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ABSTRACT

This paper presents two different theories to account for the effects of a negative income tax on labor supply. The static theory predicts a work disincentive while the dynamic theory contends that no a priori prediction can be made. Within this context, preliminary results are given for the effects of 18 months' experimental payments on the probabilities of employment and labor force participation for male heads of households. The results indicate that the payments have not changed these probabilities. Should the results hold up as additional data become available, two conclusions would seem warranted. First, the dynamic theory would appear to be preferable to the static as a description of labor supply behavior under a negative tax. Second, the costs of a national negative tax program may be substantially less than estimates from analyses of nonexperimental data which have presumed the validity of the static theory.

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This is a preliminary report on the labor force participation of male heads of households during the first 18 months of the urban negative income tax experiment in New Jersey and Pennsylvania. The grounds for the study of labor force participation are twofold. First, both economic and sociological theory predict that, other things equal, labor force participation will decline if the cost of not working is reduced by the presence of negative income tax benefits. Second, some economists and sociologists suspect that while theory would predict a decline in the number of hours worked per week, most workers, because of the way work is organized, cannot decide how many hours they will work but only whether they will work. There is a dual rationale for this belief. In the first place the theory predicting a decline in hours worked is predicated on the assumption of a continuous demand schedule for hours of labor, i.e., workers can adjust their hours supplied to the market in response to its changing preferences. The accuracy of this assumption is doubtful in the short-run and in loose labor market conditions. (See Cain and Watts, forthcoming.) In the second place, labor supply flexibility is particularly limited for the wage-earning occupations in which the urban working poor are concentrated. Employers set the number of hours per week an employee must work in a particular job, and the employee works fewer hours at the risk of discharge. Thus labor force participation may be a more critical labor supply response to the experiment than the number of hours per week worked.[1]

As this is a preliminary report, it must be stressed at the outset that the results reported here, as with other preliminary reports from the experiment, are to be seen as provisional and subject to change as additional data become available and fuller analyses are performed. Moreover, we shall not recapitulate here a detailed description of the experiment as it is already available in Watts (1969) and Elesh et al. (1971).

The Static Economic Model

The basic, static economic model of labor force participation is an application of the general theory of choice. The unit of analysis--and in this case the decision-making unit--is defined as the household, although the focus here is on the male head's labor supply. The household is made up of individuals or groups of individuals whose preference functions are combined to form the aggregate household preferences and whose activities determine its resource base. It is possible to conceptualize the time utilization of each member of the household, or indeed the whole household, as falling into two categories, work or nonwork: in other words, into resource-producing or resource-expending activities. All activities, with the exception of market work, consume time which, given its general scarcity, could have otherwise been used to produce more income. Hence, in terms of the household's ability to maximize its utility function, these activities cost the family through lost resources. This resource development-resource expenditure dichotomy is

commonly referred to in economic literature as the work-leisure decision. (Leisure in this usage comprises all nonwork activities including, for example, investment in training programs and job searches in order to find a better job.) The household must determine its division of labor by allocating its members' time into work or leisure activities. This time allocation decision, then, is a function of four different factors: (1) the family's values or preferences; (2) the expected market earning rate of its members; (3) the household's expected nonmarket earning rate; and (4) the household's total resource constraint. This last factor is a function of the total number of hours available to be allocated, the set of prospective market and nonmarket earning rates, whatever other earnings the household expects to receive, and the monetary value of the household's saleable assets.

Of the four parameters of the household's allocation decision, values --usually the emptiest of the economist's conceptual boxes, as Bowen and Finegan (1969) point out--reflect the household's demand for goods and income. The expected return on market work reflects the probability of an individual finding gainful employment and his rate of return for his work. The expected nonmarket earning rates reflect the costs of the individuals working to the household. The family's resource constraint reflects the household's potential assets and liabilities. Using these four factors, the economists then posit a relationship between the household's allocation of time to work and leisure, and changes in the levels of these factors.

The two general classes of effects on labor force participation in the economists' model are the income and the substitution effects. Both

of these effects involve an adjustment in household labor supply as responses to changes in economic incentives. The income effect postulates that an increase in the disposable income of the household will lead to a purchase of more leisure and, hence, have a negative effect on hours worked. The substitution effect, on the other hand, asserts that an increase in the return for work will increase a household's labor supply activity, since work will be substituted for leisure. A change in any one of the variables mentioned above could lead to either of these two effects, and certain types of changes can lead to a joint effect. For example, an increase in wage rate will lead to a substitution effect in that the cost of leisure has increased relative to the cost of work while, on the other hand, an increase in the return for work will lead to an increase in income, leading in turn to an increase in the consumption of all goods, including leisure. Other variable changes might include increases in nonemployment income (e.g., in the context of the experiment, the benefits), in the nonmarket costs of work (e.g., expenses for child care), and a deterioration of health (which may reduce the return for work). All of these changes should produce a negative effect on labor supply.

Structural variables such as unemployment rate, industry mix, and general wage structures of the locality are added to the model when the data are aggregated. These variables would ordinarily be included as specifications of the household's total resource constraint since they affect the probability of success in looking for market work, the return on market work, and the value of a family's assets.

In circumstances in which both an income and a substitution effect are expected, the economists' results normally suggest that the income effect predominates (hence the backward bending supply curve of labor). Given the work-leisure preference which underlies this phenomena, however, the decision to increase work or leisure is a function of the individual household's marginal preference for each, i.e., their values. And as mentioned above, this concept, neither easily quantified or conceptualized within this model, is not well suited to the economists' ananlysis. Individual characteristics of the household which affect their demand structure are crucial to the operation of this model, but generally fall outside the scope of the economists and into the realm of the sociologists.

Sociological Perspectives

Sociological perspectives on the labor supply response to the experiment are at once more vague and more complex than the relatively straightforward economic model, inasmuch as they introduce the values the economic model largely ignores and other factors which may condition and interact with the individual's market and nonmarket earning rates and resource constraints. Moreover, the list of potentially relevant factors is essentially arbitrary in length, since there has been little systematic theoretical or empirical work which assesses their relative importance or interconnections. Thus, the model presented here is but one among a number that might be specified with equal a priori plausibility.

The importance of values has been stressed in much of the noneconomic literature on work incentives and the causes of poverty. One major segment of opinion (e.g., Lewis, 1966; Banfield, 1970) has posited a culture, characteristic of those in poverty, which acts to inhibit social mobility. For example, Banfield (1970) has argued that the poor's present time orientation prevents them from taking those actions which would move them out of poverty. Ostensibly, those in the culture of poverty differ from the economists' utility maximizers in that the former would somehow use the experimental benefits to purchase more leisure than the latter or spend it more frivolously. However, the validity of the culture of poverty viewpoint recently has been challenged on both theoretical and empirical grounds (Valentine, 1968; Gans, 1968; Elesh, 1973), and consequently its usefulness here is dubious. Values are probably more fruitfully seen as associated with ethnic, racial, and other groupings or as independent of such categories. From this perspective, values do not distinguish the poor from the rest of society but rather may differentiate among the groupings in society in terms of their preferences for work over leisure. If these differential preferences do exist, they can enter the theoretical model in two ways. First, other things equal, the greater the preference for work over leisure, the greater the labor force participation rate. Second, since experimental benefits are paid to part of the sample--reducing the cost of leisure--there may be an interaction between being in the experimental group and work preferences resulting in a greater purchase of leisure relative to the control group. The importance of these interactions may be particularly salient to an examination

of the labor force participation of prime-age married males, for whom the socially prescribed "breadwinner" role is likely to be particularly strong and hence produce a very strong taste for work over leisure. This preference is reflected in the very high participation rates for married males, wife present, aged 25-54, consistently found in labor force studies (e.g., Bowen and Finegan, 1969; Cain, 1966). Values also enter the model through their correlations with the other factors which condition and interact with labor supply response, since it is seldom possible to separate entirely the value and nonvalue components of these factors.

The other factors which shall be examined here are the age of the male head of the household, his ethnicity or race, his family size, family type, health, education, history of labor force participation, job satisfaction, local labor market conditions, and welfare status. All but the last of these factors have been frequently employed in studies of labor force participation. The effect of age is complex, reflecting cohort changes in life expectations among different generations (a somewhat difficult concept to quantify), the value the labor market places on men of different ages in low-skill occupations, and the individual perceptions of the value of remaining income-producing years. Some writers (Blauner, 1964:118-119, 164) have argued that alienation from work is concentrated among the young; on the other hand, Hannerz's (1969) division of his black adult male population into swingers (mostly young and socially active), mainstreamers (older and more conforming to normal mores), and

street corner men (also older but more marginal than either swingers or mainstreamers) would suggest that it may be difficult to ascribe alienation from work to specific age groups in terms of cohort differences. The value the labor market places on men of different ages in low-skill occupations is considerably clearer. Studies by Miller (1966), Hanoch (1967), and Rees and Schultz (1970) have consistently shown that peak earnings typically occur before the age of 40-45 and decline thereafter. Employers frequently say that such workers are "irresponsible" in their twenties, "responsible" in their thirties, and "over the hill" in their forties. Given the relatively short period during which these men are at the peak of their earning power, one might expect that, relative to men older or younger, men in their thirties would prefer work to leisure. At the same time, the younger men may use experimental benefits to opt for leisure. These hypotheses, of course, depend upon other things being equal and a "rational" perception of the value of one's remaining work life. These considerations lead to the expectation that age will have direct, albeit unpredictable, effects on labor-supply response and indirect effects operating through an interaction with values.

The ethnicity or race of the male head of household is also a complex factor combining both the discrimination which hampers the advancement of blacks and Spanish-speakers and the distinctive subcultural values of these groups. It has been argued that the barriers to these groups have created alienation to work within them (Clark, 1965; Dizard, 1968) frequently supported by subcultural values which have

arisen to rationalize the inability to achieve occupationally. To the extent that such alienation and barriers exist, one may expect the experimental benefits to lead to the choice of leisure over work for these groups.

Family size and type are factors which affect a male head's need to work. In general, the male head of a large family, having more dependents than the head of a small one, will feel a greater need to work. His wife probably will be less able to contribute to family income, since the higher cost of child care will increase the value of her nonmarket earnings and tend to keep her out of the labor force; she also may have infants whose care she may not wish to delegate (Cain, 1966). Moreover, to rephrase the arguments of Moynihan (1965), Hannerz (1969), and Leibow (1967), the male head of a nuclear family will feel more need to work than the head of one of the more marginal family types because of his identification of the working role with family headship. If this is the case, then the role obligations of family headship may serve to insulate the male from the effect of the experimental payments.

More generally, it seems useful to divide the labor force into stable and unstable participants on the basis of individuals' histories of participation. Stable participation indicates in part a commitment to work as a value. But it may also reflect the lack of a physical or mental disability, the availability of work, and the match of demanded and supplied skill levels. Thus the interpretation of an individual's history of participation must be conditioned by the extent to which his health permits him to work, the fluctuations of the labor market, and the extent to which his level of education makes him a demanded worker.

To be sure, an individual's health, education, and local labor market conditons also will affect his work decision directly. If he has a chronic health condition or is chronically in poor health, he will likely work less than a healthy person; he is also likely to be predisposed to substitute experimental benefits for work--particularly since, as Liebow (1967) suggests, his health may make the kind of work he can get both difficult and unpleasant. Indeed, he may only be able to hold certain kinds of jobs. On the other hand, the higher an individual's educational attainment, the more he should choose to work inasmuch as education typically carries a desire for a higher standard of living along with a better ability to command it. Thus, one may expect the substitution effect of education to dominate the income effect. Similarly the more satisfaction the individual derives from his job, the less likely he is to decrease his work effort, since high job satisfaction reflects not only the individual's unwillingness to endanger his current position by reducing his work effort but also the lower psychic costs of working at that job compared with less satisfying positions. However, perhaps the effect of local labor market conditions is the most fundamental in that the significance of the decision to work or not to work depends upon whether work is available, and market conditions may interact with experimental status through the operation of what have been termed the "discouraged and encouraged worker hypotheses" (Mincer, 1962). The discouraged worker hypothesis is an explanation for the common finding

that if labor market conditions are loose and unemployment is high, more people leave the labor force (or fail to enter) than would be expected in terms of economic conditions. The converse hypothesis is used to explain, in tight labor markets, the converse finding. Now suppose that, ignoring labor market conditions, the income effect dominates the substitution effect and more of the experimental than the control group do not work. In a loose labor market, this effect may interact with the receipt of experimental benefits inasmuch as one can better "afford" to be discouraged, and more workers may leave the labor force than the simple additive effects of labor market conditions and experimental status would lead one to predict. Conversely, if we assume that the substitution effect dominates the income effect such that more of the experimental than control group work and that there is a tight labor market, the interaction between experimental benefits and labor market conditions may show more workers entering the labor force than economic conditions would lead one to expect.

Finally, there are the possible effects on labor supply which are due to the fact that we are dealing with an experiment rather than a national program and the experiment exists in a natural setting which includes a welfare program as an alternative to the experiment's benefits. Although the welfare problem exists in both states where there are experimental sites (New Jersey and Pennsylvania) it is most severe in the former as that state enacted an Aid to Dependent Children-Unemployed Person (AFDC-UP) program during the experiment's first year which paid

benefits to intact families competitive with those of the experiment.[2] Other things equal, those on welfare will necessarily have lower labor force participation rates than those who are not since they must be unemployed at the initial receipt of it. Consequently, the welfare status of the families must be controlled.

It should be evident that the above factors, taken together with the negative tax treatments, constitute a rather complex model for the labor supply response to the experiment. But it also should be remembered that however the nontax variables may modify the effects of the negative tax, the static theory presumes that the net effect of the negative tax will remain negative, i.e., labor supply will be reduced. The role of the nontax factors is simply to specify how the disincentive varies with the characteristics of the tax recipients in order to estimate the dollar value of the disincentive for various subpopulations and the population as a whole.

Outlines of a Dynamic Theory

However, one may alternatively view a negative tax from a dynamic perspective, and from this standpoint, its effect on labor supply is not so clear. Despite the seemingly universal expectation of a disincentive--perhaps because of the great attention given the static theory in the literature--a negative income tax need not reduce the supply of labor. Indeed, in the long run, it may increase labor supply. In saying this, we note that as the machinations of a dynamic theory require "the long run," and as the relevant data are not as yet available,

the results presented here do not allow judgments as to the relative appropriateness of the alternative models. Consequently, this section should be considered a reminder that the problem in terms of economic and sociological theory is not how well the response to the experimental payment fits the static model but rather it is which of the two alternative models most appropriately describes the data.

We begin by noting that the possible responses to a negative tax are bounded on the one hand by a simple addition of the experimental benefits to income without any reduction of work effort, and on the other hand by a reduction in work effort commensurate with the dollar value of the benefit. Given these extremes, it seems reasonable to assume that many, if not most, individuals will respond somewhere between these two alternatives. For those with at least some additional income, the question thus arises as to how they will consume it. They may either consume it through the purchase of goods and services from which no return is expected or they may buy goods and services which will bring a return. Either option may cause them to maintain or increase their work effort. Conlisk (1968) has suggested that if they choose the first, they may grow accustomed to the new standard of living their payments make possible and continue working to maintain it; or they may incur financial obligations which they then feel obligated to continue to meet. If they choose the second alternative and receive a return on their consumption, they may be investing in themselves and their family in ways which may improve both the quality and quantity of their labor. For example, Boskin (1967) has suggested that recipients may invest their benefits to improve

their education and health. Similarly, Garfinkel (1971) believes they may invest in on-the-job and off-the-job training. Thus whether the long-run, net, aggregate effect of a negative tax is positive, negative, or zero depends upon the extent of and rates of return to investment and the extent to which families change their standards of living. It is quite possible that investment will be large enough to produce a change in labor supply sufficient to increase earnings--an incentive effect.

There are at least six basic forms of investment capable of creating incentive effects.[3] First, the male head may undertake a search for a better job (one which offers higher wages or surer prospects for advancement). Second, he may migrate to a tighter labor market in order to command a better job. Third, he may take on-the-job training. Fourth, he may take off-the-job training. Fifth, he may seek other forms of education to improve his general skill level. Sixth, if he has a health problem (or his family has a health problem) which limits his ability to work or the kind of work he can do, he may obtain medical assistance or improve his diet or housing to reduce or to eliminate his problem. None of these forms of investment are mutually exclusive (in fact, in some cases, investment in health may be an a priori condition for investment in the others)[4] and although they are probably the most important types of investment, they also are not exhaustive. An individual may, if he feels the need and has the resources, invest in all six (and other) ways.

But clearly, those who receive negative tax benefits are, other things equal, more likely than those who do not receive benefits to invest since they have more resources with which to invest and, perhaps,

a sense of security that comes from knowledge that the program provides a guaranteed floor to their incomes. It further follows that investment should increase with the generosity of the tax plan. Thus, in the long run, those who invest their benefits should be able to command jobs at higher wages. And since, as argued above, wage earners cannot choose how many hours per week they will work (except, of course, for overtime) and the normal work week is unlikely to vary much from job to job, it follows that their earned incomes will probably rise. Thus, while the static theory predicts a work disincentive due to negative tax payments, the dynamic theory predicts the payments will produce a work incentive for those who invest their benefits.

But not all of the poor are equally likely to invest. Among their characteristics which influence this probability are age, ethnicity, family size, wage rate, and values. It is almost axiomatic in the human-capital literature that because the rate of return to investment declines with age so will the probability of investment. Ethnicity affects the probability of investment (1) through the discrimination which various ethnic groups face in the opportunities for investment, opportunities for employment, and rates of return to investment and (2) through the discouragement such discrimination produces. Family size will affect the probability in that as family size increases so do the hardships created by the diversion of income from consumption to investment. Similarly, investment should increase with wage rates since the further a family is from the minimal income required for sustenance, the freer they will feel about investing. Finally, if a family aspires to a standard of living

higher than that which they currently possess, they will be more likely to invest.

Moreover, not all forms of investment are equally probable for the poor. The most likely investment would be in health through the improved diet likely to result from greater expenditures on food.[5] The second most likely investment is the job search, since it generally will be perceived (and often will have) the lowest cost and quickest return of the remaining five possibilities. Benefits could be used to subsidize longer searches and to permit acquisition of greater amounts of job information. The probabilities of the remaining four investment forms are not so clearly orderable, although it seems likely that job-specific training will be chosen more often than general education, since it is likely to be perceived as offering a quicker return. The difficulties in determining the relative probabilities of investment reflect the fact that probabilities also depend on (1) the nature of the labor market and (2) the composition of the poor with regard to the personal characteristics which affect the probability of investment.

It should be evident from this discussion that a dynamic approach does not permit an a priori deduction as to the direction of the labor supply response to a negative tax. For example, a negative tax response in the short run is quite compatible with a long run positive response, given that investment may entail a reduction in wage rates, hours worked, or even employment in the short run. It is also possible for a negative tax to show no net effect because those who decreased their labor supply were matched by others who increased it. And, to be sure, the net effect

may be negative, as predicted by the static theory. The point is that the result can not be predicted from existing knowledge.

The Data

1. The Sample

Since the major purpose of the experiment is to assess the labor supply response of the poor in intact families, the urban experiment is restricted to families (1) with at least one dependent person and a nonstudent male who is able to work and between 18 and 58 years of age and (2) with total family income no more than 150 percent of the poverty line for each family size. The sample was drawn from poverty tracts: Trenton, Paterson-Passaic, and Jersey City, New Jersey; and Scranton, Pennsylvania. To provide administrative flexibility the sample was drawn sequentially beginning in August, 1968, in Trenton and ending in September, 1969, in Scranton. Since administrative procedures were improved as experience warranted, the effects of these procedural changes may vary over the different cities. Consequently, the experimental site, in addition to the local unemployment rate, is introduced into our analysis as a control for this possibility.

The basic design contains an experimental and a control group. Once eligibility was determined from a special screening interview, families were assigned to one of eight negative tax plans, which together define the experimental group, or to the control group using a design optimization model developed by Conlisk and Watts (1969); this uses a measure of a family permanent income adjusted for family size for the assignment.

The objective of the design model was to minimize, given certain budgetary constraints, the error variance of the estimates of changes of family earnings induced by the experimental payments. To attain comparable precision with a purely random assignment would have required a substantially larger sample at much greater cost. However, use of this model means that the various experimental plans differed in average family income at the beginning of the experiment, and consequently all analyses must control for the income stratum of the family used in the assignment process. 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 are the rates at which benefits are reduced as family income rises. Guarantee levels are the annual values of the benefits paid when family income is zero.[7] Tax rates range from 30 to 70 percent, and guarantee levels vary from 50 to 125 percent of the poverty line for each family size. Table 1 shows the combinations selected for experimentation. Seven hundred twenty-five families were assigned to the experimental group and 632 were placed in the control group. The sample was stratified by the three major ethnic groups in the experimental sites--blacks, whites, and Spanish-speakers--and by three income strata where income was weighted by family size.

After families were assigned to groups, all (experimental and control) received a pre-enrollment interview to obtain baseline data in a variety of areas uncontaminated by knowledge of the experiment or the inception of transfers. Subsequently, the three-year program was explained to the experimental families and they began to receive payments, which are made

every two weeks. Their only obligation is to report their income and family composition each month. Both the experimental and control groups are interviewed quarterly.

In assessing the characteristics of the sample for this analysis, it is important to keep several points in mind. First, neither the experiment nor this sample contains a random sample of America's poor. This sample is restricted to urban household heads who are between 18 and 58 years of age. The large number of poor who are old, single, or who live in female-headed households or in rural areas are not represented. Second, the urban areas for which this sample is drawn are located exclusively in the northeastern part of the country. Third, because the data are collected over time, attrition of sample observations which is selective in terms of experimentally related characteristics may bias the estimates of experimental parameters. In an attempt to measure the effects of such attrition, our results are given for two samples. The first or full sample (N=1, 166) contains all male-headed families for which a pre-enrollment interview exists.[8] The second or "continuous" sample (N=909) consists of all male-headed families for whom the pre-enrollment and sixth quarterly interviews are available and who missed no more than two of the remaining five interviews. Since the full sample contains the attriters prior to their departure, comparison of the results for the full and continuous samples provides a gross measure of the effects of attrition.[9]

The total sample size available for this analysis is 1,166. Of the 1,357 families in the total enrolled sample, 141 were omitted because they

were additional control observations introduced after the pre-enrollment interview, 47 were dropped for lack of a male head of household at pre-enrollment either because of family splits between the screening and pre-enrollment interviews or because a few female-headed households with a qualifying male were enrolled, and three families are missing their pre-enrollment interview.

2. The Estimated Model

Two measures of labor force participation are analyzed in this paper. Following the definitions of the U.S. Census, the male head was defined as being employed if, during the week prior to the interview, he reported doing any work for pay or having a job from which he was temporarily absent; he was defined as being in the labor force if he was employed in terms of the previous definition or if he stated he was looking for work. All other responses were defined as being out of the labor force. The first measure of labor force activity is the number of quarters a respondent reported being employed as a percent of all quarters for which an interview exists for him (Pct. Emp.). The second measure of participation is the number of quarters a respondent reported being in the labor force as a percent of all quarters for which an interview exists for him (Pct. L.F.). Thus the two measures are interpretable as a probability of employment and a probability of being in the labor force, respectively, given that an interview was conducted.

These two measures are then, consistent with the theoretical perspective above, regressed on measures of the male head's age, ethnicity, education, family size, family type, stability of labor force participation,

health, job satisfaction, welfare status, city of residence, experimental status, city unemployment rate, and income stratum. In addition, the full model contains several hypothesized interactions: experimental status is crossed with age, ethnicity, family types, stability of labor force participation, health, city, and job satisfaction; the measure of job satisfaction is crossed with ethnicity and income stratum.[10]

Clearly, the model to be estimated is quite large. However, large models are characteristic of analyses of labor force participation and simply reflect the fact that a variety of factors affect participation rates. Except for the interaction terms, which are necessary to assess fully the impact of the experimental payments, the model is quite consistent with those in the literature (c.f., Bowen and Finegan).

Results

If the static theory is correct, then a major outcome of the experiment should be an increase in the proportion receiving payments who either are unemployed or out of the labor force. A crude test of this prediction is to compare the labor force status of the experimental and control groups at each quarter. In making this comparison we exclude all families on welfare and control for the income stratum of the remaining families. To include the welfare families in the analysis would be to compare the experimental group with a control group containing families receiving benefits similar to the experiment's. But it also must be noted that the exclusion of experimental families on welfare has the effect of biasing the experimental group toward the high payment plans,

since it is known from administrative analysis (data not shown) that it was the families in the low payment plans who were the most likely to switch to welfare. Since it is in the high payment plans that the static theory predicts the greatest disincentive, the effect of the bias is to increase the probability of finding a disincentive. Because, as noted, families were assigned to experimental plans on the basis of their income stratum, this variable is controlled for. To assess the effects of sample attrition, the analysis is performed on both the full and continuous samples. But Table 2 reveals that, despite these controls, there are no differences in labor force activity by experimental status that are not readily attributable to chance. There is no pattern to the differences between the two groups nor is there a consistent decline of labor force participation over time among those families receiving payments.

However, these results compare only aggregate rates at each point in time and therefore may mask a difference which may exist in terms of individual labor force histories. For example, the experimental benefits may influence the stability of the male head's labor force participation. One way to assess the experiment's impact on the stability of participation is to compare the distribution of the two groups by the number of quarters the respondents have worked. The data is presented for the continuous sample only, since it is the sequence of labor force activity which is the focus of interest. Table 3 contains the relevant data, but, once again, no significant experimental differences emerge.

It is also possible that the gross division of the sample into experimental and control groups may mask an effect which is concentrated

in a few treatments. For example, there may be a threshold effect in the payments with the predicted disincentives occurring only among the highest paying plans. To examine this notion, we regressed the two dependent variables, percent of quarters employed (Pct. Emp.), and percent of quarters in the labor force (Pct. L.F.) on the four guarantee levels, three tax rates, and eight guarantee-tax rate combinations, respectively, controlling for welfare status and stratum.

Panel A of Table 4 gives the results for the four guarantee levels. According to the static theory, the coefficients should be negative and increase in absolute value as the guarantee level increases. But looking first at the full sample data, it can be seen that neither the signs nor the sizes of the coefficients fit expectations. Although disincentive effects, statistically significant at the .05 level on a one-tailed test, are found for the 50 and 100 percent guarantees in the Pct. Emp. equation, and the 100 percent guarantee in the Pct. L.F. equation, inspection of the coefficients for the continuous sample suggests that these results are largely attributable to differential attrition, since only the 100 percent guarantee in the Pct. Emp. equation remains significant. Moreoever, the sizes of the coefficients in the continuous sample data show the same lack of expected pattern as in the full sample equations.

In contrast, the coefficients for the tax rates in panel B appear to fit expectations reasonably well. With the exception of the full sample, Pct. L.F. equation, in both the full and continuous samples the coefficients become increasingly negative as the tax rate rises, although only the 70 percent tax rate is statistically significant.

In panel C, the various guarantees and tax rates are combined to test the effects of the eight experimental plans, and once again, there is little support for the static theory in either the full or continuous samples. Contrary to theoretical predictions and regardless of sample or dependent variable, two of the three strongest disincentive effects occur for the 50 percent guarantee-50 percent tax rate and the 75 percent guarantee-70 percent tax rate plans, the two plans which pay the lowest level of benefits and which, accordingly, should have the smallest negative effects. Conversely, the smallest negative effect (in the full sample equations, a positive effect) is displayed by the most generous treatment, the 125/50 plan. This anomaly is repeated in the pattern of statistical significance: in the equations for Pct. Emp., both the 75/70 and the 100/50, the latter one of the highest paying plans, are significant. In the equations for Pct. L.F. only the low paying 75/70 plan is significant. In summary, the effects of the experimental treatments are anomalous in terms of both the static and dynamic theories and are, at this point, substantively uninterpretable. Consequently, the eight experimental plans will be collapsed to an experimental-control dichotomy in the remaining analysis.

We turn now to the analysis of the model for labor force participation outlined earlier. We shall proceed in two steps: First, we shall present the coefficients for the additive, linear effects of the independent variables. Subsequently, we shall add the hypothesized interaction

terms to these equations and assess the full set of theoretical predictions. Table 5 gives the first set of results for the full and continuous samples.

Column 1 gives the coefficients for Pct. Emp. in the full sample, and it is easily seen that, except for the effects of age, education, family size, job satisfaction, and experimental status, the results are as hypothesized. The coefficients for ethnicity are shown as deviations from nonSpanish whites and, as expected, both black and Spanish-speakers are less often employed than whites, although only the black coefficient is statistically significant. Being a husband-wife family increases the probability of the male head's being employed as does the number of weeks the head worked in the prior year. The two strata variables, it should be recalled, measure total income for the year prior to the experiment adjusted for family size and show that male heads in the lowest stratum are significantly less likely to have worked than those in the two higher strata. Of the three health variables, only that which describes the most restricted activity has a significant negative effect on the probability of working; reducing it by more than half and making it by far the strongest effect in the equation. A negative effect also results from being on welfare at pre-enrollment, although this arises almost by definition since the AFDC-UP programs in both New Jersey and Pennsylvania require that the male head be unemployed at the time of application for benefits. Finally, the effects of experimental site are indicated by the city and unemployment variables. Both effects are significant, suggesting that city differences cannot be totally attributable

to differences in labor market conditions. To the extent that the unemployment rate fully captures labor market differences, it would appear that the city differences may be attributable to differences in the administration of the experiment across sites or other unmeasured differences.

However, inspection of the Pct. Emp. equation for the continuous sample suggests that some of these results stem from attrition. In particular, the significant negative coefficient for the lowest income stratum in the full sample becomes both positive and insignificant, suggesting that those who worked less in this stratum were prone to drop out. The insignificance of the unemployment rate in the continuous sample indicates that those who dropped out were more negatively affected by adverse labor market conditions than those who stayed. On the other hand, those with moderate health problems and who worked less were more likely to stay in the experiment, since the coefficient for moderate health is significantly negative only in the continuous sample.

The pattern of relationships in the Pct. L.F. equation is much the same, except for the effects of age and health. Apparently, male heads under 30 years of age who had no employment problems were more likely to attrite than those who had such problems, since the coefficient for this age group is significantly positive in the full but not in the continuous sample. However, attrition appears to have had no effect on the coefficient for moderate ill health; the probability of being in the labor force is reduced by about 6 percent in both samples by this variable. Note also that the health variables appear to reduce the probability of being in

the labor force more than the probability of working, suggesting that health may more directly influence the decision to join the labor force than the probability of finding a job once that decision has been made.

However, the validity of these interpretations is contingent on the evaluation of the results of the full model with the hypothesized interactions. The coefficients for this model are given in Table 6. While initial inspection of these coefficients appears to indicate major changes in the sizes of the coefficients, closer examination reveals that the changes are due to the instability of the interactions. Comparison of the coefficients of determination in Table 6 with those in Table 5 shows that they are essentially unchanged, indicating that the interactions add no explanatory power to the linear, additive model. This impression is confirmed by F tests for the statistical significance of the increases to the explained variance produced by the addition of the interactions to the model: none showed statistically significant gains. Re-estimation of the model with only the largest interactions also failed to produce statistically significant increases to the explained variance. Thus it appears that a simple linear, additive model provides an adequate description of the results.

Discussion

As was expected, these results offer little basis for choosing between the static and dynamic theories of labor supply under a negative income tax. But it should be noted that the data do not display any

disincentive--the result predicted by the static theory and an outcome consistent with the dynamic theory. There are several possible explanations for this ambiguity, some of which are related to the experiment qua experiment and some of which are related to the nature of labor force activity among male heads of households. First, since the data represent only the first 18 months of the experiment, it is possible that more definite negative (or positive) effects may appear later as the experimental families become more accustomed to the receipt of benefits.[11] No a priori basis exists for estimating the lag between the initiation of benefits and the appearance of possible effects, consequently the present failure to find effects may only indicate that insufficient time has elapsed for them to become measurable. Indeed, the experiment may not last long enough for effects to appear. Second, it is possible that the effects may be too small to be noticeable in terms of our measures. However, attempts to measure changes in earnings or hours worked have fared little better (Watts, 1971; Elesh et al., 1971). Third, it is possible that, in the aggregate, the positive effects attributable to investment may roughly equal the negative effects due to the income and substitution effects, creating an ambiguous net effect. Fourth, it is possible, although unlikely, that the experimental benefits, which average $1,100 per family per year, are insufficient to generate a response.

On a more general level, the explanation for our findings may be in the nature of labor force activity among male heads of households. First, to the extent that the male head's labor force activity is a familial decision and the wife's or other earners' employment is less highly valued,

it may be decided that it is more useful to substitute the benefits for the wife's or other earners' earnings than to substitute them for the male head's. Thus there may be a disincentive in terms of the family's labor supply but not in terms of the male head's. Watts (1971) and Elesh et al. (1971b) present some evidence to support this thesis; however, Watts (1971) further shows that the decrease in the wife's and other earners' labor force participation is not enough to decrease family earnings, partly because there is some tendency for the wage rates of experimental male heads to rise relative to the wage rates for male heads in the control group. Second, men may define work as so central to their identities that they do not countenance the thought of not working. The fact that, in the full and continuous samples, the mean percentages of quarters in labor force were 91 and 94, respectively, may provide some support for this view. If this explanation is correct, then studies of the labor supply of able-bodied prime age, male household heads among the urban poor--such as the experiment, given the control for health--may be primarily trying to explain the labor force activity of outliers or deviants. That is, the characteristics of such male heads other than age, health, family status, and residential location may be more pertinent to the question of the return male heads of households receive for work than for whether they will work.

On balance, taking together our results and the above possible explanations for them, it would appear that the static theory is the more threatened by the lack of a clear disincentive in the data. Except for the question of the lag between experimental payments and response, it offers no real explanation for the failure to find a disincentive. If indeed, the

static theory of labor force participation is the more threatened by these results, then a related implication of these findings is that many estimates of the cost of a national negative tax program may be inflated. Insofar as these cost estimates are based on nonexperimental data analyzed within the framework of the static theory with its predicted disincentive effect, then they build into their calculations an increase in the size of the transfer payments necessitated by a decline in work effort and concommitantly in earnings. If, as the results reported here suggest, the static theory is not the most appropriate model for analyzing this labor force behavior, then the cost estimates based on this model will err in the direction of overestimation.

With regard to the general model of labor supply, the results suggest that it is overly complex for the behaviors under study. Particularly notable is the lack of statistical significance for the interactions; some terms even had signs contrary to theoretical expectations. Moreover, several of the additive variables were also without predictive ability. Despite their prominence in the literature on labor force participation, age, education, family size, and job satisfaction were all without effect, and we have no evidence that their lack of effect is attributable to truncated distributions in these variables.

The remaining variables characterize the male heads with the highest probabilities of labor force activity as being healthy heads of intact, white families living in Jersey City who have histories of stable labor force participation and who were not on welfare at pre-enrollment. The male heads with the lowest probabilities of labor force activity are unhealthy heads of split, black families living in Trenton who have unstable labor force histories and who were on welfare at the experiment's start.

Conclusion

This paper has presented two different theories to account for the effects of a negative income tax on labor supply, and some preliminary results on the effects of 18 months experimental payments on the probabilities of employment and labor force participation for male heads of household. The results thus far indicate that payments have not changed these probabilities. Several possible explanations for these results are offered.

FOOTNOTES

[1]Alternatively, one could use the sum of hours worked per week over some extended time period, say, a year or the duration of the experiment.

[2]We continue to interview families who go on welfare although they receive no experimental payments; and they may return to the experiment at any time.

[3]Because our focus is on the investment, we ignore here the role of motivation. We shall return to that issue later.

[4]We are indebted to M. J. Lefcowitz for this observation.

[5]We recognize that families may not see their expenditure on food as an investment, but the effects of such investment are quite independent of the reasons for which they were made.

[6]The labor market of the poor is typically characterized by high rates of unemployment with the available jobs offering low pay, requiring little or no skill, having few prospects for advancement, and little security. Firms employing the poor are often small and economically marginal, they use labor intensively but with little differentiation. Their low wages and lack of advancement opportunities generally lead to high turnover. Under these conditions few firms could hope to recover their costs from on-the-job training programs, and consequently one would expect few of them to exist. Thus on-the-job training is probably the least likely investment effect of a negative tax. The relative probabilities of the remaining types of investment are less clear and in terms of the nature of the labor market depend upon its tightness, size, diversity, and the extent to which it discriminates against particular population subgroups. Other things equal, one would expect investment in off-the-job training, general education, and migration to be higher in loose labor markets since the gap between the existing wage rates and the wages potentially obtainable as the result of such investment will be higher. In contrast, job search should increase with the tightness of the labor market since the cost of foregone earnings will decrease. The probability of investment in off-the-job training and education will increase with the size and diversity of the labor market and lack of discrimination, since both the training and education and opportunities to employ them will be more available. Conversely, the probability of migration will increase

as the size and diversity of the labor market decreases and the extent of discrimination increases. Investments in health should also increase with the size and diversity of the labor market because although investment may reduce a health problem, the latter may continue to restrict occupational choice.

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

[8]The criteria for selection of the sample analyzed in this paper differ somewhat from the criteria used by Watts (1971) in his preliminary analysis. The primary distinction between Watts's sample and the one used here relates to the treatment of split families in which interviews exist for both husband and wife who are living apart. In Watts's sample the decision on inclusion in the sample was based on the residence of the children. In other words, if the husband and wife split and the children went to the wife, her interview was used and the husband's was dropped from the sample. For the purposes of examining head's labor force response in this paper, the decision was made to retain the husband's interview regardless of the residence of the children. A second distinction between this sample and that of Watts relates to the treatment of families which attrited from the sample during the first year. Most of Watts's analysis was based on "continuous families," which he defines as families that completed a full year with no more than one missed interview and were present for the fourth quarterly (the last in the first year) interview. In an attempt to assess the effects of attrition, we shall present results both for the continuous sample and the sample of all families for whom quarterly data are available.

[9]More sophisticated techniques for the adjustment of bias are currently being developed by David Elesh and Glen Cain and Seymour Spilerman.

[10]The operational definitions of the variables were as follows: Head's age under 30 = 1 if the male head is age 30 or less, 0 otherwise; Head's age over 40 = 1 if the male head is age 41 or more, 0 otherwise; Black = 1 if the male head is black, 0 otherwise; Spanish-speaking = 1 if the male head is Spanish-speaking, 0 otherwise; Head's education = years of school completed; Family size = Number in family; Pct. husband-wife family = percent of all quarters for which there is an interview that the family had both spouses present; Head's weeks worked last year = number of weeks worked in year prior to the experiment; Income stratum 1 = 1 if family is in the lowest of the three income strata used in the allocation of sample families to experimental treatments, 0 otherwise; Income stratum 3 = 1 if the family is in the highest income stratum, 0 otherwise (the three income strata are classifications of total family income for the year preceding the experiment as a percentage of the

Social Security Administration's poverty levels, adjusted for family size). Stratum 1 = family incomes up to 100 percent of poverty level; Stratum 2 = family incomes between 100 percent and 125 percent of poverty level; Stratum 3 = family incomes between 125 percent and 150 percent of poverty level. Head unhealthy = 1 if the male head has a long-term illness or disability which limits his work but which has never stopped him from working 3 months or longer, 0 otherwise. Head unhealthy 2 = 1 if the male head has a long-term illness or disability which limits his work and in the past has stopped him from working 3 months or longer, 0 otherwise; Head unhealthy 3 = 1 if the male head has a long-term illness or disability which stops him from working now, 0 otherwise (these three health variables were constructed from data in the second quarterly interview); Trenton = 1 if the male head resides in Trenton, 0 otherwise; Paterson-Passaic = 1 if the male head resides in either Paterson or Passaic, 0 otherwise; Scranton = 1 if the male head resides in Scranton, 0 otherwise; On welfare at pre-enrollment = 1 if the male head was on welfare at pre-enrollment, 0 otherwise; job satisfaction = score (0-4) on scale of job satisfaction as defined below; In experiment = 1 if the family is in experimental group, 0 if in control (see text); Average unemployment rate = the average unemployment rate from pre-enrollment to sixth quarter for the city in which the family resides for every quarter for which family was present; Interaction of experimental status and head's age under 30 = 1 if in experimental group and head's age under 30, 0 otherwise; Interaction of experimental status and head's age over 40 = 1 if in experimental group and head's age over 40, 0 otherwise; Interaction of experimental status and black = 1 if in experimental group and black, 0 otherwise; Interaction of experimental group and Spanish-speaking = 1 if in experimental group and Spanish-speaking, 0 otherwise; Interaction of experimental status and family size = family size if in experimental group, 0 otherwise; Interaction of experimental status and head's weeks worked last year = head's weeks worked last year if in experimental group, 0 otherwise; Interaction of experimental status and head unhealthy 2 = 1 if in experimental group and male head has a long-term illness or disability which limits his work and in the past has stopped him from working 3 months or longer, 0 otherwise; Interaction of experimental status and head unhealthy 3 = 1 if in experimental group and male head has a long-term illness or disability which stops him from working now, 0 otherwise; Interaction of experimental status and Trenton = 1 if in experimental group and living in Trenton, 0 otherwise; Interaction of experimental status and Paterson-Passaic = 1 if in experimental group and living in either Paterson or Passaic, 0 otherwise; Interaction of experimental status and Scranton = 1 if in experimental group and living in Scranton, 0 otherwise; Interaction of experimental status and on welfare = 1 if in experimental group and on welfare at pre-enrollment, 0 otherwise; Interaction of experimental status and job satisfaction = score on job satisfaction scale if in experimental group, 0 otherwise; Interaction of job satisfaction and black = score on job satisfaction scale if black, 0 otherwise;

Interaction of job satisfaction and Spanish-speaking = score on job satisfaction scale if Spanish-speaking, 0 otherwise; Interaction of job satisfaction and income stratum and income stratum 1 = score on job satisfaction scale if in income stratum 1, 0 otherwise; Interaction of job satisfaction and income stratum 3 = score on job satisfaction scale if in income stratum 3, 0 otherwise.

The job satisfaction scale was constructed from the following items: (1) Thinking about your (most recent) job, how satisfied are you with it in general? Would you say you are very satisfied, fairly satisfied, a little dissatisfied, or very dissatisfied? (2) How would you compare your present (last) job to all other jobs you had? The response categories to this second item are: Best, Not as good, About same. The responses were then rated from high to low for each item and then summed. Respondents answering don't know or not answering to both items were assigned the mean value for the scale. Respondents answering don't know or not answering to one item were assigned the same score for both items. The sum of the item scores were then recoded 0-4 based on a high-medium-low trichotomy with 0 scores only for those answering very dissatisfied and job not as good and 4 scores only for those responding very satisfied and best job.

[11]Yet we expected our best results to appear at 18 months, the mid-point of the experiment. The earlier results might quite plausibly have been distorted by the families' adjustment to the experimental payments, while the latter results might reflect the families' adjustment to the experiment's termination.

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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	X	X	
.75 poverty line	X	X	X
1.00 poverty line		X	X
1.25 poverty line		X	

TABLE 2

Labor Force Activity by Quarter, Income Stratum, and Experimental Status, Full and Continuous Samples

	Full Sample			Continuous Sample		
	Experimental	Control	N	Experimental	Control	N
Percent employed						
Pre-enrollment						
Income stratum 1	77.1	87.4	245	78.2	89.7	184
Income stratum 2	92.3	92.5	341	91.3	92.5	275
Income stratum 3	90.7	91.8	408	91.7	95.3	320
First quarter						
Income stratum 1	74.5	80.1	222	76.7	85.3	181
Income stratum 2	88.3	91.2	319	88.9	93.8	273
Income stratum 3	86.9	88.1	389	88.6	90.5	319
Second quarter						
Income stratum 1	81.8	81.4	212	83.5	87.6	182
Income stratum 2	87.4	85.9	308	87.4	89.4	272
Income stratum 3	90.0	88.6	369	92.1	95.2	315
Third quarter						
Income stratum 1	80.8	80.6	202	81.2	85.6	182
Income stratum 2	88.3	89.6	300	88.8	93.8	271
Income stratum 3	87.9	94.5	352	90.9	95.2	313
Fourth quarter						
Income stratum 1	77.5	82.1	195	77.6	87.5	181
Income stratum 2	88.9	89.7	294	88.3	94.0	273
Income stratum 3	84.5	94.3	346	85.2	96.8	316

TABLE 2 (cont.)

	Full Sample			Continuous Sample		
	Experimental	Control	N	Experimental	Control	N
Fifth quarter						
Income stratum 1	80.0	84.3	192	81.4	88.4	181
Income stratum 2	90.5	93.6	288	90.6	91.1	270
Income stratum 3	91.5	93.3	335	91.1	94.8	317
Sixth quarter						
Income stratum 1	84.4	81.4	192	85.1	87.4	184
Income stratum 2	89.0	84.7	281	89.9	95.2	274
Income stratum 3	85.3	95.4	329	86.0	92.7	319
Percent in Labor Force						
Pre-enrollment						
Income stratum 1	88.1	89.8	245	88.5	89.7	184
Income stratum 2	94.8	94.6	341	94.2	92.5	275
Income stratum 3	93.7	94.7	408	95.3	95.3	320
First quarter						
Income stratum 1	81.1	83.6	222	83.7	85.3	181
Income stratum 2	92.5	95.0	319	92.8	93.8	273
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TABLE 2 (cont.)

	Full Sample			Continuous Sample		
	Experimental	Control	N	Experimental	Control	N
Third quarter						
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Income stratum 2	94.3	88.9	281	95.2	89.5	274
Income stratum 3	91.9	95.4	329	92.7	96.0	319

TABLE 3

Number of Quarters Worked and in Labor Force
by Income Stratum and Experimental Status
Continuous Nonwelfare Sample

	Experimental				Control			
	1	2	3	Total	1	2	3	Total
Quarters worked								
0	5.7	2.4	.5	2.2	4.1	1.5	0	1.7
1	1.1	1.9	2.6	2.0	2.1	1.5	0	1.0
2	3.4	1.0	1.5	1.6	1.0	0	.8	.7
3	5.7	1.4	1.5	2.2	3.1	1.5	.8	1.7
4	2.3	4.3	5.7	4.5	5.1	4.5	2.4	3.8
5	9.2	2.9	5.7	5.1	9.3	13.4	7.1	9.3
6	31.0	21.6	19.7	22.5	21.6	16.4	20.5	19.9
7	41.4	64.4	62.7	59.6	53.6	61.2	68.5	61.9
N	87	208	193	488	97	67	127	291
Quarters in labor force								
0	3.4	.5	0	.8	3.1	1.5	0	1.4
1	1.1	1.4	.5	1.0	3.1	1.5	0	1.4
2	3.4	2.4	1.0	2.0	1.0	0	.8	.7
3	1.1	0	2.1	1.0	0	0	.8	.3
4	4.6	2.9	2.1	2.9	3.1	1.5	.8	1.7
5	4.6	1.9	4.7	3.5	8.2	7.5	5.5	6.9
6	27.6	19.7	18.1	20.5	20.6	20.9	18.9	19.9
7	54.0	71.1	71.5	68.2	60.8	67.2	73.2	67.7
N	87	208	193	488	97	67	127	291

TABLE 4

Gross Effects of Experimental Parameters on Labor Force Activity

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
A. Guarantee levels				
Constant	92.75*	94.58*	91.50*	93.19*
	(1.70)	(1.45)	(1.85)	(1.56)
50% guarantee	-5.87*[a]	-2.98	-4.16	-1.52
	(2.59)[b]	(2.22)	(2.96)	(2.50)
75% guarantee	-3.97*	-3.80*	-2.89	-1.79
	(1.88)	(1.60)	(2.05)	(1.73)
100% guarantee	-4.86*	-1.50	-4.49*	-.67
	(2.29)	(1.96)	(2.41)	(2.03)
125% guarantee	1.32	.85	-.79	-.37
	(2.45)	(2.09)	(2.48)	(2.09)
Income stratum 1	-10.07*	-6.75*	-6.13*	-3.88*
	(1.94)	(1.65)	(2.12	(1.79)
Income stratum 3	-1.80	-.31	1.47	1.96
	(1.80)	(1.53)	(1.92)	(1.62)
On welfare at pre-enrollment	-10.60*	-7.67*	-10.66*	-6.97*
	(2.09)	(1.78)	(2.25)	(1.90)
$\bar{R}^2$[c]	.06	.04	.04	.03
B. Tax rates				
Constant	92.34*	94.40	91.26*	93.23*
	(1.67)	(4.66)	(1.81)	(1.53)
30% tax rate	-2.03	-1.74	-1.75	-.30
	(2.39)	(2.04)	(2.61)	(2.00)
50% tax rate	-2.35	-.88	-2.59	-.56
	(1.70)	(1.45)	(1.81)	(1.53)

TABLE 4 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
70% tax rate	-7.06*	-5.82*	-4.77*	-3.44*
	(2.28)	(1.94)	(.244)	(2.06)
Income stratum 1	-9.65*	-6.53*	-5.89*	-3.96*
	(1.90)	(1.62)	(2.06)	(1.74)
Income stratum 3	-1.18	-.02	1.79	1.91
	(1.74)	(1.49)	(1.84)	(1.55)
On welfare at pre-enrollment	-10.54*	-7.77*	-10.49*	-6.94*
	(2.09)	(1.78)	(2.25)	(1.89)
$\bar{R}^2$	.06	.04	.04	.03
C. Guarantee-tax rate combinations				
Constant	93.48*	95.19*	92.05*	93.66*
	(1.73)	(1.48)	(1.90)	(1.61)
50/30[d]	-4.36	-2.36	-2.58	.63
	(3.97)	(3.38)	(4.62)	(3.89)
50/50	-7.30*	-3.80	-5.50	-3.09
	(3.12)	(2.66)	(3.53)	(2.97)
75/30	-.99	-1.46	-1.42	-.60
	(2.77)	(2.36)	(2.97)	(2.50)
75/50	-2.25	-1.75	-1.29	.53
	(2.66)	(2.27)	(2.87)	(2.42)
75/70	-10.14*	-9.62*	-7.40*	-6.87
	(3.02)	(2.58)	(3.34)	(2.81)
100/50	-5.18*	-.65	-6.39*	-.95
	(3.06)	(2.61)	(3.22)	(2.71)
100/70	-4.74	-2.46	-2.89	-.52
	(2.98)	(2.54)	(3.10)	(2.62)

TABLE 4 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
125/50	1.58	1.06	-.60	-.20
	(2.45)	(2.09)	(2.48)	(2.09)
Income stratum 1	-10.74*	-7.32*	-6.62*	-4.29*
	(1.99)	(1.70)	(2.19)	(1.85)
Income stratum 3	-3.09*	1.37	.48	1.09
	(1.87)	(1.60)	(2.03)	(1.71)
On welfare at pre-enrollment	-10.46*	-7.56*	-10.56*	-6.84*
	(2.09)	(1.78)	(2.25)	(1.90)
$\bar{R}^2$	.06	.04	.04	.03
N	1166	1166	909	909

* Statistically significant at the .05 level
[a] Raw regression coefficients
[b] Standard errors in parentheses
[c] $\bar{R}^2$ corrected for degrees of freedom
[d] Guarantee level/tax rate

TABLE 5

A Linear, Additive Model of Labor Force Activity

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Constant	65.35*	77.98*	65.08*	77.28*
	(5.72)	(4.66)	(10.76)	(8.31)
Head's age under 30	1.29[a]	2.67*	-.83	.90
	(1.57)[b]	(1.28)	(1.62)	(1.25)
Head's age over 40	.06	-.58	.94	-.06
	(1.55)	(1.26)	(1.51)	(1.17)
Black	-4.70*	-3.34*	-4.95*	-3.15*
	(2.34)	(1.83)	(2.18)	(1.68)
Spanish-speaking	-2.03	-2.49	-2.38	-2.24
	(2.34)	(1.91)	(2.34)	(1.81)
Head's education	.03	.13	.06	.19
	(.24)	(.20)	(.24)	(.18)
Family size	.42	.23	.35	-.01
	(.33)	(.27)	(.34)	(.25)
Pct. husband-wife family	.14*	.08*	.24*	.14*
	(.04)	(.03)	(.05)	(.03)
Head's weeks worked last year	.40*	.28*	.37*	.25*
	(.05)	(.04)	(.05)	(.04)
Income stratum 1	-2.49*	-.74	.53	1.51
	(1.70)	(1.39)	(1.72)	(1.33)
Income stratum 3	-.75	.10	.99	1.05
	(1.49)	(1.21)	(1.47)	(1.14)
Head unhealthy 1	.62	.01	-.83	-1.31
	(2.64)	(2.15)	(2.50)	(1.93)
Head unhealthy 2	-4.07	-6.04*	-5.08*	-5.91*
	(2.92)	(2.37)	(3.45)	(2.12)

TABLE 5 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Head unhealthy 3	-53.23*	-55.60*	-59.98*	-63.56*
	(3.34)	(2.72)	(3.45)	(2.67)
Trenton	-10.01*	-7.09*	-14.66*	-8.42*
	(2.29)	(1.87)	(4.53)	(3.51)
Paterson-Passaic	-5.78*	-2.98*	-6.70*	-3.40*
	(1.62)	(1.32)	(2.52)	(1.95)
Scranton	-4.00*	-3.97*	-7.49*	-5.47*
	(2.46)	(2.01)	(3.48)	(2.69)
On welfare at pre-enrollment	-7.24*	-4.28*	-7.19*	-3.21*
	(1.82)	(1.48)	(1.84)	(1.42)
Job satisfaction	.03	-.13	.59	.33
	(.57)	(.46)	(.57)	(.44)
In experiment	-1.07	-.57	-1.08	.28
	(1.31)	(1.07)	(1.33)	(1.03)
Average unemployment rate	-.04*	-.03*	-.19	-.11
	(.02)	(.01)	(.12)	(.10)
$\bar{R}^2$ [c]	.33	.38	.40	.49
N	1166	1166	909	909

* Statistically significant at the .05 level on the appropriate one or two tailed test

[a] Raw regression coefficients

[b] Standard errors in parentheses

[c] $\bar{R}^2$ corrected for degrees of freedom

TABLE 6

An Interactive Model of Labor Force Activity

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Constant	73.41*	83.38*	67.87*	78.06*
	(7.53)	(6.15)	(12.02)	(9.33)
Head's age less than 30	-.16[a]	.78	-.60	.47
	(2.50)[b]	(2.05)	(2.60)	(2.02)
Head's age over 41	-2.71	-3.14	-.15	-.75
	(2.47)	(2.01)	(2.45)	(1.90)
Black	-16.28*	-10.22*	-16.10*	-7.38*
	(5.67)	(4.64)	(5.80)	(4.50)
Spanish-speaking	-8.42	-7.31	-11.95*	-8.61*
	(5.58)	(4.56)	(6.18)	(4.80)
Head's education	.06	.14	.08	.19
	(.24)	(.20)	(.24)	(.19)
Family size	-.19	.13	-.04	.07
	(.53)	(.43)	(.56)	(.43)
Pct. husband-wife family	.14*	.08*	.23*	.13*
	(.04)	(.03)	(.05)	(.04)
Head's weeks worked last year	.38*	.25*	.41*	.27*
	(.08)	(.07)	(.08)	(.07)
Income stratum 1	7.47	5.62	11.24*	7.75*
	(4.78)	(3.91)	(4.80)	(3.72)
Income stratum 3	4.09	4.43	5.13	5.32
	(4.33)	(3.54)	(4.36)	(3.38)
Head unhealthy 1	1.23	.50	-.32	-.96
	(2.66)	(2.17)	(2.52)	(1.95
Head unhealthy 2	-8.13*	-10.61*	-6.56	-7.65*
	(4.55)	(3.72)	(4.41)	(3.42)

TABLE 6 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Head unhealthy 3	-54.27*	-54.34*	-63.84*	-65.15*
	(5.49)	(4.49)	(5.85)	(4.54)
Trenton	-5.84	-5.62	-6.83	-5.13
	(3.92)	(3.21)	(6.09)	(4.73)
Paterson-Passaic	-3.30	-3.10	-2.69	-3.45
	(2.69)	(2.20)	(3.53)	(2.74)
Scranton	-4.18	-3.58	-7.19	-3.86
	(3.74)	(3.05)	(4.66)	(3.62)
On welfare at pre-enrollment	-7.47*	-5.53*	-8.46*	-4.00*
	(2.78)	(2.28)	(2.78)	(2.16)
Job satisfaction	-1.36	-1.20	-.73	-.82
	(1.02)	(.83)	(1.06)	(.82)
In experiment	-14.94*	-9.37	-5.12	.06
	(7.68)	(6.27)	(7.80)	(6.06)
Average unemployment rate	-.04*	-.03*	-.17	-.11
	(.02)	(.01)	(.12)	(.10)
Interactions with experimental status				
Head's age under 30	1.77	2.84	-.42	.92
	(3.21)	(2.62)	(3.31)	(2.57)
Head's age over 40	4.44	4.10	1.93	1.26
	(3.12)	(2.55)	(3.07)	(2.38)
Black	3.32	1.38	-.30	-2.96
	(4.61)	(3.77)	(4.68)	(3.63)
Spanish-speaking	-.35	-2.51	-1.26	-4.40
	(4.71)	(3.85)	(4.85)	(3.77)

TABLE 6 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Family size	.94	.14	.64	-.03
	(.66)	(.54)	(.68)	(.53)
Head's weeks worked	-.00	.02	-.09	-.06
	(.10)	(.08)	(.11)	(.08)
Head unhealthy 2	7.03	7.32	3.08	2.83
	(5.95)	(4.86)	(5.65)	(4.39)
Head unhealthy 3	.16	-3.46	3.32	.20
	(7.05)	(5.76)	(7.41)	(5.75)
Trenton	-5.87	-1.77	-10.05*	-4.46
	(4.76)	(3.89)	(5.39)	(4.19)
Paterson-Passaic	-4.13	.03	-5.74	-.22
	(3.37)	(2.75)	(3.58)	(2.78)
Scranton	.58	-.02	-.29	-2.22
	(4.93)	(4.03)	(4.88)	(3.78)
On welfare	.31	2.11	2.37	1.34
	(3.66)	(2.99)	(3.69)	(2.86)
Job satisfaction	2.70*	2.01*	2.19*	1.95*
	(1.17)	(9.56)	(1.20)	(.93)
Interactions with job satisfaction				
Black	10.15*	6.49	11.76*	6.55*
	(4.97)	(4.06)	(4.86)	(3.77)
Spanish-speaking	7.46	7.00*	11.04*	9.97*
	(4.76)	(3.89)	(5.26)	(4.08)
Income stratum 1	-10.57*	-7.07	-11.28*	-6.85*
	(4.92)	(4.02)	(4.95)	(3.84)

TABLE 6 (cont.)

	Full Sample		Continuous Sample	
	Pct. Emp.	Pct. L.F.	Pct. Emp.	Pct. L.F.
Income stratum 3	-5.04	-4.71	-4.36	-4.55
	(4.40)	(3.59)	(4.40)	(3.41)
$\bar{R}^2$ [c]	.33	.38	.41	.49
N	1166	1166	909	909

* Statistically significant at the .05 level on the appropriate one or two tailed test

[a] Raw regression coefficients

[b] Standard error in parentheses

[c] R^2 corrected for degrees of freedom