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INSTITUTE FOR RESEARCH ON POVERTY DISCUSSION PAPERS

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

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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

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*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 <u>and</u> provides incentives for recipients to augment their benefits <u>must</u> 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 "naar poor" are directly affected by such a program it would be very foolish to evaluate it on the basis of a <u>minority</u> 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

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*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.

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*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.

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*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

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*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 analyzed 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			Guarantee	
			30%	50%	70%	Low	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		Ethnic Group			Stratum		
		Trenton	Paterson-Passaic	Black	Non-Black	Spanish	I	II	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: 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	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 <u>more</u> 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.