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AFTER 15 MONTHS: PRELIMINARY RESULTS FROM THE URBAN NEGATIVE INCOME TAX EXPERIMENT

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ABSTRACT

This paper presents a preliminary report from the urban negative income tax experiment.[1] Its purposes are three: (1) it places negative taxation within the general context of welfare reform; (2) it describes the urban negative tax experiment currently in progress; and (3) it reports some of the initial findings of the experiment with respect to its major objective, the measurement of labor supply response, and with respect to the respondents' understandings of the program.

After 15 Months: Preliminary Results From the Urban Negative Income Tax Experiment

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THE CONTEXT OF WELFARE REFORM

As Tobin, et al. (1967) have noted, there is consensus both about the ills of the present welfare system and about the fact that reform is needed. Very few disagree that the system is grossly inadequate, ineffective, and inefficient. Two-thirds of the poor are untouched by present programs, as most are outside of the eligible recipient categories: the aged, the blind, the disabled, and the female-headed family with dependent children. Moreover, of those who do receive aid, only 17 percent are raised above the poverty line by that assistance. Standards and benefit levels vary enormously from state to state, reflecting less differences in the recipients' needs and the cost of living than differences in local political climates.[2] Finally, these inequities are compounded by the fact that the current system encourages dependency by reducing benefits by an amount approximately equal to any earned income.[3] The probable effect of this high tax rate on earnings is to keep potentially productive workers out of the labor force, since there is little financial incentive to work.

Consequently, it is generally agreed that proposals for welfare reform must speak to these issues. Specifically, the current disincentives to work should be lessened and, in contrast to Aid to Families with Dependent Children, should include no incentives for family disruption. Eligibility should be based soley on objective criteria, and the criteria should be uniform across the nation. These requirements would greatly simplify the administration of welfare, since caseworkers would no longer determine eligibility or benefit levels.

Most of the various income maintenance schemes that have been proposed to fully or partially replace the present public welfare system incorporate some or all of the above reforms. The three most widely discussed schemes are:

(1) Guaranteed employment, a program that would make the federal government the employer of last resort for those who could not find jobs;

(2) Children's allowances, which would provide to families a specified grant of money for each child;

(3) Negative income tax, the program of concern in this paper, which provides specified supplements to annual income based on family size, and includes a financial incentive to work feature which reduces payments by some fraction of a dollar for each dollar earned, to insure that those who work always have more income than those who do not.

Criteria for evaluating programs. In order to properly evaluate the relative advantages and disadvantages of each scheme, we need a set of criteria applicable to all. Scholars in the field of income maintenance have emphasized a variety of criteria [Marmor, 1969; Weisbrod, 1969; President's Commission, 1969]. In the absence of a generally accepted set, we offer the following tentative list of factors that most would consider a minimal guideline in evaluating any new income maintenance program:

(1) Adequacy of benefits--how near or above the poverty line will the poor be after payments, or what percent of lost earnings will be replaced by the program?

(2) Scope of coverage--what percent of the poor or the risk population will be covered by the program?

(3) Leakage--how efficient is the program in terms of the percent of total costs spent on administration and in payments to nonpoor as opposed to direct benefits to the poor?

(4) Cost--how much will the program cost the taxpaying public?

(5) Dignity and restraints on behavior--does the program dispense funds without disagreeable surveillance or screening procedures? Does the program restrict freedom of movement or choice in the labor or consumer market?

(6) Adverse side effects--does the program have inadvertent consequences such as disruption of family organization, discouragement of labor force participation, or encouragement of labor force withdrawal in order to qualify for funds? Does the program interfere with other programs or create undesirable patterns of migration?

(7) Clarity of application and minimization of discretionary power--does the program minimize, if not eliminate, the power of administrators to determine final treatment of recipients? Are there clear and precise rules that specify the allocation of benefits in the program?

(8) Equity--are there precise rules for horizontal equity, i.e., the equal treatment of all who are equally placed? Are there rules for vertical equity, i.e., clear-cut and reasonable criteria by which groups are differentiated in terms of needs?

(9) Automatic flexibility--is there built into the program anticipations of changing statuses of recipients and economic conditions which provide for automatic shifts in benefits?

(10) Economic stability--does funding or operation of the program have adverse effects on the economy or labor markets?

No attempt has been made to be exhaustive in this list. We have intentionally avoided listing specific program attributes having to do with definition of income and family unit, benefit structure, length of

accounting and payment period, how the program shall be paid for, and other features that are quite obviously critical in the final operation of a program. At present it is not possible to know with any certainty the outcomes for each program on each criterion listed, much less for those details not listed. We lack precisely the kind of evidence for program evaluation that is being collected in the urban experiment. However, it is possible to make some estimates of how each program might fare in terms of the above criteria so long as we keep in mind the possible influences of variations in program details.

Space does not allow a detailed application of these criteria to the three comprehensive income maintenance schemes. We present instead a brief summary of the most important weaknesses and strengths of each program.

1. Guaranteed employment. The major advantages of a guaranteed employment program are its utilization of manpower and the fact that income would be dignified by work rather than stigmatized as "given away." The major weaknesses of a guaranteed employment program have to do with adequacy, scope, and adverse side effects. Unless a guaranteed employment program were tied to a generous wage supplement scheme, it could not provide the occupationally unskilled poor with incomes above the poverty line. Nor would making the federal government an employer of last resort assist the one-third of poor families who simply do not have employable members. Finally if, as employer of last resort, the federal government paid unskilled workers wages or supplements sufficient enough to bring incomes above the poverty line, these jobs might very well attract many low paid semi- and unskilled workers from the private

sector, an undesirable side effect that would require the imposition of restrictive eligibility rules and tests of need for qualification.

2. Children's allowances. The major advantages of a children's allowance program are that it sets up a simple, easily administered, and dignified right to income based on size of family, a criterion that is not considered by employers in setting wages of workers. There are, however, a number of weaknesses, with respect to adequacy, leakage, and adverse side effects. A good deal depends upon the size of the allowance. If payment per check is as low as in Canada and most other nations (excluding France), it would be inadequate to lift most poor families out of poverty. The major weakness, however, is that children's allowances are very inefficient in eliminating poverty because most of the transfers go to the nonpoor. To make the program efficient most of the payments to the nonpoor must be recovered through positive taxation. One major side effect of the program is the possibility that some person may withdraw from the labor force if benefits rival wages [Green, 1967]. This is particularly important since the program does not encourage labor force participation. These disadvantages are sufficient to suggest that, despite its several merits, a children's allowance is not an efficient means of alleviating poverty.

3. Negative income tax. This scheme has many obvious advantages. It would be universal in coverage, provide a dignified way to transfer funds to the poor without screening or surveillance, avoid possible disruption of family organization, and, with the work incentive factor built in, encourage voluntary labor force participation. It also minimizes the discretionary power of administrators and provides clear and precise rules of horizontal and vertical equity. As in the federal

income tax system, shifts in the organization of the program or recipient status vis-a-vis the program could be easily and automatically accommodated.

The major problems with a negative income tax program have to do with adequacy, cost, and adverse side effects. Adequacy would depend entirely upon where the breakeven points and guarantees are set. Most programs now being discussed would not do away with poverty. They are seen as needed minimal supplements to earned income. To wipe out poverty via the negative income tax would be expensive, costing in the neighborhood of $25 billion. Insofar as benefits rival wages, a negative tax program, like a children's allowance, may contain work disincentives.

THE URBAN NEGATIVE INCOME TAX EXPERIMENT

1. Objectives

Whatever the presumed benefits of a negative tax scheme, there are a variety of questions that must be answered before its adoption. With some oversimplification, they can be reduced to one: What is the cost of a negative tax program? To answer this question, we must specify a particular program--a particular tax rate, guarantee level, and a set of eligibility criteria--and examine empirically the work effort under the program.

If tax rates, guarantee level, and eligibility criteria were all that were needed to calculate cost, empirical research would be unnecessary, aside from the determination of the number of eligibles. Nor would research be required if it were possible to determine the work response of participants from theory or analysis of existing data, but neither economic nor sociological theory is sufficiently developed to provide us with quantitative forecasts in these areas. Both economic

and sociological theory will give us qualitative predictions: We expect some people to choose less work as the cost of not working decreases, but we cannot say by how much. We need to know how the response will vary with the tax rate-guarantee level combination and, within combinations, by labor market status, age, race, ethnicity, education, residential location, family size and composition, occupational history, values, etc.

The usual types of economic and sociological data--governmental and private censuses and surveys--are not adequate for answering these questions, for it is extremely unlikely that we could find natural analogues of sufficient size and permanence to be comparable to the exogenously induced changes in a family's unearned income which would be provided by a negative tax program. What evidence we have on the unearned part of a family's income indicate that it is of little consequence for families of low annual income [Weisbrod and Hansen, 1967]. Consequently, we are led to an experimental design for research into the response to a negative income tax.[4]

2. Design

Since the major purpose of the experiment is to assess work effort response, and since most of the poor workers are in intact families in urban areas, the experiment is restricted to families with nonstudent male heads, 18-58 years of age, able to work, and with normal[5] family incomes no more than 150 percent of the poverty line for each family size. The sample has been drawn from poverty tracts in Trenton, Paterson-Passaic, and Jersey City, New Jersey, and Scranton, Pennsylvania. The first part of the sample was drawn in Trenton in August, 1968; the final segment was selected in Scranton in September, 1969. Our experience is that roughly 80 percent of the eligibles fall between 100-150 percent of the poverty line.

The basic design contains an experimental and a control group.[6] Once eligibility was determined from a special screening interview, families were randomly assigned to one of eight negative tax plans which together define the experimental group or to the control group. The experimental group contains 724 families and the control group consists of 489 families.

The eight tax plans are combinations of tax rates and guarantee levels which, in our judgment, encompass the area of greatest policy interest. Tax rates range from 30 to 70 percent, and guarantee levels vary from 50 to 125 percent of the poverty line (thus for a family of four, the range of guarantees at the beginning of the experiment ran from $1,650 to $4,125).[7] Table 1 shows the combinations selected for experimentation. Table 2 gives the guarantee levels by family size.

Tables 1 and 2 About Here

After families were assigned to groups, all, experimental and control, received a pre-enrollment interview. The purpose of this interview was to obtain baseline data in a variety of areas uncontaminated by knowledge of the experiment or the inception of transfers. Subsequently, the experimental families were visted by enrollers who explained the program to them and solicited their cooperation. If obtained (less than seven percent refused),[8] they receive payments for three years. Their only obligation is to report their income and family composition each month. They are interviewed quarterly. The control group is also interviewed quarterly. The size of this group (489 families) reflects a concern for attrition which grew as sampling and interviewing progressed.

Because of a concern for ethnic and racial difference in responses, an effort was made to balance the sample in terms of these variables. We employed a form of stratified random sampling in order to ensure adequate numbers of black, Puerto Rican, and white families. Had this not been done, there would have been an excess of Puerto Ricans and too few whites. At the beginning of the experiment, the sample composition was 34 percent black, 29 percent Puerto Rican, and 34 percent white (3 percent were "other").

Finally, our design recognizes that the experiment exists in competition with current welfare programs, and during its existence these programs may provide higher support levels. The likely result of such a situation is that some families will elect to receive welfare in preference to the experiments benefits. Rather than simply drop these families from our program and lose all of the effort invested in and information obtained from them, we chose to continue these families as part of the experimental group, but pay them only the minimum benefit. It would, of course, be of little use to pay them more, since welfare would only cut their payment by an equivalent amount. We do not believe this is by any means an optimal solution to the problem, but as yet we do not know of a better one.

FINDINGS

1. Earnings and Labor Supply

For this analysis, data were available for 15 months from Trenton and Paterson-Passaic. These two experimental sites represent 508 families or 42 percent of the 1213 families enrolled at the beginning of the experiment. However, attrition and incomplete interviews have reduced the total

number of cases to 385 families for whom income data were available from the pre-enrollment interview and the fifth quarterly interview. The small sample means that estimates rapidly become unstable as the number of variables examined simultaneously increases. Further, the Trenton, Paterson-Passaic subsample is not representative of the full sample as it is only 13 percent non-Spanish-speaking white (hereafter termed simply "white")--too few even to analyze when ethnicity is controlled. Consequently, we can say little about the behavior of these families.

Since the data base is considerably smaller than the initial sample of 508 families, some discussion of the discrepancy is in order. The main reason for the difference is panel mortality. Of the 363 families originally assigned to the experimental group and 145 families assigned to the control group, 19.3 percent were lost by the end of 12 months and 22.8 percent at the end of 15 months. Not unexpectedly, the rate is higher in the control group (34 percent) than in the experimental group (18.8 percent). It is also higher among whites (35.9 percent) than among the Spanish-speakers (25 percent) or blacks (20.5 percent).[9] At the end of the first year, the rate was lowest among those receiving higher than the minimum payment (8.3 percent; comparable data are not yet available for the fifth quarter). In Trenton 27.7 percent and in Paterson-Passaic 19.6 percent were lost. The latter differential is largely attributable to higher base payments and other efforts to cut attrition which resulted from our experience in Trenton.

Panel mortality is, of course, nothing new to survey analysts. But the cost of the usual solution, oversampling at the start, is far greater

in an income maintenance experiment than in other panel studies. Moreover, our lack of experience with panel studies of low income populations has meant that many of our customary methods for maintaining a panel were ineffective and new ones had to be devised. To be sure, some of our missing cases will be recovered as we locate some of those who have moved, but others will disappear. At this point it is difficult to estimate our final sample size.

Aside from attrition, the major cause of the decrease in sample size is that, for various reasons, some questionnaires are incomplete. Indeed, if we were to base the analysis on only those cases for which complete income information was available for both the pre-enrollment and fifth quarterly, the sample size would be 299 or 59 percent of the original 508 families. To recover some of this missing information, several steps were taken.[10] First, if, in a given interview, income was reported for one spouse and not for the other, the latter was assumed to be zero. Second, if a spouse reported that he (or she) was not working the previous week or if he (or she) did not answer the work question, the earnings item was assumed to be zero. Third, if a spouse reported that he (or she) was working and did not answer the earnings question, the observation was eliminated from the analysis. Fourth, if a respondent was female and without a spouse, her work and income data were defined as being of the head of the household. These procedures enable recovery of 93 observations, bringing the total subsample size for analysis to 392 or 77 percent of the initial data base.

Table 3 shows the effects of attrition on the Trenton, Paterson-Passaic subsample between the pre-enrollment and fifth quarterly interviews and provides a comparison with the full sample as of pre-enrollment.

Table 3 About Here

As noted in the Trenton, Paterson-Passaic subsample, the percentage of blacks has increased; the control group is now over half black. The average number of years of school completed has dropped by about one year while the average age of the head of the household has increased by about one year. Mean incomes have risen in both the experimental and control groups, with the experimental effect being concentrated in head's earnings. The probable effect of these changes is to make the estimates of experimental effects slightly conservative. Since the job market is less hospitable to blacks, the less educated, and older persons than to whites (including Spanish-speakers), the better educated, and younger persons, the greater the number of the former in the sample the less likely are differences between experimental and control groups.

Four measures of experimental effects are used here: (1) change in weekly earnings from all jobs of the head of the household; (2) change in the combined weekly earnings of the head and spouse (total family earnings); (3) change in the number of hours per week the head worked at all jobs; and (4) change in the wife's participation in the labor force.

Table 4 gives the results by experimental status. The percentage of the experimental group having the head's earnings increase by more

Table 4 About Here

than $25 exceeds the control group by 9 percent. In his earlier report for the first 12 months of the experiment, Watts (1970) found the differential to be only 6 percent, indicating a slight increase over the last quarter. In contrast, both Watts' data and our data reveal no

difference between the experimental and control groups with regard to total family income. Panel C of Table 4 suggests that some part of the increase in head's earnings may be due to the longer hours worked by the experimental group over the 15 months. The change in wife's labor force participation is somewhat stronger and indicates a greater tendency for experimental group wives to remain out of the labor force.[11] Sixty-five percent of the experimental group wives were out of the labor force at both points in time, while 57 percent of the control group stayed out.

The latter finding was generally expected by the experiment's staff, and so comes as no surprise. Since the best estimates (not very good ones) of the costs of work for working mothers indicate that half their income goes for job-related expenses (Addiss, 1963), and since cultural norms still define the woman's place as in the home, the benefits should keep secondary wage earners such as wives out of the labor force. The other findings were also expected.

Table 5 gives the same relationships, controlling for ethnicity. Because of their small number, whites were eliminated from the analysis.

Table 5 About Here

Panel A suggests that most of the difference between the experimental and control groups in Table 4 with respect to head's earnings are due to the Spanish-speaking part of the sample. Among blacks there is no substantial difference due to the payments, but a 10 percent difference exists among the Spanish-speakers. However, in terms of family earnings, there are again no differences between the experimental and control groups, although as a group blacks are about 15 percent more likely to increase their family income. This finding is essentially identical to one reported by Watts (1970) for the 12-month period. Part of the reason

for the lack of difference between the experimental and control blacks with regard to head's earnings may be that 12 percent of the experimental group decreased the number of hours they worked. In contrast, among Spanish-speakers 17 percent more of the experimental group increased their hours worked. Table 5 also reveals striking ethnic differences in wives' labor force participation. Regardless of experimental status, Spanish-speaking wives are far less likely than black wives to be in the labor force. Fully 70 percent of the Spanish-speaking wives in the control group were housewives at both points in time as against 45 percent of the black wives. Among the experimental group, the comparable figures were 72 percent and 53 percent. While there is a small experimental-control group difference in addition, it is utterly swamped by the ethnic difference. Clearly, the traditional sex roles are far more prevalent among the Spanish-speakers than among the blacks.

In Table 6 the basic relationships are stratified by husband's age. Among the age 18-30 group, head's earnings appear to decrease relative to the control group: 32 percent of the former saw their

Table 6 About Here

earnings decline as opposed to 20 percent of the controls. Matters reverse slightly among the age 31-40 group, but it is among the age 41-60 group that the experimental effect is strongest: fully 26 percent more of the experimental group increase their earnings by more than $25 a week.

The results for family earnings parallel those for head's earnings, in itself a striking contrast to the previous two tables. We have no particularly apt explanation for the impact of the experiment on older families. It may be that older men are more committed to traditional work values, but as yet we have no direct supporting evidence.

The pattern for head's earnings is repeated in the number of hours he worked. Those under 30 in the experimental group decreased their work week relative to the control group, while those over 30 increased it. As before, the change is particularly striking for the age 41-60 group. Perhaps it may involve some early or gradual retirement in this group which the experimental benefits in some way delay.

With respect to wives' labor force participation, there is a 10 percent experimental effect for the older two age groups, i.e., experimental wives are about 10 percent more likely to have been housewives at both points in time.

The effects of education are given in Table 7. They appear to interact with experimental status. Note that about 40 percent of the

Table 7 About Here

experimental group show an increase in head's earnings regardless of education, but among the control group those with 9 or more years of education are 10 percent more likely than those with less education to show an increase. In other words, the experimental effect appears to be greatest among those with the least education. With regard to family earnings the same pattern holds in diminished form: there is a small difference among those with less than 9 years of education and no difference among the more highly educated.

The number of hours the head worked also interacts with education. Among those with fewer than 9 years of education, the experimental group was 14 percent more likely to increase their hours. Among those with 9 or more years of education, there is only a 7 percent difference in the number who worked longer hours. Similarly, the data for spouses' labor force participation reveals interaction. Experimental status appears to have no effect among the low education group, but among the

high education group, spouses receiving benefits are 18 percent more likely to be housewives at both points in time. However, of the 18 percent only about 9 percent represents control group women who entered the labor force by the end of the 15-month interval. This may account for the rather small differential between the experimental and control group with respect to family earnings.

In addition to these gross measures of the family's style of and ability for coping with its economic situation, we sought to create a measure of the head's own estimate for his ability to deal with his situation. Accordingly, we constructed a "future control" scale of four items, each designed to elicit the respondent's perception of his ability to control his future. It is not used here as a personality index but rather as the respondent's experientally based estimate of his power to influence the course of his life. The four items comprising the index were:

1. People can control their own future and can determine how their lives will turn out.

2. Planning only makes a person unhappy since your plans hardly ever work out anyhow.

3. Nowadays with world conditions the way they are, the wise person lives for today and lets tomorrow take care of itself.

4. There is no sense in taking a chance failing at something new when I'm doing all right as I am.

Persons agreeing with the first item and disagreeing with the remaining three were given a score of one for each item with which they agreed or disagreed. The alternative responses were scored zero. The scale was created by summing the "one" scores, producing scale scores ranging

between zero and four. It was then dichotomized between the values of one and two. This formulation of the scale minimized the block response evident in the data: approximately 30 percent of the respondents gave the first response, regardless of the direction in which the item was phrased. Table 8 gives the results.

Table 8 About Here

In terms of head's earnings, the scale appears to affect only the experimental group, interacting with the experimental benefits to increase the experimental-control group difference from 5 percent among those low on the scale to 12 percent among those high on the scale. However, there is little difference between the groups in terms of change in family earnings.

However, there is more difference in hours worked. Those low on future control show little difference, but among those who are high on future control the experimental group is 11 percent more likely to increase their hours worked.

The major difference among the groups in terms of spouse's labor force participation also is between the low control group and the others. Spouses in the former are roughly 7-13 percent less likely to have been housewives during the entire 15 months than spouses in the latter groups.

To examine the effects of the five independent variables simultaneously, a regression model was constructed allowing for tests for interactions as suggested by the tabular analysis. Of course, the utility of the regression depends upon the vast majority of the hypothesized interactions being found to be zero, since a regression equation increasingly approximates its cross-tabular analog with all its sample size instabilities as the number of interaction terms increases. This

difficulty proved disabling in the regression models tested. A substantial number of interaction terms appeared to be statistically significant at the .05 level; moreover, there were significant interactions between experimental status and the other independent variables and among the other independent variables. However, interpretation of these results at this time is hazardous. Suffice to say that they are broadly consistent with the cross-tabular analysis, and further investigation must await the inclusion of additional sample observations.

Although we have thus far assumed that a respondent's self-perceived ability to control his future affects his response to the experimental benefits, it is also possible that the security offered by these benefits, in turn, affects his perceived (and/or real) ability. To test this notion, a second future control scale, identical to the first, was constructed from the fifth quarterly interview. The relationship between this second scale and the first, created from pre-enrollment items, was then examined, controlling separately for experimental status, guarantee level, tax rate, and income stratum. The results are in Table 9.

Table 9 About Here

Panel A of Table 9 shows that if one is in the experimental group and starts high in future control he is considerably more likely to remain high (65 percent do) than if he starts high and is in the control group (52 percent do). Moreover, if one starts low he is 6 percent less likely to remain low if he is in the experimental group. Panel B is comparable, except for an inexplicable dip in the percentage remaining high at the 75 percent guarantee level. Note that at the highest guarantees, 55 percent of those who started low end up high.

Panel C shows that as the tax rate increases the percentage who finish high decreases. Note that it is true for both those who started high and those who started low. Once again, at the highest benefit level (30 percent tax rate), a clear majority end up high regardless of their initial scale position.

Much the same pattern is repeated by income strata in Panel D. As income increases, so does the proportion who are high at the end of the fifth quarter.

It seems clear, then, that the benefits help sustain and can occasionally improve a respondent's impression of his ability to control his future. However, an assessment of the implications of this finding for respondent's economic performance must await data from future quarterly interviews.

2. Recipient Reaction to the Experiment

To get an initial idea of whether the respondent's behavior was affected by their knowledge of and attitudes toward experimental objectives, satisfaction with administrative details, and tolerance of the interviews, we decided to do open-ended interviews with a one-tenth random subsample in each of the five experimental sites. Six interviewers were specially trained for this task, and, collectively, they conducted 66 interviews lasting one and one-half to two hours.

The resulting data was grouped into three categories: (a) understanding of the program; (b) reactions to administrative practices; and (c) attitudes toward experimentation and the interviews.

A. Understanding of the Program

Data on the respondents' understanding of the program were obtained in two ways: they were asked what they remembered of the explanations given at the beginning of the program, and hypothetical situations were

presented to learn whether they understood how changes in their status might affect their receipt of benefits.

In general, understanding of the program was found to be poor. Families frequently misunderstood the eligibility criteria for the program and the manner in which their benefits were determined. Some families thought they qualified simply because of the size of their family; others thought it was because of their immediate financial situation. Most families knew that their payments were tied to their income, but few could specify the relationship other than to say that their benefits went down when their income went up. Few know their maximum payment of the procedure for determining it, and fewer still knew their breakeven point. Almost none knew that OEO was the source of funds for the program, and there was a similar lack of knowledge of the relation of the experiment to existing welfare programs. The most commonly known fact was the length of the program.

B. Reactions to Administrative Practices

Families were asked to report their income monthly and to submit pay stubs along with their reports. We wanted to learn whether the procedures for submitting these reports, their timing and the timing of payments created difficulties. In general, we found few complaints. Few families found the timing of either reports or of payments a problem, and almost all praised the simplicity of the income report form. However, a number of families found it difficult to save and submit pay stubs, particularly where there are children to find them.

C. Experimentation and Interviews

Since recipients were told that the program was experimental, we asked them for their views on experimentation and the interview sessions. Most respondents replied with a crude definition of experimentation, saying that it is to see if the program works. Others tended to answer vaguely, but approvingly. As for interviews, most simply said that the interviews were "OK," although there was frequent wonderment as to why some of the questions were asked.

D. Summary of Recipient Reactions

On balance, there seems to be little detailed knowledge or concern about the experiment's objectives or research or administrative procedures. Families are aware that the program affords some income security and know enough to complete the forms, but it cannot be said that they are fully cognizant of the consequences of their actions in terms of benefits. Their attitudes toward the experiment itself are vague, but generally favorable.

CONCLUSION

At the beginning of the urban negative tax experiment, few of us gave serious though to the possibility that it would have a positive incentive effect on head's earnings. Most anticipated either no effect or a mild disincentive. The question was how much would the program cost when the disincentive was measured. But at this point these expectations appear a bit pessimistic, and it seems fair to say that evidence for a disincentive is lacking--except, possibly, for secondary wage-earners. Whether an <u>incentive</u> effect exists is less clear; virtually none of the differences reported here are statistically significant, although the

consistency of the findings across the controls applied here and across time is impressive. The major difference between Watts' results for the 12-month period and those reported here is that the incentive effect appears to have increased somewhat over the 12 to 15 month interval. Moreover, we are probably underestimating the true incentive effect, since those families which have dropped out of the experiment are very largely those which, a priori, we would expect to show the greatest increases in earned income. Analyses of attrition in the full four-site sample in November 1969 and October 1970 revealed that at both points in time 71 percent of the attriters were at or above the breakeven point in earned income at the time of their attrition. If data for these lost families were included, it would in all likelihood increase the incentive effect observed.

But we must emphasize that these results are preliminary and should be interpreted with caution. Much data evaluation, data refinement, and analysis remains to be done. In a recent analysis of data for the full sample for the first year and half sample (Trenton and Paterson-Passaic) for the first 18 months, Watts (1971) found a small decrease in the head's number of hours worked and in family earnings, contrary to the findings reported here. However, Watts' data were based on "continuous" families--those which missed at most one interview. While we cannot at this point be certain, it appears that the discrepancy between the findings may be due to the fact that our data included families who missed more than one interview. It appears that these latter families are much like the attriters, i.e., they are close to or above their breakeven points. Consequently, they increase the average hours worked by the head and family earnings. Otherwise, our results are generally consistent with those of Watts.

NOTES

[1]Although called the "urban experiment," the experiment is not the only one in an urban area as there are other experiments underway in Gary, Seattle, and Denver. It has captured the name merely because it was the first experiment, and the second is based in rural areas.

[2]Under the present system, a poor family that qualifies for public assistance averages $9.50 monthly, whereas the same family in Massachusetts would receive $65.50 monthly (President's Commission, 1969).

[3]In some cases, the amount deducted may be slightly less than equal to earnings because of provisions in the welfare law which permit a small amount of earned income to be "overlooked." In several states, there are now small AFDC-UP programs which permit payments to male-headed households and which tax earnings at less than 100 percent.

[4]The intitial proposal for a negative tax experiment was developed by an economist, Heather Ross, in 1965. Her work led directly to the creation of the present experiment which is a joint undertaking of the Office of Economic Opportunity, the Institute for Research on Poverty at the University of Wisconsin and MATHEMATICA. The Poverty Institute has the major responsibility for the design of the experiment, interview construction, and data analysis. MATHEMATICA has prime responsibility for the field operations, payments, and coding. Overall responsibility rests with OEO.

[5]"Normal" income refers to an empirical approximation to a long-run income concept such as Friedman's Permanent Income. A regression is being developed to describe the average relation between family income and a fairly eclectic set of household characteristics; they are fitted to give a good approximation at the low end of the income distribution. "Normal" income is an interpolation between (1) a household's income as predicted by this and (2) its actual income over the most recent year as reported in a special screening interview.

[6]Because of the serial nature of the sampling, we were able to get some idea of the amount of panel mortality the experiment might experience. This knowledge led us to add 141 families to the control group at the end of the first year, and these families will enable us to obtain an estimate of possible Hawthorne effects. For a full description of the experimental procedures, see Urban Opinion Surveys (1971).

[7]Our transfers are annually adjusted for changes in the cost of living.

[8]Almost all of the refusals would have received the minimum payment.

[9]At the end of 12 months, Watts found the highest rate to be among the Spanish-speakers (Watts, 1970:14).

[10]The first three steps are identical to those reported by Watts (1970:15-16).

[11]The variable wife's labor force participation was obtained from the following question asked at both points in time: What were you doing during most of lask week--working for pay, keeping house, looking for work, or what? If a respondent replied that she was "working for pay," "with a job but not at work," or "looking for work," she coded as in the labor force. If she replied she was "keeping house," she was coded as a housewife. If she said she was "disabled" or "other," she was coded "other."

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TABLE 1

Negative Income Tax Plans in the New Jersey Experiment

("X" marks plans in use)

	Tax Rates		
Guarantee Levels	30%	50%	70%
.50 Poverty Line ($1,650)*	X	X	
.75 Poverty Line ($2,475)	X	X	X
1.00 Poverty Line ($3,300)		X	X
1.25 Poverty Line ($4,125)		X	

*Figures in parentheses were guarantee levels for a family of four at the beginning of the experiment. The guarantees are annually adjusted for increases in the cost of living.

TABLE 2

Guarantee Levels by Household Size

	Household Size						
Guarantee Levels	2	3	4	5	6	7	8+
.50 Poverty Line	$1,000	$1,375	$1,650	$1,850	$2,025	$2,175	$2,300
.75 Poverty Line	$1,500	$2,062	$2,475	$2,775	$3,037	$3,262	$3,450
1.00 Poverty Line	$2,000	$2,750	$3,300	$3,700	$4,050	$4,350	$4,600
1.25 Poverty Line	$2,500	$3,437	$4,125	$4,625	$5,062	$5,437	$5,750

Note: Figures are given for the beginning of the experiment (August, 1968); they are annually adjusted for increases in the cost of living.

TABLE 3

Comparison of Samples

	Full Sample, Pre-enrollment		Trenton, Paterson-Passaic, Pre-enrollment		Trenton, Paterson-Passaic, Fifth Quarter[a]	
	Exp.	Con.	Exp.	Con.	Exp.	Con.
Ethnicity						
White	33%	41%	13%	12%	12%	8%
Black	39	31	45	48	49	51
Spanish	29	28	42	40	40	41
Mean Weekly Income of Head[b]	$78.96	$79.82	$72.77	$69.59	$84.58	$72.48
Mean Weekly Income of Family	$88.70	$88.30	$80.66	$76.53	$93.02	$85.39
Mean Education of Head	8.63	8.69	7.96	7.46	7.09	6.27
Mean Age of Head	36.1	36.3	36.5	36.6	36.9	37.0

[a] Fifteen months after pre-enrollment

[b] Earnings from all jobs

TABLE 4

Measures of Experimental Effect by Experimental Status, Pre-enrollment and Fifth Quarterly Interviews

	Experimental	Control
A. Change in Head's Earnings		
Increased by more than $25	38%	28%
Stayed within $25	39	47
Decreased by more than $25	24	25
Total	101	100
N =	(290)	(102)
B. Change in Family's Earnings		
Increased by more than $25	38	37
Stayed within $25	35	36
Decreased by more than $25	27	27
Total	100	100
N =	(290)	(102)
C. Change in Hours Worked		
Increased	35	26
Stayed the same	30	39
Decreased	36	35
Total	101	100
N =	(290)	(102)
D. Change in Spouse's L.F. Part.		
In L.F. at t_1 and t_2	8	7
In L.F. at t_1, housewife at t_2	7	8
Housewife at t_1, in L.F. at t_2	9	15
Housewife at t_1 and t_2	65	57
Other	11	14
Total	100	101
N =	(269)	(92)

TABLE 5

Measures of Experimental Effect by Ethnicity and Experimental Status, Pre-enrollment and Fifth Quarterly Interviews

	Blacks		Spanish-speakers	
	Experimental	Control	Experimental	Control
A. Change in Head's Earnings				
Increase by more than $25	41%	40%	30%	20%
Stayed within $25	34	38	46	61
Decreased by more than $25	25	22	24	20
Total	100	100	100	101
N =	(133)	(50)	(111)	(41)
B. Change in Family's Earnings				
Increase by more than $25	43%	46%	29%	29%
Stayed within $25	29	32	41	46
Decreased by more than $25	28	22	30	24
Total	100	100	100	99
N =	(133)	(50)	(111)	(41)

TABLE 5 (con't)

	Blacks		Spanish-speakers	
	Experimental	Control	Experimental	Control
C. Change in Hours Worked				
Increased	36%	40%	32%	15%
Stayed the same	26	34	35	46
Decreased	38	26	32	39
Total	100	100	99	100
N =	(133)	(50)	(111)	(41)
D. Change in Spouse's L.F. part.				
In L.F. at t_1 and t_2	15%	10%	4%	3%
In L.F. at t_1, housewife at t_2	7	8	7	9
Housewife at t_1, in L.F. at t_2	13	18	4	9
Housewife at t_1 and t_2	53	45	72	70
Other	13	18	13	9
Total	101	99	100	100
N =	(123)	(49)	(105)	(33)

TABLE 6

Measures of Experimental Effect by Husband's Age and Experimental Status, Pre-enrollment and Fifth Quarterly Interviews

	18-30		31-40		41-60	
	Experimental	Control	Experimental	Control	Experimental	Control
A. Change in Head's Earnings						
Increased by more than $25	33%	40%	37%	29%	42%	16%
Stayed within $25	34	40	42	54	39	45
Decreased by more than $25	32	20	22	17	19	39
Total	99	100	101	100	100	100
N =	(87)	(30)	(106)	(41)	(97)	(31)
B. Change in Family's Earnings						
Increased by more than $25	33%	53%	39%	39%	41%	19%
Stayed within $25	32	27	37	41	35	39
Decreased by more than $25	34	20	25	20	24	42
Total	100	100	101	100	100	100
N =	(87)	(30)	(106)	(41)	(97)	(31)

TABLE 6 (con't)

	18-30		31-40		41-60	
	Experimental	Control	Experimental	Control	Experimental	Control
C. Change in Hours Worked						
Increased	33%	40%	35%	22%	34%	16%
Stayed the same	28	33	29	41	33	42
Decreased	41	27	36	37	32	42
Total	100	100	100	100	99	100
N =	(87)	(30)	(107)	(41)	(99)	(31)
D. Changes in Spouse's L.F. Part.						
In L.F. at t_1 and t_2	5%	0%	8%	5%	11%	17%
In L.F. at t_1, housewife at t_2	8	7	6	8	7	8
Housewife at t_1, in L.F. at t_2	8	27	10	13	8	4
Housewife at t_1 and t_2	68	67	67	58	60	50
Other	11	9	9	16	15	21
Total	100	100	100	100	100	100
N =	(79)	(30)	(101)	(38)	(89)	(24)

TABLE 7

Measures of Experimental Effect by Husband's Education and Experimental Status, Pre-enrollment and Fifth Quarterly Interviews

	Less Than 9 Years		9 Years or More	
	Experimental	Control	Experimental	Control
A. Change in Head's Earnings				
Increased by more than $25	42%	25%	40%	35%
Stayed within $25	37	53	37	38
Decreased by more than $25	20	22	24	27
Total	99	100	101	100
N =	(132)	(51)	(126)	(37)
B. Change in Family's Earnings				
Increased by more than $25	42%	35%	40%	41%
Stayed within $25	33	37	34	35
Decreased by more than $25	25	27	25	24
Total	100	99	99	100
N =	(132)	(51)	(126)	(37)

TABLE 7 (con't)

	Less Than 9 years		9 Years or More	
	Experimental	Control	Experimental	Control
C. Change in Hours Worked				
Increased	36%	22%	39%	32%
Stayed the same	31	47	24	27
Decreased	33	31	37	41
Total	100	100	100	100
N =	(132)	(51)	(126)	(37)
D. Change in Spouse's L.F. Part.				
In L.F. at t_1 and t_2	9%	4%	4%	3%
In L.F. at t_1, housewife at t_2	8	8	5	6
Housewife at t_1, in L.F. at t_2	7	10	10	19
Housewife at t_1 and t_2	65	67	71	53
Other	11	10	9	19
Total	100	99	100	100
N =	(126)	(48)	(115)	(32)

TABLE 8

Measures of Experimental Effect by Pre-enrollment Future Control Scale and Experimental Status, Pre-enrollment and Fifth Quarterly Interviews

	Low		High	
	Experimental	Control	Experimental	Control
A. Change in Head's Earnings				
Increased by more than $25	33%	28%	42%	30%
Stayed within $25	41	46	36	49
Decreased by more than $25	26	26	21	22
Total	100	100	99	101
N =	(141)	(65)	(149)	(37)
B. Change in Family's Earnings				
Increased by more than $25	34%	37%	42%	38%
Stayed within $25	35	34	35	41
Decreased by more than $25	31	29	24	22
Total	100	100	101	101
N =	(141)	(65)	(149)	(37)

TABLE 8 (con't)

	Low		High	
	Experimental	Control	Experimental	Control
C. Change in Hours Worked				
Increased	31%	25%	38%	27%
Stayed the same	33	40	27	38
Decreased	36	35	35	35
Total	100	100	100	100
N =	(141)	(65)	(149)	(37)
D. Change in Spouse's L.F. Part.				
In L.F. at t_1 and t_2	9%	7%	8%	8%
In L.F. at t_1, housewife at t_2	5	10	8	3
Housewife at t_1, in L.F. at t_2	6	10	11	23
Housewife at t_1 and t_2	64	53	66	60
Other	17	19	7	6
Total	101	99	100	100
N =	(127)	(58)	(142)	(35)

TABLE 9

Interrelation between Future Control Scales by Experimental Status, Guarantee Level, Tax Rate, and Income Stratum

	Fifth Quarter High	Fifth Quarter Low	Total	N=
A. Experimental status				
Experimental				
High	65	35	100%	(127)
Low	48	52	100	(139)
Control				
High	52	48	100	(29)
Low	42	58	100	(64)
B. Guarantee level[a]				
50% of poverty line				
High	68	32	100	(28)
Low	45	55	100	(31)
75% of poverty line				
High	59	41	100	(54)
Low	44	56	100	(59)
100-125% of poverty line				
High	69	31	100	(45)
Low	55	45	100	(49)
C. Tax rate[b]				
30% tax				
High	74	26	100	(35)
Low	55	46	100	(22)
50% tax				
High	64	36	100	(67)
Low	49	51	100	(71)
70% tax				
High	52	48	100	(25)
Low	44	57	100	(46)

TABLE 9 (con't)

	Fifth Quarter			
	High	Low	Total	N=
D. Income stratum[c]				
I				
High	59	41	100	(49)
Low	43	57	100	(68)
II				
High	59	41	100	(49)
Low	46	54	100	(59)
III				
High	67	33	100	(58)
Low	50	50	100	(76)

[a]A zero guarantee defines the control group above.

[b]A tax rate of zero defines the control group above.

[c]The three strata are (I) family income below $3300 per year for a family of four; (II) $3301 to $4125 a year for a family of four; and (III) $4126 to $4950 for a family of four. The levels are based upon revisions of the 1965 Social Security Administration poverty lines.