variables. Percent differentials are computed using the mean of the control as the base. Slight differences from the equivalent table in OEO's May 1971 release were produced by reruns on a corrected version of the tape. For detailed explanation see Appendix III.

*Significant at the .95 level. **Significant at the .99 level.

TABLE 9

ADJUSTED MEAN ESTIMATES DERIVED FROM REGRESSION ESTIMATES OF

EXPERIMENTAL DIFFERENTIALS IN EMPLOYMENT, HOURS, AND EARNINGS

Family Totals - Alternate Samples

	(1) # employed per family	(2) hours per employee	(3) hours per family	(4) earnings per hour	(5) earnings per family
All continuous families					
Control mean	1.18	33.6	39.7	2.42	96.09
Abso. diff.	- .16**	+ 1.3	- 4.0**	+ .22	- 2.02
Exper. mean	1.02	34.9	35.7	2.64	94.07
% differ.	-13.5%	+ 4.0%	-10.1%	+ 9.1%	- 2.1%
All continuous "non-welfare" families					
Control mean	1.29	34.8	44.8	2.43	108.95
Abso. diff.	- .19**	+ .1	- 6.4**	+ .27	- 5.35
Exper. mean	1.10	34.9	38.4	2.70	103.60
% differ.	-14.4%	+ .3%	-14.3%	+11.1%	- 4.9%
The balance in "welfare" families					
Control mean	.86	27.6	23.8	2.29	54.62
Abso. diff.	- .15	+ 2.0	- 2.8	+ .15	- 3.34
Exper. mean	.71	29.6	21.0	2.44	51.28
% differ.	-17.7%	+ 7.2%	-11.7%	+ 6.6%	- 6.1%
Half sample continuous families					
Control mean	1.15	33.0	38.0	2.44	92.66
Abso. diff.	- .17*	+ .7	- 5.0*	+ .26	- 3.52
Exper. mean	.98	33.7	33.0	2.70	89.14
% differ.	-15.1%	+ 2.4%	-13.1%	+10.7%	- 3.8%

NOTE: See note for Table 8 and Appendix III.

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having the same (sample average) values for all of the other variables in the regression--i.e., they would be identical to the crude means in a sample that was exactly balanced between control and experimental groups. The entries for hours per employee and earnings per hour have been calculated from the adjusted means in adjacent columns, and from them the absolute and percentage differences have been derived.

In Table 8 the family aggregates are shown and also broken down for the husband-wife families into components attributable specifically to the husband, the wife, and the total of any other earners there may be in the family.

The family aggregates in the first segment of the table indicate significant negative differentials for both employment per family and total hours. Quantitatively, experimental families are approximately 12 percent below control families in both respects. The differential for total earnings per family is much smaller (3%) and is not significantly different from zero. These coefficients agree with the observed tendencies discussed above in the tables of means. And once again there is implied a sharp difference in the movement of average hourly earnings--nearly 10 percent.

The lower part of the table shows three components of these family totals--husband, wife, and "other earners." Here we note that the largest differential in employment (and the only component which is significant) is that for "other earners." This makes up half the total family differential. Just over half the balance is accounted for by the wife. The reason this difference represents such a large percentage change is because the average employment rate of spouses is relatively small (one out of six for the controls). It should be noted here that the principle of sample selection used has been partly responsible for the small fraction of employed wives. Families with total incomes above 150 percent of the poverty line were not eligible and this made it differentially hard for two-earner families to get into the experiment in the first place, even though the husband might be a relatively low-wage or "poor" earner.

When we consider hours, we find that two-fifths of the differential is accounted for by the husband's apparent response, although the only statistically significant differential is again for the "other earners" who account for one-third of the total. The most marked differential in hours per worker occurs also for the "other earners," and this moderates their reduction in total hours. The quite minor and statistically non-significant difference in earnings is compounded from a minute positive effect for the husband, offset by roughly equal-sized negative ones for the wife and the "other earners." These movements of components imply very similar (7 percent) positive differences in average hourly earnings for each of the three parts of the family total. The higher 10 percent increase for the total comes about because of the compositional difference whereby the husband's hours or earnings become a larger fraction of the total (made up, of course, of husbands plus wives plus other earners).

Table 9 shows the results for alternate samples of families. The results discussed earlier from Table 8 were for the husband-wife families, which are only a subset of the sample of continuous families shown in the first section of Table 9. The results here are qualitatively very similar to those previous findings—not a surprising finding since husband-wife families make up 88 percent of all the continuous families.

The next two segments of the table show comparable regressions for "non-welfare" and "welfare" families separately.[3] The larger "non-welfare" subsample again displays the same general pattern of results for the same reasons. But the smaller group of "welfare" families, after allowing for the reduction in statistical precision, again shows a very consistent pattern of experimental differentials. While the differentials for hours and employment are not significant here, they are of similar sign and percentage magnitude. Finally, the same basic pattern emerges if only the half sample (Trenton and Paterson-Passaic) is used and when, moreover, the values from the fourth, fifth, and sixth quarterlies are taken into account to get a more stable indication of family response.[4]

The evidence reviewed so far does add up to an indication of substantial and significant negative differences in employees per family for the experimentals. In terms of family aggregates these amount (the small so-called welfare group aside) to 12-15 percent. This reduction in employment is partly offset by positive differences in hours per employee, so that the similar range of experimental differentials in hours is 9-14 percent. Larger offsetting differences (in the neighborhood of 10 percent for the family aggregates) completely eliminate the significance of the differential in family earnings.

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[3]For definitions see Appendix III.

[4]See Appendix III.

Inspection of Tables 1-7 suggests that much of the presumed response occurred between the pre-enrollment and first quarterly observations. This leads to asking whether all the significance in the results cited above comes from adjustments during the first quarter, with no subsequent adjustment large enough to meet the criterion of statistical significance. To test this, additional regressions were carried out, producing the adjusted means shown in Table 10. These new regressions examine the differentials at the first quarter, including among the adjustments the value of the same variable at the time of pre-enrollment, and as a second step show the differentials at the fourth quarter after adjustment for whatever differences already existed at the first quarter.

These estimates verify that the largest single adjustment did indeed take place during the first quarter, but they also show an equally significant and quantitatively roughly equal change over the third-quarter period from the first quarterly to the fourth. In neither case do we observe significant differences in earnings. Consequently the same positive differential in average hourly earnings is observed over both comparisons.

FURTHER EXPLORATION

Two aspects of the results cited above have been explored further in an effort to get a more complete picture of the nature of the divergences between control and experimental groups. First, there is the question whether the observed difference in hours and employment for experimental families is caused by a few persons who either leave

TABLE 10

ADJUSTED MEAN ESTIMATES

Intermediate Adjustments from Pre-enrollment to First Quarter
and First Quarter to Fourth Quarter

	(1) # employed per family	(2) hours per employee	(3) hours per family	(4) earnings per hour	(5) earnings per family
First quarter means holding pre-enrollment value constant					
Control mean	1.151	35.0	40.2	2.32	93.26
Abso. diff.	- .103**	+ .1	- 3.5*	+ .18	- 1.38
Exper. mean	1.048	35.1	36.7	2.50	91.88
% differ.	-9.0%	+ .3%	- 8.7%	+7.8%	- 1.5%
Fourth quarter means holding first quarter value constant					
Control mean	1.126	34.0	38.3	2.48	94.99
Abso. diff.	- .099**	+ .4	- 3.1*	+ .17	- 1.83
Exper. diff.	1.024	34.4	35.2	2.65	93.16
% diff.	-8.9%	+ 1.1%	- 8.1%	+6.8%	1.9%

NOTE: See note for Table 8 and Appendix III.

or stay out of the labor force, or whether it is a more pervasive incremental change in behavior. Second, one may ask how the distribution of average hourly earnings has changed for the control and experimental families to produce the apparent difference in favor of the latter.

The first question has been examined by asking how many husbands in husband-wife families who were employed at the outset were found to be employed at each of the interviews conducted 6, 9, and 12 months later. They are tabulated in Table 11(A) according to whether such husbands were found employed at 3, 2, 1, or none of the successive periods. As can be seen, there is no evident tendency for the experimental group to gain "retirement cases" relative to the control group. The excess of "notemployed" appears rather to be spread out over many persons who are out of work for shorter periods. Table 11(B) is tabulated in a comparable way for husbands who were not employed initially. Here again there is no suggestion that the overall reduction is concentrated in a few dropouts.

The second question has been approached by looking at the distributions of average hourly earnings for husband-wife families and for the husband and wife separately within such families. Table 12(A) indicates (for experimentals and controls) how family average hourly earnings were distributed at pre-enrollment and at fourth quarterly. Table 12(B) shows how each pre-enrollment group had changed its distribution by the fourth quarterly.

Tables 13 and 14 show comparable tables for the earnings status and changes in it for the head and spouse respectively. These tables indicate that there was a tendency for average hourly earnings to

TABLE 11

EMPLOYMENT STATUS OF HEAD

Husband-Wife Families

A. Of Those Employed at Pre-enrollment

	Control		Experimental		Total	
	No.	%	No.	%	No.	%
Employed at none of quarterlies 2, 3, 4	8	2.4	8	1.6	16	1.9
Employed at one of quarterlies 2, 3, 4	15	4.5	19	3.7	34	4.0
Employed at two of quarterlies 2, 3, 4	31	9.3	58	11.4	89	10.6
Employed at all of quarterlies 2, 3, 4	280	83.8	425	83.3	705	83.5
TOTAL	334	100.0	510	100.0	844	100.0

B. Of Those Not Employed at Pre-enrollment

	Control		Experimental		Total	
	No.	%	No.	%	No.	%
Employed at none of quarterlies 2, 3, 4	9	23.7	17	27.9	26	26.3
Employed at one or two of quarterlies 2, 3, 4	12	31.6	20	32.8	32	32.3
Employed at all of quarterlies 2, 3, 4	17	44.7	24	39.3	41	41.4
TOTAL	38	100.0	61	100.0	99	100.0

TABLE 12

A. INITIAL AND 4TH QUARTER DISTRIBUTION OF HUSBAND-WIFE FAMILIES: BY FAMILY AVERAGE HOURLY EARNINGS

	Status at Start				Status at Fourth			
	Control		Experimental		Control		Experimental	
	No.	%	No.	%	No.	%	No.	%
No one employed & not available	48	12.9	78	13.6	54	14.5	102	17.9
$2.25 or less	160	43.0	248	43.4	127	34.1	248	43.4
$2.26-$3.50	150	40.3	226	39.6	168	45.2	226	39.6
More than $3.50	14	3.8	19	3.3	23	6.2	19	3.3
TOTAL	372	100.0	571	100.0	372	100.0	571	100.0

B. BREAKDOWN OF MOVEMENTS IN FAMILIES' AVERAGE HOURLY EARNINGS FROM PRE-ENROLLMENT TO FOURTH QUARTER

	Control				Experimental			
	No one empl. & NA	$2.25 or	$2.26 to $3.50	More than $3.50	No one empl. & NA	$2.25 or less	$2.26 to $3.50	More than $3.50
Status at start	N=48	N=160	N=150	N=14	N=78	N=248	N=226	N=19
Breakdown by 4th quarter status:								
No one employed & not available	25.0 %	12.5 %	12.6 %	21.4 %	30.8 %	15.7 %	15.9 %	15.8 %
$2.25 or less	35.4	50.6	18.7	7.1	15.4	31.9	12.0	5.3
$2.26-$3.50	31.3	34.4	62.7	28.6	41.0	46.8	59.3	36.8
More than $3.50	8.3	2.5	6.0	42.9	12.8	5.6	12.8	42.1
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

TABLE 13

A. INITIAL AND 4TH QUARTER DISTRIBUTION OF HUSBAND-WIFE FAMILIES: BY AVERAGE HOURLY EARNINGS OF HUSBAND

	Status at Start				Status at Fourth			
	Control		Experimental		Control		Experimental	
	No.	%	No.	%	No.	%	No.	%
No one employed & not available	58	15.6	96	16.9	71	19.1	130	22.8
$2.25 or less	141	37.9	215	37.6	102	27.4	91	15.9
$2.26-$3.50	159	42.7	238	41.7	173	46.5	283	49.6
More than $3.50	14	3.8	22	3.8	26	7.0	67	11.7
TOTAL	372	100.0	571	100.0	372	100.0	571	100.0

B. BREAKDOWN OF MOVEMENTS IN HUSBAND'S AVERAGE HOURLY EARNING FROM PRE-ENROLLMENT TO 4TH QUARTER

	Control				Experimental			
	No one empl. & NA	$2.25 or less	$2.26 to $3.50	More than $3.50	No one empl. & NA	$2.25 or less	$2.26 to $3.50	More than $3.50
Status at start	N=58	N=141	N=159	N=14	N=96	N=215	N=238	N=22
Breakdown by 4th quarter status:								
No one employed & not available	44.9 %	14.2 %	13.8 %	21.4 %	50.0 %	15.8 %	18.9 %	13.6 %
$2.25 or less	24.1	46.1	13.9	7.1	10.4	30.2	6.7	0.0
$2.26-$3.50	24.1	36.2	65.4	28.6	27.1	46.5	61.8	45.5
More than $3.50	6.9	3.5	6.9	42.9	12.5	7.5	12.6	40.9
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

TABLE 14

A. INITIAL AND 4TH QUARTER DISTRIBUTION OF HUSBAND-WIFE FAMILIES: BY AVERAGE HOURLY EARNINGS OF WIFE

	Status at Start				Status at Fourth			
	Control		Experimental		Control		Experimental	
	No.	%	No.	%	No.	%	No.	%
No one employed & not available	335	90.1	499	87.4	317	85.2	500	87.6
$2.00 or less	28	7.5	53	9.3	38	10.2	38	6.6
More than $2.00	9	3.4	19	3.3	17	4.6	33	5.8
TOTAL	372	100.0	571	100.0	372	100.0	571	100.0

B. BREAKDOWN OF MOVEMENTS IN WIFE'S AVERAGE HOURLY EARNINGS FROM PRE-ENROLLMENT TO 4TH QUARTER

	Control			Experimental		
	No one empl. & NA	$2.00 or less	More than $2.00	No one empl. & NA	$2.00 or less	More than $2.00
Status at start	N=335	N=28	N=9	N=499	N=53	N=19
Breakdown by 4th quarter status:						
No one employed & not available	89.8 %	42.9 %	44.4 %	92.8 %	52.8 %	47.3 %
$2.00 or less	8.1	39.3	0.0	3.8	32.1	10.5
More than $2.00	2.1	17.8	55.5	3.4	15.1	42.2
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0

increase over the first year of experience for both experimentals and controls, but that the shift in distribution was much greater for the experimental group. (Two statuses do not permit calculation of hourly earnings here. One, of course, is where no member of the family is employed. The other is where some information is missing. These two categories are shown together, along with a three-way division of the computable hourly earnings. These again have been calculated by simply dividing total hours worked into total family earnings.)

Tables 15(A) and 16(A) indicate how hours worked per week were distributed at pre-enrollment and at fourth quarterly for husbands and wives respectively. Tables 15(B) and 16(B) show how each pre-enrollment group had changed its distribution by the fourth quarterly. Clearly the likelihood of gaining or retaining full-time work is much higher for husbands than for wives, and it is also the case that less than one third of the thirty-nine wives employed full time at enrollment were still so employed a year later (at the 4th quarterly), even though there was a net increase of nine full-time working wives.

Part-time work is generally more prevalent among the wives than among the husbands. About half of the control husbands who were not working full time at the outset were doing so at the fourth quarter. In the case of the experimental husbands, the initially part-time workers are shown to be more likely than the controls to move into full-time employment. The experimental heads who were not employed at all at the beginning are, by contrast, less likely than equivalent controls to move to full-time employment. In the case of wives the

TABLE 15

A. INITIAL AND 4TH QUARTER DISTRIBUTION OF HUSBAND-WIFE FAMILIES: BY HOURS WORKED LAST WEEK BY HUSBAND

	Status at Start				Status at Fourth			
	Control		Experimental		Control		Experimental	
	No.	%	No.	%	No.	%	No.	%
No one employed & not available	58	15.5	94	16.5	70	18.8	129	22.6
30 or less	13	3.5	41	7.2	16	4.3	43	7.5
31-39	40	10.8	67	11.7	31	8.3	44	7.7
40 or more	261	70.2	369	64.6	255	68.6	355	62.2
TOTAL	372	100.0	571	100.0	372	100.0	571	100.0

B. BREAKDOWN OF MOVEMENTS IN HUSBAND'S HOURS WORKED LAST WEEK

	Control				Experimental			
	No one empl. & NA	30 or less	31-39	40 or more	No one empl. & NA	30 or less	31-30	40 or more
Status at start	N=58	N=13	N=40	N=261	N=94	N=41	N=67	N=369
Breakdown by 4th quarter status:								
No one employed & NA	44.8%	23.1%	20.0%	12.6%	48.9%	17.1%	19.4%	17.1%
30 or less	5.2	0.0	12.5	3.1	6.4	17.1	7.5	6.8
31-39	1.7	30.8	22.5	6.5	7.5	14.6	16.4	5.4
40 or more	48.3	46.1	45.0	77.8	37.2	51.2	56.7	70.7
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

TABLE 16

A. INITIAL AND 4TH QUARTER DISTRIBUTION OF HUSBAND-WIFE FAMILIES: BY HOURS WORKED LAST WEEK BY WIFE

	Status at Start				Status at Fourth			
	Control		Experimental		Control		Experimental	
	No.	%	No.	%	No.	%	No.	%
No one employed & not available	335	90.0	498	86.2	317	85.2	499	87.4
20 or less	11	3.0	19	3.3	7	1.9	16	2.8
21-39	15	4.0	26	4.6	27	7.3	29	5.1
40 or more	11	3.0	28	4.9	21	5.6	27	4.7
TOTAL	372	100.0	571	100.0	372	100.0	571	100.0

B. BREAKDOWN OF MOVEMENTS IN WIFE'S HOURS WORKED LAST WEEK

	Control				Experimental			
	No one empl. & NA	20 or less	21-39	40 or more	No one empl. & NA	20 or less	21-39	40 or more
Status at start	N=335	N=11	N=15	N=11	N=498	N=19	N=26	N=28
Breakdown at 4th quarter status;								
No one employed & not available	89.8%	36.4%	40.0%	54.5%	92.6%	63.2%	46.2%	50.0%
20 or less	1.2	18.2	6.7	0.0	2.0	21.0	3.8	3.6
21-39	5.7	18.2	33.3	9.1	2.0	15.8	38.5	21.4
40 or more	3.3	27.3	20.0	36.4	3.4	0.0	11.5	25.4
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

numbers are too small at this point to warrant any attempt at interpreting the transitions--beyond the observation that over the first year control wives entered employment in substantial numbers, while there was a net reduction in employment among experimental wives.

Summing up all this and the indications mentioned in the previous section, I think it is clear that differential response of experimental families does exist--evidenced by fewer people employed at any one period in time, and correspondingly reduced total hours of labor supply. This differential is largely offset by increases in hourly earnings which are in turn partly produced by compositional changes of the kind described above. The rest of the differential is due to achieved increases in earning rates on the part of individual earners. The lower number of hours and fewer employees do not seem to be concentrated in a few lie-abouts, nor are they primarily accounted for by changes attributable to the head.

This is an unanticipated outcome, there having been a tacit assumption that any disincentive effect would show up in all the indicators of labor supply and earnings. A substantial amount of further work needs to be done both to verify this result more completely and to come to a satisfactory explanation of the process that has produced it.

As usual, it is not difficult to find a rationalization for the results. Currently, the most promising one is that the experimental treatment provides the security to enable earners to get better jobs. This process probably involves a longer search for some which would account for at least part of the reduced employment and hours. We have at this point no clue as to how much of the hours and employment differential is attributable to this, and there remains, of course, a good theoretical basis for expecting an income effect working toward increased leisure. Certainly other explanations are possible, and much work over the coming months will be devoted to developing and testing alternative hypotheses. Indeed, one of the primary purposes behind this presentation of preliminary results is to stimulate discussion of such alternatives.

DIFFERENTIALS BY EXPERIMENTAL PLAN AND BY ETHNIC GROUP

Differential responses within the experimental group according to treatment parameters, and differential responses by ethnic group are a central concern of this study, as well as being two areas of acute general interest. However, at this time we can give no satisfactory account of what has been happening, even in a preliminary way. The reason for this is that the sequential way in which the families were enrolled in the four sites produced a substantial statistical confounding of site, ethnicity, and experimental treatment.

The methodological problem of sample allocation was not finally solved until after the families in Trenton, Paterson, and Passaic had been selected and enrolled. The assignment used in Trenton was a relatively uniform allocation over seven of the experimental plans and the control group, and our Trenton sample thus contains no one on the most generous 125 percent guarantee plan, and has a significant underrepresentation of controls. The families in Paterson-Passaic were allocated according to a relatively heuristic scheme which also turned out to have insufficient control families as compared to the finally approved "optimal" allocation, as well as the wrong-sized cell groups for the plans themselves. The allocations in Jersey City and Scranton thus obviously had to be chosen to optimize the overall allocation. Hence, even in these sites there were departures from the "optimal" allocation--this time deliberate ones to offset the previous lack of optimality in the plan allocations. By the time enrollment in Scranton was complete, therefore, the allocation to experimental plans over the experiment as a whole had become satisfactory. But the problem of too few controls in the first sites had not yet been solved, and 141 new controls were enrolled in those sites (Trenton, Paterson, Passaic) the following year.

The problem is further complicated by the fact that the ethnic distributions in the four cities are by no means uniform. The most obvious disparity is that Scranton is almost all non-Puerto-Rican white and accounts for 75 percent of our entire white group; but our sample also contains a more than proportional number of blacks in Trenton and Jersey City and of Puerto Ricans in Paterson-Passaic. Consequently, even though there was an ethnically random assignment of experimental treatment within each site, there is a decidedly non-random overall allocation of experimental treatments to the several ethnic groups. Since we had too few controls in Trenton and Passaic at enrollment, this disparity also means that at the pre-enrollment interview we had more blacks in the experimental group than in the controls, and more whites in the controls than the experimentals. (At least part of this disparity, of course, will be eventually eliminated by the additional controls that were enrolled late.)

In spite of the confounding described above, which we have not yet disentangled analytically, we did perform a few crude tests and found no statistically significant differences either by plan or by ethnic group. We shall indicate here the tests that have been made and their outcome, but it would be imprudent to extrapolate that other tests—and more appropriate ones--will prove equally negative.

The first analytical effort used a variety of simple and obvious specifications of models to capture intra-experimental group differences. Groups of treatments were formed in various ways and represented by two to five binary variables. These were then used in regressions including (additive) binary variables for controlling city and ethnic group, the pre-enrollment value of the dependent variable (employment, hours, or earnings), family size, and age of head. Tests (Standard F-Ratio) were made on the ability of these groups of binaries to improve on the explanatory power of a single overall experimental effect, and none of them exceeded the 5 percent critical value. The values of the tax rate and the (index) level of the guarantee were also introduced as continuous parameters, and though their coefficients generally had the appropriate (negative) sign they were not significant (jointly or individually) in regressions of the kind cited above. Again, the ability to improve on a single overall experimental response was the criterion.[5]

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[5]It must be noted here that the use of binary variables to provide a relatively "form-free" description of the response is quite prodigal in its use of degrees of freedom. The more economical continuous specifications on the other hand are more restrictive, and of these only the very special linear form has yet been used. It should also be stressed again that there are many ways in which these first descriptive regressions must be respecified before they can be seriously regarded as appropriate models for explaining the response variables.

Equally crude efforts were made to search for any differential response among ethnic groups. It is, of course, generally conceded that the major ethnic minorities represented in our sample--blacks and Puerto Ricans--face different sets of alternatives in the labor market; and it is quite possible that cultural factors are responsible for some additional differences in their response to any given set of alternatives (such differences may also exist within the heterogeneous non-black and non-Puerto Rican white group).

In order to measure differences among the distinguishable ethnic groups in their response to the experimental treatment separate experimental binaries for each ethnic group (white, black, Spanish, and "other") were introduced in regressions like those described above. Again, these were found to add an insignificant amount to the explanatory ability achieved by a single overall experimental response. Differences were observed--some of which approached significance on an individual basis--but they appear too uncertain to warrant any interpretation at this time.

FINAL REMARKS

In closing this review of the first impressions from an extremely interesting body of new data, we must stress again how mush more analysis is yet to be done. First, there are additional data yet to be collected, coded, and finally put into usable form. Only the first year of a three-year panel is currently available for the full sample (and even this does not yet include the "extra controls" added in the first enrolled sites). Second, a large number of variables--economic, attitudinal, demographic, etc.--have been collected and are as yet unexploited. Finally, a wide range of analytic models, empirical methods, and hypotheses have yet to be brought to bear on the main (labor supply) objective of the experiment as well as a variety of subsidiary concerns relating to the effect of income-conditioned transfers.

APPENDIX I: OTHER REFERENCES

For a general description of how the experiment was set up, of the characteristics by which the sample was chosen, the rules of operation and so on, see Harold W. Watts, "Graduated Work Incentives: An Experiment in Negative Taxation," The American Economic Review, Volume LIX, No. 2, May 1969 (Institute for Research on Poverty Reprint # 39).

For a statistical exposition of the experimental sample design see John Conlisk and Harold Watts, "A Model for Optimizing Experimental Designs for Estimating Response Surfaces," Proceedings of the Social Statistics Section, American Statistical Association, 1969 (Institute for Research on Poverty Reprint # 54).

The first set of preliminary figures put out by the Institute on the experiment can be found in Harold W. Watts, Adjusted and Extended Preliminary Results from the Urban Graduated Work Incentive Experiment (Institute for Research on Poverty Discussion Paper # 69-70).

The Office of Economic Opportunity has so far issued two pamphlets on the New Jersey Experiment as follows. The first one appeared in February, 1970, entitled "Preliminary Results of the New Jersey Graduated Work Incentive Experiment"; and the second one was issued in May, 1971 entitled "Further Preliminary Results of the New Jersey Graduated Work Incentive Experiment."

APPENDIX II: ANALYSIS OF EARNINGS CHANGES

The principal tool used to examine the response to experimental treatments in the preliminary reports issued last year was a comparison of the distribution of changes in earnings for families in the control and experimental groups respectively. This tool was chosen as more suited to the state of the data at that time than more sensitive methods such as mean values and regressions. It is, however, a very cumbersome tool, particularly when one must provide control for other variables. Consequently, now that the data is in a more reliable, "cleaned" form it is time to discontinue this method of analyzing and developing results.

To provide an element of more direct comparability, however, this Appendix shows a selection of change distributions in Tables II-1 through II-5. Very briefly, and with one minor exception, there is no evidence of an experimental effect on earnings change. Chi-square tests were carried out for income changes over the first year for the full sample, and over the first 18 months for the half sample, where the experimentals were divided into two groups (those on low and high plans respectively).

Contrasts in total family earnings changes are displayed in Tables II-1 through II-3 for families that were interviewed continuously* through the 4th (or 6th for the half sample) quarter. Table II-2 is limited to

*"Continuously" means that they have missed no more than one quarterly and have satisfactorily completed the most recent one.

those families who were not on welfare; and Table II-3 is limited to husband-wife families. In all these cases there is no significant difference between the controls and either of the two sets of experimentals. In Table II-4 we do find evidence (in the full sample) of a significant reduction in earnings of the experimental wives. However, it is worth noting that the findings of the half sample through the 6th quarter are decidedly not significant. Finally, Table II-5 gives the earnings changes for male heads. No significant differences appear.

TABLE II - 1

FAMILY EARNINGS CHANGE

ALL CONTINUOUS FAMILIES

		Control		Low Plans		High Plans		Total Experimentals		Total Families	
		No.	%	No.	%	No.	%	No.	%	No.	%
Full Sample	+	139	32.9	115	31.7	87	30.0	202	30.9	341	31.7
(Preenrollment-	=	171	40.5	137	37.7	121	41.7	258	39.5	429	39.9
4th Quarterly)	-	97	23.0	96	26.5	67	23.1	163	25.0	260	24.2
	na	15	3.6	15	4.1	15	5.2	30	4.6	45	4.2
	Total	422	100.0	363	100.0	290	100.0	653	100.0	1075	100.0
Half Sample	+	35	35.0	68	36.0	34	32.1	102	34.6	137	34.7
(Preenrollment-	=	35	35.0	54	28.6	36	34.0	90	30.5	125	31.7
6th Quarterly)	-	23	23.0	56	29.6	30	28.3	86	29.2	109	27.6
	na	7	7.0	11	5.8	6	5.7	17	5.8	24	6.1
	Total	100	100.0	189	100.0	106	100.0	295	100.0	395	100.0

\+ increase of more than $25
= change $25 or less
\- decrease of more than $25
na undetermined because at least one earnings observation is missing

Full: χ^2(d.f. = 6) = 3.41; Pr = .76

Half: χ^2(d.f. = 6) = 2.66; Pr = .85

TABLE II - 2

FAMILY EARNINGS CHANGE

ALL CONTINUOUS NON-WELFARE FAMILIES

		Control		Low Plans		High Plans		Total Experimentals		Total Families	
		No.	%	No.	%	No.	%	No.	%	No.	%
Full Sample	+	123	38.0	95	36.0	79	33.3	174	34.7	297	36.0
(Preenrollment-	=	131	40.4	108	40.9	100	42.2	208	41.5	339	41.1
4th Quarterly)	-	59	18.2	50	18.9	49	20.7	99	19.8	158	19.2
	na	11	3.4	11	4.2	9	3.8	20	4.0	31	3.8
	Total	324	100.0	264	100.0	237	100.0	501	100.0	825	100.0
Half Sample	+	31	41.3	53	43.4	26	33.3	79	39.5	110	40.0
(Preenrollment-	=	27	36.0	32	26.2	25	32.1	57	28.5	84	30.6
6th Quarterly)	-	12	16.0	30	24.6	22	28.2	52	26.0	64	23.2
	na	5	6.7	7	5.7	5	6.4	12	6.0	17	6.2
	Total	75	100.0	122	100.0	78	100.0	200	100.0	275	100.0

+ increase of more than $25
= change $25 or less
- decrease of more than $25
na undetermined because at least one earnings observation is missing

Full: χ^2(d.f. = 6) = 1.60; Pr = .95

Half: χ^2(d.f. = 6) = 5.48; Pr = .48

TABLE II - 3

FAMILY EARNINGS CHANGE

ALL CONTINUOUS HUSBAND-WIFE FAMILIES

		Control		Low Plans		High Plans		Total Experimentals		Total Families	
		No.	%	No.	%	No.	%	No.	%	No.	%
Full Sample	+	130	35.0	110	36.0	84	31.7	194	34.0	324	34.4
(Preenrollment-	=	157	42.2	125	40.9	111	41.9	236	41.3	393	41.7
4th Quarterly)	-	74	19.9	58	19.0	57	21.5	115	20.1	189	20.0
	na	11	3.0	13	4.3	13	4.9	26	4.6	37	3.9
	Total	372	100.0	306	100.0	265	100.0	571	100.0	943	100.0
Half Sample	+	31	39.7	65	43.6	31	34.8	96	40.3	127	40.2
(Preenrollment-	=	29	37.2	44	29.5	31	34.8	75	31.5	104	32.9
6th Quarterly)	-	13	16.7	33	22.2	22	24.7	55	23.1	68	21.5
	na	5	6.4	7	4.7	5	5.6	12	5.0	17	5.4
	Total	78	100.0	149	100.0	89	100.0	238	100.0	316	100.0

\+ increase of more than $25
= change $25 or less
\- decrease of more than $25
na undetermined because at least one earnings observation is missing

Full: χ^2(d.f. = 6) = 2.97; Pr = .81

Half: χ^2(d.f. = 6) = 3.72; Pr = .71

TABLE II - 4

WIFE EARNINGS CHANGE

CONTINUOUS HUSBAND-WIFE FAMILIES

		Control		Low Plans		High Plans		Total Experimentals		Total Families	
		No.	%	No.	%	No.	%	No.	%	No.	%
Full Sample	+	40	10.8	30	9.8	13	4.9	43	7.5	83	8.8
(Preenrollment-	=	317	85.2	255	83.3	225	84.9	480	84.1	797	84.5
4th Quarterly)	-	14	3.8	15	4.9	23	8.7	38	6.7	52	5.5
	na	1	0.3	6	2.0	4	1.5	10	1.8	11	1.2
	Total	372	100.0	306	100.0	265	100.0	571	100.0	943	100.0
Half Sample	+	8	10.3	13	8.7	7	7.9	20	8.4	28	8.9
(Preenrollment	=	63	80.8	126	84.6	75	84.3	201	84.5	264	83.5
6th Quarterly)	-	6	7.7	8	5.4	6	6.7	14	5.9	20	6.3
	na	1	1.3	2	1.3	1	1.1	3	1.3	4	1.3
	Total	78	100.0	149	100.0	89	100.0	238	100.0	316	100.0

+ increase of $15 or more
= change of less than $15
\- decrease of $15 or more
na undetermined because at least one earnings observation is missing

Full: χ^2(d.f. = 6) = 18.19; Pr = .006 (significant)

Half: χ^2(d.f. = 6) = .86; Pr = .99

TABLE II - 5

HEAD'S EARNINGS CHANGE

ALL CONTINUOUS MALE-HEADED FAMILIES

		Control		Low Plans		High Plans		Total Experimentals		Total Families	
		No.	%	No.	%	No.	%	No.	%	No.	%
Full Sample	+	101	26.5	105	33.4	82	30.5	187	32.1	288	29.9
(Preenrollment-	=	203	53.3	149	47.5	129	48.0	278	47.7	481	49.9
4th Quarterly)	-	73	19.2	56	17.8	50	18.6	106	18.2	179	18.6
	na	4	1.1	4	1.3	8	3.0	12	2.1	16	1.7
	Total	381	100.0	314	100.0	269	100.0	583	100.0	964	100.0
Half Sample	+	22	27.5	61	39.9	36	40.0	97	39.9	119	36.9
(Preenrollment-	=	38	47.5	59	38.6	32	35.6	91	37.5	129	39.9
6th Quarterly)	-	19	23.8	31	20.3	19	21.1	50	20.6	69	21.4
	na	1	1.3	2	1.3	3	3.3	5	2.1	6	1.9
	Total	80	100.0	153	100.0	90	100.0	243	100.0	323	100.0

+ increase of more than $25
= change $25 or less
- decrease of more than $25
na undetermined because at least one earnings observation is missing

Full: χ^2(d.f. = 6) = 8.36; Pr = .21

Half: χ^2(d.f. = 6) = 5.94; Pr = .43

APPENDIX III: TECHNICAL NOTES

This appendix is intended to provide more complete documentation of the regressions underlying the adjusted means shown on Tables 8, 9 and 10. First the process by which the adjusted means are obtained from the regression estimates will be explained, and then the precise specification of the several regressions.

All the regressions contain (1) a set of additive "conditioning" variables the effect of which is to be removed from the differential between control and experimental groups, and (2) a simple binary or dummy variable which is equal to one for experimental families and zero for others. The coefficient of this binary variable measures the experimental differential taken <u>net</u> of the additive effects of the other variables in the regression. And this differential is precisely equal to the difference between the similarly net means for the control and experimental groups respectively.

The overall average of the dependent variable for the entire sample is simply a weighted average of the adjusted control and experimental means using the proportions of experimental and control families as weights. Having both the difference and the weighted average one can solve easily for the two adjusted means. Thus:

$$\bar{Y}_c = \bar{\bar{Y}} - P\Delta_x$$

$$\bar{Y}_x = \bar{Y}_c + \Delta_x,$$

where Δ_x is the regression estimate of the experimental differential, and P is the proportion of experimental families.

The estimates in Table 8 were derived, as described above, from regressions of the following form:

$$\left.\begin{matrix} Y1F(4) \\ Y1H(4) \\ Y1W(4) \\ Y1O(4) \end{matrix}\right\} = a + a_1\, Y_iF(0) + \left\{\begin{matrix} a_2TR \\ a_4PP \\ a_4SC \\ 0\ JC \end{matrix}\right\} + \left\{\begin{matrix} a_5BL \\ a_6SP \\ a_7OT \\ 0\ WH \end{matrix}\right\}$$

$$+ a_8NA(4) + a_9NC(4) + a_{10}YNG + a_{11}X.$$

The symbols in this equation are defined below:

Y1F	=	No. of adults employed in family
Y1H	=	1 if Husband employed otherwise zero
Y1W	=	1 if Wife employed otherwise zero
Y1O	=	Number of other adults employed in family
Y2F	=	Total hours worked in family
Y2H	=	Hours worked by husband
Y2W	=	Hours worked by wife
Y2O	=	Hours worked by other adults in family
Y3F	=	Total earnings for all members of the family
Y3H	=	Earnings of husband
Y3W	=	Earnings of wife
Y3O	=	Earnings of other adults in the family

The parenthetical argument denotes the questionnaire from which the factor was taken

0 = pre-enrollment

1 = first quarterly questionnaire

2 = second quarterly questionnaire

3 = third quarterly questionnaire

4 = fourth quarterly questionnaire

The following are the other independent or regressor variables used in the equation:

TR	=	Trenton = 1, zero otherwise
PP	=	Paterson/Passaic = 1, zero otherwise
JC	=	Jersey City = 1, zero otherwise
SC	=	Scranton = 1, zero otherwise
BL	=	Black = 1, zero otherwise
SP	=	Spanish-speaking whites = 1, zero otherwise
WH	=	Other whites = 1, zero otherwise
OT	=	Other and not determined = 1, zero otherwise
NA	=	Number of adults in the family (16 years or over)
NC	=	Number of children in the family (under 16)
YNG	=	Binary variable = 1 if head is under 35, zero otherwise
X	=	Binary variable = 1 for experimental families 0 for control

(It will be noted that the a_{11} in the regression equation is the source of the Δ_x in the adjusted mean formulas.)

These regressions were carried out for the subset of husband-wife families only. There were 943 such families altogether--which was also the number used in the employment regressions. Thirty families did not have usable responses for 4th quarter hours worked. Hence the hours regressions were based on 913 families. The comparable loss in numbers used for the earnings regression was 37, leaving 906 usable observations.

The regressions behind Table 9 are of the same form but do not include NA, NC, and YNG. The first segment of the table (all continuous families) is drawn from the 1075 families who were continuous participants from pre-enrollment through the 4th. Once again some families had to be dropped out because of incomplete information--31 families for the hours regressions and 37 families for the earnings regression. The regression then is:

$$YiF(34) = a_0 + a_1 Y_i F(0) + \left\{\begin{matrix} a_2 TR \\ a_3 PP \\ a_4 SC \\ 0\ JC \end{matrix}\right\} + \left\{\begin{matrix} a_5 BL \\ a_6 SP \\ a_7 OT \\ 0\ WH \end{matrix}\right\} + a_{11} X \quad (i = 1,2,3).$$

The variables used here are already defined above, except that the parenthetical argument denotes an "opportunistic average of the variable" for the third and fourth quarters--YiF(34). This is an average that uses all of the information that is present and assumes that missing information is equal to the average of what is there. Zeros are not treated as missing data.

The next two segments show the results when the identical regressions were estimated for two partitions of the continuous families--(1) the 825 "non-welfare" families, and (2) the remaining 250 families who received some welfare payments either during the last quarter and/or during more than one of the other quarters. The non-welfare subsample loses 23 families because of incomplete hours and 31 because of incomplete earnings. The "welfare" group loses 13 and 14 respectively. The parenthetical argument here refers to the fourth quarter only--YiF(4).

The last segment of Table 9 shows results for the half sample (in Trenton-Paterson-Passaic). This of course means that Trenton is the only city dummy, with PP as the zero. Here the parenthetical argument denotes an opportunistic average of the variable for the fourth, fifth and sixth quarterlies--YiF(456). The 395 families in the half sample were again all available for the employment regressions but 11 were dropped for the hours regressions and 17 for the earnings.

Finally, the regressions in Table 10 reintroduce the variables for number of adults, number of children, and age of head (NA, NC, YNG) that were used in the first equation described for Table 8. The dependent variable for the first segment of Table 10 is YiF(1), and YiF(0) is used as a control variable. In the second segment, YiF(4) is the dependent variable and YiF(1) is a control variable. These regressions have been calculated from the subsample of continuous families that had usable responses for all of the three dependent variables at each of the observation points 0, 1 and 4. There were 986 families in this "complete information" sample.

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The Gary Income Maintenance Experiment

DESCRIPTION AND OBJECTIVES OF THE PROJECT

This experiment tests the behavioral effects of a negative income tax plan, combined with day care and social services on black, urban families with particular emphasis on female-headed families who comprise about 60% of the sample, a particular group not covered by either of the two OEO experiments.

The main focus of the Gary experiment is on the family work decision and how it is affected by an income maintenance transfer system. Thus the experiment will attempt to measure key economic responses such as labor supply, consumption patterns and investments in human capital. However, a major effort will also be devoted to measuring the sociological impact by looking at changes in such variables as family functioning, individual motivation, and aspirations. Because the experimental design splits the sample between a financial treatment group, a social services only group, and a combined social services-financial treatment group (as well as a control or null treatment group), the experiment should be able to determine whether there is an interaction between the receipt of services and income maintenance transfers and whether the social benefits deriving from the interaction of the two programs in combination exceeds the sum of the benefits when each program is operated in isolation. The observed interaction between the receipt of services and of income transfers should prove helpful in determining proper levels of social investment in both programs.

The second major focus of the experiment will be to measure the demand for, and to a lesser extent, the impact of, separately administered social services (such as day care, homemaker services, and counseling) when their provision and acceptance is no longer conditioned upon the receipt of assistance payments. It has long been argued that even if a secure basic income floor could be established, there would remain a need for specialized problem-solving services. The magnitude of need has not yet been established, nor has the cost-effectiveness of various service types been determined.

SUMMARY OF FACTS

1. Status

(*a*) Initiation of project: design phase initiated July 1969.

(*b*) Start-up date of operations: Enrollment of families began in March 1971 and was completed in August 1971.

(*c*) Duration: three years of operation.

2. Findings to date

No behavioral findings are available since still in first year of operations. First major behavioral findings will be available in late 1973 after the completion of two years of operation, with final report of findings in late 1974. For other findings see Section I.

3. Project Characteristics

(*a*) Location: low income areas of Gary, Indiana, concentrating on model cities area.

(*b*) Number of families covered: a total of 1,782 experimental and control families, 60% are female-headed and 40% are male-headed.

(*c*) Sample size by experimental treatment:

Experimental	1,287
Financial only	466
Financial and Social Services	466
Social Services only	355
Control	495

(*e*) Payment plans: two support levels of $3300 and $4300 will be tested, with two different tax rates, 40% and 60%.

(*f*) Other elements of the project: an expanded program of day care and social services is offered to part of the experimental sample.

4. *Administrative Arrangements*

This project is funded by an HEW contract with the State of Indiana Department of Public Welfare. The design and operation of the project is carried by the University of Indiana via a subcontract with the State Welfare Department.

5. *Financial Data*

(*a*) Estimated annual cost of operations: $3,500,000 [1]

(*b*) Obligations to date by fiscal year and funding authority:

	Sec. 1115	Sec. 1110	Title IV	Total
1969	$492,625			$492,625
1970		$940,000	[1] $1,054,375	1,994,375
1971		2,761,296		2,761,269
1972 estimate		3,500,000		3,500,000

[1] Matching of $492,625 of extended 1969 sec. 1115.

[1] The annual year costs are funded from a combination of funds from more than one fiscal year.

The Seattle-Denver Experiment

DESCRIPTION AND OBJECTIVES OF THE PROJECT

The Seattle-Denver experiment will focus on the degree to which public programs designed to facilitate employment of poverty and near poverty individuals will influence the work effort response of participants in a graduated work incentive income maintenance program. This, of course, is an issue of great concern in the further development of the Family Assistance Plan. Thus, in this particular experiment, the income transfer program itself will be supplemented by one or more manpower programs including (a) job training programs to enhance the employability of young and unskilled workers; (b) counseling and vocational guidance services; and (c) day care services for working mothers.

EXPERIMENTAL POPULATION AND DESIGN

The Seattle-Denver sample includes a broad cross section of whites, non-whites, male and female headed families. The 2044 experimental and control families in the Seattle sample will be equally divided between black and white facilies; Denver will approximate this allocation and include as well an additional component of 800 Mexican-American families. The opportunity to include the Mexican-American population in the experiment is a decided advantage, since any answers with regard to the responses of poor people to an income maintenance experiment which is limited to the black or non-spanish speaking white population would be missing a sizable minority group that will be important in an eventual income maintenance program.

Since the purpose of the experiment is to explore the effects of income maintenance and manpower programs, both separately and in combination, the 5100 families will be divided among treatment groups and locations as follows:

Treatment	Number of families	
	Seattle	Denver
Financial only	369	600
Financial and manpower	742	1,100
Manpower only	412	600
Null (control)	521	700
Total	2,044	3,000

Note: The exact distribution for the Denver sample has not yet been determined.

The division of the sample between the two experimental sites is in itself a control factor. The recent deterioration of the labor market in the city of Seattle has made it necessary to extend the experiment to a city with a healthy economy in order to control for the labor demand variable.

GOALS

The primary hypothesis to be tested in Seattle is that manpower training in combination with a rational system of cash transfers will yield a policy payoff exceeding the sum of the outcomes of the two separate components. In addition, Seattle-Denver will provide vital information concerning the proper mix of manpower and cash, thereby suggesting the most efficient allocation of scarce government funds in the future.

The experiment will measure the effects of different combinations of income maintenance support levels and manpower programs by looking at the:

(a) Work effort of the household;
(b) Productivity of the household as measured by changes in earnings;
(c) Investment of the household in training or other education;
(d) Changes in attitudes toward the future; and
(e) Changes in household stability.

Two unique features of the Seattle-Denver experiment will provide needed answers concerning the complexity of labor responses. One is the use of a nonlinear negative tax schedule. Up to now, our ignorance of response to tax rates has forced us to choose a system at random. The Seattle-Denver project, it is hoped, will provide information to make rational policy decisions about this aspect of income maintenance programs.

The second is the location of the experiment in two sites, Seattle and Denver, which are similar in demographic characteristics, but vary in the condition of their respective labor markets. Comparison of program effects between a stable labor market situation and a deteriorating one allows us to filter out the differential effects of changes in the labor supply and demand, and to measure the impact of income maintenance programs on normal adjustments to the business cycle.

SUMMARY OF FACTS

1. Status

(*a*) Initiation of project: design phase began July 1969.

(*b*) Start-up date of operations: Initial enrollment of families in Seattle began in November 1970, with final enrollment completed in October 1971. Initial enrollment began in Denver in November 1971 with final enrollment expected to be completed in April 1972.

(*c*) Duration: Three years of operation, except that a small subsample of approximately 20% will continue on the program for two additional years to verify that the experimental results from the total sample as well as the other three experiments are not unduly biased by the effects of a transitory change in income.

2. Findings to Date

No behavioral finding are available since still in first year of operations. For other findings see Section I.

Since the first year of the operation probably contains sizable transitory elements of response, heavy reliance cannot be placed on any first year results. The optimal year for analyzing experimental results is the second year of the experiment. Because of the considerable economic dislocation in Seattle, the results from the Seattle site must be interepreted in the light of findings from the Denver site which represents the national economic norm. Therefore the timing of reporting of results depend upon the schedule in Denver. Hence the first major findings from the Seattle-Denver experiments will be available early in 1974, two years after final enrollment in Denver. (If the economic situation in Seattle sufficiently improves during the course of the experiment, then results from the Seattle portion of the experiment may be available earlier.) A final report on the three year program will be available in 1975.

3. Project characteristics

(*a*) Location: low income areas of Seattle, Washington and Denver, Colorado.

(*b*) Number of families covered:

A total of about 5,100 families will be enrolled in the Seattle-Denver Project of which about 2,100 will be in Seattle and 3,000 in Denver.

(*c*) Characteristics of the sample: Urban poor of which 60% are two parent families and 40% are single (primarily female-headed) families. The Seattle sample is about equally divided between black and white families and is matched by a similar sample in Denver. In addition, the Denver sample contains about 800 Mexican-American families.

(*d*) Payment plans: There are three support levels, $3800, 4800 and 5600, and four tax schedules. Two of these employ constant tax rates at 50% and 70%. The other two are declining rate schemes beginning at 70% and 80% with the rate of decline set at 5% for each $1000 of income.

(*e*) Other elements of project (if relevant): An expanded program of manpower services is offered to project participants including job training to improve employment abilities of young and unskilled workers, counselling and vocational services, and day care services for children of working mothers.

4. Administration

This project is funded by HEW contracts with the State of Washington Department of Public Assistance and the Colorado State Department of Social Services. Mathematica Corporation is the SRI subcontractor for administrative operations at both sites while Seattle Community College and Denver Community College are SRI subcontractors for the manpower program in Seattle and Denver respectively.

5. Financial data

(*a*) Estimated annual cost of two site operations, $8,000,000[1]

(*b*) Obligations to Date by fiscal year and funding authority:

	Sec. 1115	Sec. 1110	Title IV	Total
1969	[1] $501,000			$501,000
1970		[1] $5,100,000	[1,2] $1,177,386	6,277,386
1971	[2] 5,990,761			5,990,761
1972		[1,2] 7,200,000		7,100,000

[1] For Seattle site.
[2] Matching of extended sec. 1115 grant.
[3] For Denver site.

[1] The first full year cost is funded from a combination of funds from more than one fiscal year.

Gary Experiment (HEW)

Location: Gary, Indiana.

Contractor: Indiana State Department of Public Welfare; Indiana University (sub).

Sample: 1,800 families: 1,300 experimental, 500 control; all black; 1,100 female headed, 700 male headed.

Dates: Design phase begun July 1969; Enrollment of families completed August 1971; Three year duration.

Major Experimental Variation: Tax rates: 0.4 and 0.6; Guarantee levels: $3,300 and $4,300; Day care subsidies; Social service access workers.

Seattle/Denver Experiment (HEW)

Location: Seattle, Wash.; Denver, Colo.

Contractor: State of Washington Dept. of Public Assistance; Colorado State Department of Social Services; Stanford Research Institute; Mathematica (sub-payments); Seattle Community College (sub-manpower); Denver Community College (sub-manpower).

Sample: 5,100 families: 2,100 in Seattle, 3,000 in Denver, 3,900 experimental, 1,200 control; 60% two-parent families, 40% one-parent (primarily female head); half black, half white in Seattle; equally divided among black, white and Mexican-American in Denver.

Dates: Design phase began July 1969; Enrollment complete in Seattle, October 1971; expected to be complete in Denver, April 1972; Duration: three years for 80%, five years for 20% of the sample.

Major Experimental Variables: Tax rates: constant at 0.5 and 0.7, declining from 0.7 and 0.8; Guarantee levels: $3,800, $4,800, $5,600; Manpower services at subsidized rates.

New Jersey Experiment (OEO)

Location: Trenton, Paterson, Passaic and Jersey City, N.J.; Scranton, Pa.

Contractor: Poverty Institute, U. of Wisconsin; Mathematica (sub).

Sample: 1,400 families—650 treatment, 750 control; all nonstudent, male-headed families, age 18–58; normal family income between 100% and 150% of poverty; one-third each black, Puerto Rican, white.

Dates: Payments began August 1968–September 1969; 3-year duration.

Major Experimental Variation: Tax rates: 0.3, 0.5, 0.7; guarantee levels as a fraction of Poverty level: 0.5, 0.75, 1.00, 1.25.

Rural Experiment (OEO)

Location: North Carolina; Iowa.

Contractor: Poverty Institute, U. of Wisconsin.

Sample: 825 families: half experimental, half control; 600 male head, age 18–58; 110 female head, age 18–58; 115 male or female head, over 58; normal income less than 1.5 times poverty; not stratified by race.

Dates: Payments began November–December 1969; 3-year duration.

Major Experimental Variation: Tax rates: 0.3, 0.5, 0.7; guarantee as a fraction of poverty level: 0.5, 0.75, 1.0; payment adjustment period: 3-month moving average; 1 month.

Purpose and Scope of Projects in Vermont

1. DOL E&D MANPOWER PROJECTS STATEWIDE

The purpose of this project is to develop and test upgrading mechanisms and special works projects which are directed toward WIN enrollees as well as non-WIN.

The E&D project, cinfined last year to the Burlington and Morrisville Districts, has this year been integrated into the WIN program, (i.e., delivery of WIN and E&D manpower services through identical employability development teams) which operates statewide. In Fiscal Year 1972, the Vermont Department of Employment Security is operating its WIN program at a level of 600 slots, the E&D Manpower services will provide, on a phase-in basis, for 300 special works project slots and for 100 upgrading training positions for the working poor.

Total cost of the E&D project for Fiscal Year 1972 was budgeted at a level of $1,165,868.

Federal project manager is Joseph Seiler, DOL E&D, Manpower Administration.

2. VOCATIONAL REHABILITATION: BURLINGTON-MORRISVILLE

This project was designed to complement the DOL E&D project in the Burlington and Morrisville Districts and to provide experience with numbers and costs of incapacity certifications and need for additional VR services for FAP (now OFP) eligibles.

Under current fiscal year funding, the project continues to operate in the two districts; it was not expanded for statewide operation. However, during this fiscal year, through inter-agency agreements, the project has begun to make disability determinations for Adult Blind, Disabled, and ANFC-incapacity applications in the two districts for the Department of Social Welfare.

Total cost of the VR project for Fiscal Year 1972 was budgeted at a level of $95,375.

Federal project manager is Jerry Turem, SRS/RSA.

3. CHILD CARE: STATEWIDE

This is the only statewide project funded by HEW.

During the past year, Vermont staff has developed, under HEW contract, a model child care system, intended to be cognizant of and responsive to the needs of FAP (now OFP) families, as well as being capable of being quickly implemented under varying political, economic and social conditions. The product of this effort purports to be a system of practical alternatives which can be exercised in a community, and which, when implemented, will result in the provision of adequate and sufficient child care services in that community.

During this fiscal year, the State is working to implement the plans. Primary objectives in Fiscal Year 1972 are:

(*a*) To furnish licensed care or a suitable employment-related child care substitute to children of 100% of the WIN and E&D training enrollees who require procured day care services in support of training and employment (an estimated 1050 children by June 30, 1972);

(*b*) To upgrade care for children of new ANFC cases who require day care services in support of employment (an estimated 415 children in care by June 30, 1972), and;

(*c*) To extend licensed care to other low income families eligible for services on a sliding scale fee basis in support of employment.

Development of before and after school programs is another priority objective.

Total cost of the child care project for Fiscal Year 1972 was budgeted at a level of $1,236,844.

Federal project manager is Sam Granato, OCD.

4. SOCIAL SERVICES: BURLINGTON-MORRISVILLE

The plan, developed last fiscal year, to separate services from income maintenance and develop a staff of service planners and specialists has been fully implemented in the Burlington and Morrisville Districts.

Priority items for the operational phase, this year, include development of services resources and provision of supportive services for persons in WIN and related manpower programs.

Total cost of the social services project for Fiscal Year 1972 was budgeted at a level of $1,075,444.

Federal project manager is Barbara Pomeroy, SRS/CSA.

5. STATE PLANNING AND COORDINATION STAFF

This staff provides project staff work necessary to the Secretary of the Agency for Human Services, which organizationally houses all project-involved agencies except the Department of Employment Security. The Secretary, with aid of this staff, serves as the decision-maker on DHEW project-related matters. He and the Commissioner of Employment Security jointly decide inter-agency matters.

The staff is responsible for planning, monitoring and coordinating all of the services activities relating to the H.R. 1 design.

Total cost of the Planning and Coordination Project for Fiscal Year 1972 was budgeted at a level of $191,000.

The Federal project manager is Carolyn Betts, WRPS/FBA, Interagency Planning.

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