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Social Programs

by

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The overall social and transfer costs of government welfare programs are determined by both the size of the benefits received by program participants and by the number of participants. Although this has been recognized at least since Pigou [15], it is remarkable that economists including Pigou have found it so difficult to incorporate a complete and correct analysis of the determinants of program participation into their calculations of the costs of various welfare programs.^{1/} This is all the more surprising as the advent of social experiments to measure the labor supply responses of a negative

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^{1/} Pigou [15] writes: "If, for example, it is understood that everybody's income will, at need, be brought up by state aid to, say, £3 a week, it will, generally and roughly, be to the interest of everybody capable of earning by work any sum less than £3 a week to be idle and earn nothing. This must damage the national dividend. How much it damages it will, of course, depend on how large the sum fixed on as a minimum is, and how many people in the country would normally earn by work less than that sum." Pigou's conclusion that only the behavior of workers earning less than the state income minimum would be affected by a welfare program is correct only if there are no substitution effects in labor supply functions, and otherwise underestimates program participation.

income tax has made such calculations into a big business.^{2/}

The continuing confusion arises from the fact that there are three related, but conceptually separate issues involved in the determination of program participation. On the one hand, incomes differ across families even in the absence of labor supply responses. It follows that more generous income-tested welfare programs will have more participants even if labor supply is exogenous. A correlation between the generosity of a welfare program and the number of participants in it cannot, by itself, establish anything more about economic incentives than that more consumption is preferred to less.^{3/} On the other hand, with convex indifference curves between non-market time (or leisure) and consumption goods some workers will find it to their advantage to reduce their incomes so as to become eligible for the receipt of welfare benefits. Not surprisingly, the key to identifying the number of workers who engage in this behavior is to isolate the effects on program participation of variations in welfare program tax rates that are independent of program generosity. Without this independent variation the economic incentive effects in program participation cannot be identified. Finally, estimates of the actual participation in existing U.S. welfare programs often indicate that many families who are eligible do not participate in them. This raises the possibility that the incidence of information, reporting, or other unobserved nonpecuniary costs are a significant additional determinant of actual program participation. The key

^{2/} The most recent simulation estimates of participation in a universal negative income tax program by Keeley, et. al. [13] use the results of the Seattle/Denver Income Maintenance Experiment as data. These estimates are based on the same assumption as Pigou's regarding program participation even though significant substitution effects in labor supply functions were estimated as part of earlier studies with the same data.

^{3/} An early example where the contrary is claimed is the analysis of general assistance payments by Brehm and Saving [3], while a more recent example is Cogan's [6] analysis of the New Jersey negative income tax experiment.

to identifying the presence of these costs is the ability to obtain an estimate of what program participation would be if these costs did not exist. In a negative income tax experiment the natural place to obtain this estimate is from the data on an independently selected control group.

The purpose of this paper is to bring these three issues together in order to set out the determinants of participation in an income-tested social program in such a way that all three issues may be explored empirically. The data used for this purpose are probably the best that will be available for some time and come from the Seattle and Denver Income Maintenance Experiments, the largest of the experiments thus far undertaken. The empirical strategy is to construct an econometric framework that treats the cross-sectional heterogeneity of incomes as an inevitable determinant of some correlation between program generosity and program participation, but that allows the data to confirm the further presence of both economic incentives and unobserved nonpecuniary participation costs. This strategy lead to a framework that differs considerably from the existing tightly structured models of labor supply that lead to joint predictions of labor supply responses and program participation.^{4/} First, these models typically leave no room for testing for the presence of unobserved nonpecuniary participation costs, which, in view of the low participation rates in some actual government programs, seems worth attempting. Second, the condition on worker preferences that rules out the importance of economic incentives in program participation is that substitution effects, but not necessarily income effects, should be negligible in labor supply functions.

^{4/} Hall [10] suggested such a model early on, and Burtless and Hausman [5] present a fully worked out case.

Most of the available models of labor supply can accommodate this restriction on substitution effects, however, only by putting empirically unreasonable restrictions onto the income effects in such models.^{5/} The econometric framework proposed here is set out with an eye to remedying both of these difficulties by providing a convenient and tractable scheme for organizing the relevant data that nests all of the relevant hypotheses to be tested.

The first section of the paper is expository and sets out the basic issues as clearly as possible. The natural focus is on the characteristics of families that make them eligible for benefits without changing their labor supply and how to distinguish these families from those that do change their behavior. Inevitably, the elementary empirical analysis it contains is instructive, but too simple. The second section of the paper contains a correct, but nevertheless easily implemented econometric framework based on a convenient specification of the classical model of labor supply. The empirical results are contained in the third section, and the conclusion contains a discussion of the further research that these results suggest may be useful.

I.

Elementary Determinants of Program Participation

A. Eligibility for Experimental Families

In a negative income tax program families receive an income guarantee

^{5/} Negligible substitution effects are consistent with linear labor supply functions or linear labor earnings functions (the case consistent with a Stone-Geary utility function) only if income effects are negligible also. This may be verified by writing the substitution effects in these models as functions of the parameters in the labor supply function and observing the conditions on these parameters consistent with zero values for the substitution effect.

of G dollars, face a tax rate of t , and receive a subsidy (D) of $D = G - ty$ dollars so long as their income (y) is less than G/t , the so-called "breakeven income." Families with incomes below the breakeven are said to be eligible for program participation, while families with higher incomes are not eligible.

Suppose at the outset that families do not control their incomes, but that incomes differ among families. Suppose also that there are no unobserved nonpecuniary costs of program participation such as welfare "stigma." In this situation families whose incomes fall below the breakeven income would then choose to receive a subsidy, while other families would be ineligible for the program. The only economic behavior involved in this prediction is the trivial assumption that more consumption is preferred to less.

To make things concrete suppose that the logarithm of income is normal with mean μ and standard deviation σ . The fraction of families eligible for benefits (P) is then simply

$$(1) \quad P = N[(\ln G/t - \mu)/\sigma] ,$$

where N indicates the value of the cumulative standardized normal distribution. Now suppose several groups are randomly selected from the pool whose log income is normally distributed and that these groups are offered negative income tax plans with varying breakevens, which is a stylized description of the Seattle and Denver Income Maintenance Experiments. Equation (1) will then describe a series of points relating the participation fraction (P) and the log of the breakeven of the offered plan. There are two observations to make about this relationship. First, higher income guarantees (G) and lower tax

rates (t) will be associated with greater participation. This relationship is purely mechanical in the sense that it merely reflects increased eligibility for program benefits as the breakeven of the plan increases and the program becomes more generous. Moreover, in this simple setup the elasticity of program participation across groups with respect to the program guarantee will be equal but opposite in sign to the elasticity of program participation with respect to the tax rate. A regression of program participation on G and t does not, therefore, establish anything more than that families offered programs with high breakevens are more likely to be eligible for program benefits even if they do not change their behavior.^{2/}

Second, a plot of the participation fraction P against the log of the breakeven will reveal the parameters of the log income distribution and can be used both to estimate those parameters and to test the normality assumption. Table 1 contains data from the Seattle/Denver Experiments to illustrate this point. Column (2) lists the fraction of all families initially offered a negative income tax program who received more than a nominal payment during any quarter of the second year of the experiment.^{3/} Program attriters are included in the data as non-participants, which is an enormous advantage for this analysis. After all, attrition is logically equivalent to non-participation and is known to be highly correlated with the program

^{2/} This point has not always been appreciated. Cogan [6], for example, computes such regressions from the New Jersey negative income tax experiment data and appears to attribute behavioral significance to them. Brehm and Saving [3] do the same with data on General Assistance payments.

^{3/} All experimental families received at least \$60 per quarter independently of their incomes as compensation for various administrative obligations. The number of families in the numerator of the participation fraction is the number of families receiving more than this nominal payment.

Table 1

Families Receiving Payments in the Seattle/Denver Negative
Income Tax Experiment

Interval for the Logarithm of the Breakeven Earnings (Experimentals) or Actual Earnings (Controls)	(1) Actual Cumulative Proportion ($\tilde{P}$ for Controls)	(2) Participation Proportion ($\tilde{P}$ for Experimentals)	(3) Adjusted Cumulative Proportion (P for Controls)	(4) Adjusted Participation Proportion (P for Experimentals)
≤ 8.832	.315	.328	.28	.29
≤ 8.925	.383	.319	.34	.29
≤ 8.982	.433	.521	.39	.45
≤ 9.085	.527	.558	.46	.48
≤ 9.170	.609	.673	.53	.57
≤ 9.324	.774	.734	.66	.64

breakeven to which a family is assigned.^{4/} As can be seen from Table 1, the participation proportion increases with the breakeven of the program, as expected.^{5/}

Unfortunately, because of the setup of the Seattle/Denver experiments the participation proportions in Table 1 are not estimates of the cumulative values of the normal distribution given by equation (1). In these experiments, as in others, families were initially screened on the basis of their pre-experimental incomes before being assigned to an experimental program. To account for the truncation of the income distribution on the basis of pre-experimental incomes I have also reported the adjusted participation proportion in column (4) of Table 1.^{6/} The actual participation proportions $\tilde{P}$ observed in the sample are upward biased estimates of the participation proportions P that would be observed in a national negative income tax program because the

^{4/} See Pencavel and West [14] for an analysis of this issue in the Seattle and Denver experimental data.

^{5/} There are several definitions of program breakeven that might be used for this analysis, but I have simply used the ratio of the nominal program income guarantee to the nominal tax rate as the breakeven throughout.

^{6/} Write the joint cumulative distribution function of experimental and pre-experimental log incomes as $N(\tilde{y}_2, \tilde{y}_1)$. The right hand side of equation (1) is then $N(\ln G/t, \infty)$, the marginal cumulative distribution of experimental income evaluated at the log of the breakeven. Taking the log of the truncation point to be T , the unadjusted participation fractions in Table 1 are estimates of $\tilde{P} = N(\ln G/t, T)/N(\infty, T)$. These may be converted to estimates of the adjusted participation fraction $P = N(\ln G/t, \infty) = N(\infty, T)\tilde{P} + [N(\ln G/t, \infty) - N(\ln G/t, T)]$, if estimates of $N(\infty, T)$ and $N(\ln G/t, \infty) - N(\ln G/t, T)$ are available. To calculate these it is necessary to estimate $\mu_2, \mu_1, \sigma_2, \sigma_1$ and ρ , the means and standard deviations of the untruncated log incomes in the experimental and pre-experimental years and the correlation (ρ) between them. The estimates of these parameters from the control group sample using a straightforward maximum likelihood scheme are $\hat{\mu}_2 = 8.993, \hat{\mu}_1 = 8.980, \hat{\sigma}_2 = .840, \hat{\sigma}_1 = .925, \hat{\rho} = .413$. These lead to an estimate of .72 for $N(\infty, T)$ and estimates of $N(\ln G/t, \infty) - N(\ln G/t, T)$ for the six breakeven incomes of Table 1 of .057, .063, .077, .081, .090, and .107.

FIGURE 1

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experimental sample overrepresents families with low incomes in the pre-experimental year and incomes are positively correlated in subsequent years.

The dashed line in Figure 1 is a plot on normal probability paper of the adjusted participation fractions against the log of the breakeven for the six programs listed in Table 1. Although there is clearly considerable variation, these points do roughly coincide with a straight line and are not strong evidence against the normality assumption.

B. Control Families

The design of the Seattle/Denver negative income tax experiments also includes provision for a control group of randomly selected families. A natural question arises as to what use the data from this group are for the analysis of program participation. The answer is that these data provide independent evidence on both the form of the cumulative distribution function of log income and the values of the parameters of that distribution.

To continue, suppose that the log incomes of control families are also normal with mean μ and standard deviation σ . The fraction of control families with log incomes below a value R is then

$$(2) \quad P = N[(R-\mu)/\sigma] \quad .$$

Choosing $R = \ln G/t$ gives a relationship between the values of the empirical cumulative distribution function of log income and the values of R among the control families that should be the same as the relationship between the participation fraction and the program breakeven among the experimental families.

As with the experimental families the control families are selected in the Seattle/Denver experiments so as to overrepresent low income families and this requires an adjustment of the observed cumulative proportions $\tilde{P}$.^{7/} These adjusted proportions P in Table 1 are also plotted in Figure 1. As can be seen from Figure 1, the estimated points of the cumulative distribution of log incomes for the control families do closely coincide with a straight line. This suggests that the normality assumption is a reasonable one. Moreover, the position of this line is close, but by no means identical, to the position of the similar relationship for experimental families. This suggests that the data for both control and experimental families are useful for the analysis of program participation, and that systematic labor supply behavior or the presence of nonpecuniary participation costs may account, in part, for the difference between them.

C. Labor Supply Behavior and Participation

The analysis to this point has emphasized the mechanical determinants of program participation by treating the family's income as exogenous. The spirit of the analysis of labor supply, however, is that by choosing their hours of work family members may manipulate their income so as to become eligible for negative income tax payments. To determine how many families will engage in this behavior it is necessary to recognize that a negative income tax offers both a harmful and a beneficial change in the opportunity

^{7/} The adjustment is identical for the control families to that made for the experimental families.

set a family faces. The harmful change is the decrease in the wage rate from w to $(1-t)w$ that a family member faces. The beneficial change is the increase in the guaranteed (unearned) income level of G that the family now has. The family that is offered the opportunity to participate in a negative income tax program presumably will do so if the harmful effect of participating is outweighed by the beneficial effect.

The simplest way to analyze this choice is to consider the minimum unearned income level required by a worker to reach a given level of utility (or indifference curve). The (excess) expenditure function $E(w,v)$, which gives the minimum unearned income required by a worker with wage rate w to reach the utility level v , is the tool for this purpose.^{8/} If the family participates in the program it needs unearned income of $E[(1-t)w,v]$ to reach the utility level v , while it needs unearned income of only $E(w,v)$ to reach the same utility level if it remains a nonparticipant. On the other hand, as a participant the family obtains an increase in its unearned income of G dollars. Clearly, the family will choose to participate in the program if $E[(1-t)w,v] - E(w,v) < G$, that is, if the extra unearned income needed to compensate the family for the damaging effects of the tax rate is less than the extra unearned income actually transferred to the family as a result of the program.

A natural procedure is to approximate the difference $E[(1-t)w,v] - E(w,v)$ by a second-order Taylor series around the nonparticipant equilibrium. Using

^{8/} The excess expenditure function is used in Ashenfelter [1], [2] for this purpose. A useful summary of this modern approach to the study of labor supply is contained in Deaton and Muellbauer [7], chapt. 4 and 11.

the properties of the excess-expenditure function that $\partial E/\partial w = -h$, where h is nonparticipant hours of work, and that $\partial^2 E/\partial w^2 = -\partial h/\partial w = -s$, where $s > 0$ is the compensated or utility-constant derivative of the labor supply function, we then have

$$\begin{aligned} (3) \quad E[(1-t)w, v] - E(w, v) &\approx \partial E/\partial w (-tw) + 1/2 (\partial^2 E/\partial w^2)(-tw)^2 \\ &= htw - 1/2 s(tw)^2. \end{aligned}$$

A family will choose to participate and receive a subsidy if

$$(4) \quad D + 1/2 s(tw)^2 > 0. \quad \underline{9/}$$

Since $s > 0$, it follows immediately that any family eligible for a positive subsidy on the basis of its nonparticipant hours decision ($D = G - twh > 0$) will participate, but also that some families above the breakeven will too. Even though these latter families will have lower total incomes as a result of their choice to participate, their decline in consumption of goods is more than compensated by the increased non-market time they consume.

The earned income level below which a family will choose to participate may be called the opting-in income level and is obtained by converting (4) to an equality and solving for

$$(5) \quad wh^0 = (G/t)(1 - .5te)^{-1},$$

^{9/} This criterion for participation has been pointed out elsewhere. A confusing discussion is in Greenberg and Kusters [8], a clear one is in Rea [16], but Hicks' [11] is the earliest. I am indebted to Robert Moffitt for these references.

where $e = s(w/h)$ is the compensated elasticity of labor supply.^{10/} Equation (5) demonstrates that the choice of program participation varies from what it would be with exogenous incomes to the extent that indifference curves between leisure and goods consumption are convex. If $e = 0$ these indifference curves are right angles and participation is determined as if income were exogenously determined. The larger is e the greater is the opting-in income level, the greater is participation, and, with a constant breakeven, the more sensitive is participation to variations in the tax rate.

It is a straightforward matter to use the criterion of equation (5) as the basis for a simple econometric model of participation. First, for reasonable values of e and t the logarithm of (5) is

$$(6) \quad \ln wh^0 \approx \ln G/t + .5et.$$

Second, assume that families choose an h based on the w and other stochastic circumstances they face in a given period. However this choice is made, suppose as before that $\ln(wh)$ is normal with mean μ and standard deviation σ . If the earned income that would be received in the absence of program participation, $\ln wh$, turns out to be less than $\ln wh^0$ in any period the family

^{10/} There are two further points to make about equation (5). First, it gives the value of earned income below which it is in the interests of a family to participate. G must therefore be measured as the change in unearned income associated with program participation and this may not coincide with the stated program guarantee. Second, if both family members work, (5) continues to be valid, but $e = \theta e_{11} + 2\theta e_{12} + (1-\theta)e_{22}$, where e_{ij} is the compensated elasticity of the i^{th} family member's labor supply with respect to the j^{th} family member's wage and θ is the share of the first family member's earnings in total family earnings. See Ashenfelter [1, p. 135].

will choose program participation. The fraction of families offered a program with breakeven G/t and tax rate t that will choose participation is thus

$$(7) \quad P = N[(\ln wh^0 - \mu)/\sigma] \\ = N[(\ln G/t + .5et - \mu)/\sigma] .$$

Equation (7) neatly separates the mechanical from the behavioral aspects of program participation. From equation (1) the mechanical component of participation is $N[(\ln G/t - \mu)/\sigma]$. It is the participation that would be expected if incomes were exogenously determined or if $e = 0$. The participation induced by a change in labor supply behavior is thus the difference between (7) and (1): $N[(\ln G/t + .5et - \mu)/\sigma] - N[(\ln G/t - \mu)/\sigma]$.

D. Nonpecuniary Costs of Participation

A natural way to investigate the effect of nonpecuniary participation costs or tastes on program participation is to assume that each family behaves as if these were of a dollar quantity Q . The condition for participation in the program for the family is then that $\ln wh < \ln wh^0 + Q$. If $Q < 0$ the family has a distaste for participation, while if $Q > 0$ the family has a taste for participation. Suppose that these tastes are independently and normally distributed with mean μ^* and standard deviation $\sigma^{*II/}$. The fraction of families offered a program with breakeven G/t and tax rate t that will choose participation is then just the fraction for whom $\ln wh - Q < \ln wh^0$

^{11/} The independence assumption has no quantitative content here since a correlation between Q and $\ln wh$ would merely change the definition of σ in (8) below.

and is simply

$$(8) \quad P = N[\ln wh^0 - (\mu + \mu^*)] / \sqrt{\sigma^2 + \sigma^{*2}} \quad] \\ = N[(\ln G/t + .5 \ln et - (\mu - \mu^*)) / \hat{\sigma}] ,$$

where $\hat{\sigma} \equiv \sqrt{\sigma^2 + \sigma^{*2}}$.

The difference between (7) and (8) is the amount by which program participation differs from what would be the case if there were no nonpecuniary participation costs or tastes. It seems plausible that the parameters of the distribution of these costs or tastes would differ according to the type of program and the manner of its administration, and equation (8) shows just how these differences will affect program participation.

Equation (8) leads naturally to a simple probit estimation scheme using the data on the experimental group. All of the parameters in (8) cannot be estimated from these experimental data alone, however. The value of the data from a randomly selected control group is that these data may be used to estimate μ and σ in order to test the importance of nonpecuniary participation costs in the determination of program participation so that all of the parameters in (8) may be identified. After all, the observed distribution of log earnings for the experimental group no longer provides straightforward information of this type as it is contaminated by any experimental effects that exist.

It is now clear why the analysis demonstrated in Figure 1 is too simple. Equation (8) indicates that the dashed line portraying the fractional participation of the experimental group may differ from the solid line indicating the

cumulative distribution of the log earnings of the control group for two different reasons. On the one hand the level and/or slopes of these lines may differ because of the presence of nonpecuniary program participation costs. On the other hand, in the absence of these costs the dashed line should lie above the solid line so long as $e \neq 0$, and the size of any systematic difference should depend on the tax rate in the negative income tax programs. We turn next to a simple estimation scheme for sorting out these effects.

II.

Econometric Framework

A. Likelihood of the Experimental Sample

Equation (8) gives the probability of observing a participant and leads directly to the likelihood of the experimental sample. Two preliminary issues require some discussion, however. First, as we have observed, the experimental families offered participation in the Seattle and Denver Income Maintenance Experiments were screened on the basis of their pre-experimental incomes. Assuming that the joint distribution of pre-experimental and experimental log earnings is bivariate normal, however, implies that the conditional log earnings distribution during the experimental period is normal with mean linear in pre-experimental log earnings. It follows that equation (8) applies directly with the understanding that μ in that equation is replaced by a linear function of pre-experimental earnings. Thus, the truncation of the sample on the basis of pre-experimental earnings may be handled by simply including an additional regressor and taking some care in the

interpretation of the parameters μ and σ .^{12/} Second, it is natural to suppose that μ and μ^* in equation (8) are not constants, but instead vary across individuals with a (row) vector of variables X_i' . This also causes no problem for the interpretation of equation (8) as $\mu - \mu^*$ may simply be replaced in (8) by putting $\mu = X_i'\beta$ and $\mu^* = X_i'\beta^*$, so that $\mu - \mu^* = X_i'(\beta - \beta^*)$, where β and β^* are vectors of coefficients. It should be understood that because of the pre-experimental screening on income the vector X_i must at a minimum contain the value of pre-experimental income, but it may contain other variables as well. Since a primary concern is to obtain a consistent estimator for the parameters e and $\hat{\sigma}$ in (8), it is most important to include variables in the vector X_i that may be correlated with the assignment of families to the various programs.

It follows that if the first n experimentals participate and the next m do not, the (log) likelihood of the observed sample is

$$(9) \quad \mathcal{L}^1 = \sum_{i=1}^n \ln N_i + \sum_{i=n+1}^m \ln(1-N_i),$$

where $N_i = N[(\ln G/t_i + .5et_i - X_i'(\beta - \beta^*)/\hat{\sigma})]$. This is nothing more than the (log) likelihood function for a simple probit analysis. Indeed, the only difference between this analysis and a simple probit analysis is that here the parameter $\hat{\sigma}$ is identified because of the structure of the model. It follows that estimates of the parameter $1/\hat{\sigma}$ may be taken as the "probit" coefficient on $\ln G/t_i$ and that the coefficients e and $\beta - \beta^*$ are simple

^{12/} Using the notation of footnote 6, $\mu = [\mu_2 - (\sigma_2/\sigma_1)\rho\mu_1] + (\sigma_2/\sigma_1)\rho \ln(wh_1)$, where $\ln(wh_1)$ is the logarithm of earnings in the pre-experimental period, and $\sigma_2^2(1-\rho^2)$.

fuctions of the ratios of these "probit" coefficients. Standard computing routines may thus be used.

An important message of this analysis is that the presence of economic incentives to program participation may be identified from data on an experimental group alone because of variation in the tax rates t_i . On the other hand, the correlates of presence of nonpecuniary program participation costs cannot be identified from experimental data alone. Regressions of program participation on the variables X_i cannot distinguish whether a variable influences participation because it is a determinant of the conditional mean of the earnings distribution or the conditional mean of the distribution of nonpecuniary participation costs.

B. Likelihood of the Control Sample

The value of the control sample is that it provides information on μ and σ , or, equivalently, β and σ , and this information identifies β^* and σ^* as well. The (log) likelihood function for the control sample is equivalent to the conventional setup for a regression function and is

$$(10) \quad g^2 = \sum \ln n_i / \sigma,$$

where $n_i = n[(\ln wh_i - X_i' \beta) / \sigma]$ and n indicates the unit normal density function. The regression estimates of β and σ that result from the maximization of (10) taken with the probit estimates of $\beta - \beta^*$ and $\hat{\sigma}$ that result from the maximization of (11) then identify all of the parameters in this model.

The form of the likelihood functions (9) and (10) suggests an important test of this model. If there are no nonpecuniary participation costs,

or if they are uncorrelated with the variables in X_i , then $\hat{\sigma} = \sigma$ or $\beta - \beta^* = \beta$. In this case the combined (log) likelihood of the experimental and control samples taken together is

$$(11) \quad = \mathcal{L}^1 + \mathcal{L}^2$$

$$= \sum_{i=1}^n \ln N_i + \sum_{i=n+1}^{n+m} \ln (1-N_i) + \sum_{i=n+m+1}^{n+m+r} \ln n_i / \sigma,$$

where r indicates the number of control observations and $N_i = N[(\ln G/t_i + .5et_i - X_i\beta)/\sigma]$. By examining the components of (11) it is clear that the parameters β and σ are common to all parts of the likelihood function. It is a relatively straightforward matter to maximize (11) with respect to these parameters and e by numerical methods and to compare the maximized value of (11) against the sum of the unconstrained values of (9) and (10) by a likelihood ratio test. The economic significance of this test is that the null hypothesis that $\beta - \beta^* = \beta$ and $\sigma = \hat{\sigma}$ is consistent with a model where program participation is not influenced by "welfare stigma" or other nonpecuniary participation costs. If this null hypothesis may be accepted it offers the opportunity of pooling the data from the control and experimental groups so as to increase the efficiency of the estimation of the effect of economic incentives on program participation by increasing the precision with which the parameter e may be estimated. If this null hypothesis is rejected, however, then these non-pecuniary participation costs must be judged important determinants of program participation and this raises questions about whether the results ought to be generalized to other populations without further investigation.

III. Empirical Results

An important message from equation (7) is that it should be fit to data on groups that come from homogeneous populations. In particular, groups with differences in mean earnings may be pooled using dummy variables to account for these differences, but this will not be sufficient to handle differences in variances because the value of σ affects every coefficient in (7). As a preliminary effort, therefore, the data were stratified by location of experiment (Seattle or Denver) and the variance in log earnings was calculated for the separate parts of the control group. The ratio of these variances (Denver/Seattle) was 1.006 and clearly did not justify stratification. A similar calculation comparing the variance in log earnings for Chicanos and others also indicated no necessity for stratification, but the same variance ratio comparing whites to blacks was 1.27 and clearly significantly different from unity at conventional test levels. Since the variance in the log earnings of whites is significantly larger than for blacks, it follows that σ in equation (7) will differ for these two groups and that special care must be taken in pooling the data for them. As a consequence, the first set of results reported below is for white families, who constitute around two-thirds of the sample.

A. Results for White Families

Column (1) of Table 2 contains estimates of the parameters σ and β obtained by maximizing equation (10). This is nothing more than a regression of log earnings on the variables indicated and the estimate of $1/\sigma$ is nothing more than the reciprocal of the estimated standard deviation of the regres-

sion disturbances. The variables in the vector X are a constant, pre-experimental log earnings, a dummy variable indicating location, and a set of dummy variables indicating normal income level. These latter variables were assigned values prior to the experiment and were used for the purpose of assignment to the eleven different negative income tax programs with various combinations of income guarantee (G) and tax rate (t).^{13/} As can be seen from Table 2, most of these variables have very little explanatory power except for pre-experimental earnings.

Column (2) of Table 2 contains the estimates of $1/\hat{\sigma}$, $\beta-\beta^*$, and e obtained by the maximization of equation (9). This is nothing more than the fit of a probit equation to the explanation of participant status among the experimental group. As with the control group, the X variables other than pre-experimental earnings have very little explanatory power in this equation. The log of the breakeven, on the other hand, is a powerful predictor of participant status. As the data in Table 1 also indicated, it follows that the major determinant of participant status is the generosity of the negative income tax plan that an experimental family was offered. The estimate of the compensated substitution elasticity, e , is around .2, which is certainly consistent with the other experimental and cross-sectional evidence available, but it is very poorly determined and has an estimated standard error of around .3.^{14/} This implies that the tax rate in the experimental plan has only a poorly determined effect on the family's participant status.

^{13/} Families within normal income or E-level groups were assigned randomly to the various negative income tax programs.

^{14/} See the summary of other experimental and cross-section results in Ashenfelter [1] and the results in Keeley, Robins, Spiegelman, and West [12], for example.

Table 2

Estimates of the Determinants of Payment Receipt for White
and Chicano Two-Parent Families (Estimated Standard
Errors in Parentheses)

	(1)	(2)	(3)	(4)
	Controls (Regression)	Experimentals (Probit)	Combined	Combined
Estimates of:				
$1/\sigma$ (or $1/\hat{\sigma}$)	1.332 (.038)	1.562 (.187)	1.328 (.036)	1.351 (.037)
e	---	.232 (.295)	.134 (.278)	.190 (.314)
Coefficient of:				
Pre-experimental Earnings	.395 (.046)	.187 (.046)	.309 (.035)	.421 (.043)
3 year treatment	---	.108 (.067)	.134 (.057)	.118 (.076)
Denver Location	.004 (.061)	-.102 (.065)	-.050 (.047)	-.039 (.048)
E0	-.383 (.243)	-.224 (.269)	-.352 (.193)	-.339 (.189)
E1	-.085 (.539)	.130 (.376)	.163 (.343)	.021 (.335)
E2	-.467 (.145)	-.155 (.156)	-.361 (.115)	-.343 (.113)
E3	-.242 (.101)	-.011 (.099)	-.141 (.074)	-.128 (.074)
E4	-.061 (.081)	.079 (.087)	-.006 (.068)	.003 (.061)
E6	.033 (.086)	.122 (.103)	.061 (.070)	.061 (.069)
Constant	5.493	7.295 (.432)	6.220 (.312)	5.243 (.385)
Pre-experimental Earnings*				.171 (.037)
Constant*				7.420 (.471)
Log Likelihood	-701.7	-479.5	-1193.5	-1184.7
Number of observations	624	800	1424	1424

* Coefficients with a star are for experimentals.

There is also very little evidence from the comparison of the estimates of $1/\sigma$ and $1/\hat{\sigma}$ in columns (1) and (2) of Table 2 that "welfare stigma" is at work in these data. Although the estimate of $1/\hat{\sigma}$ from the data for experimentals is larger than the estimate of $1/\sigma$ for controls, these estimates are clearly not significantly different. This lends remarkable support for the basic model of equation (7), especially when it is recognized that the results in column (2) are derived from observed dichotomous behavior that is based on a sample completely independent from that used to obtain the estimates in column (1) of Table 2.

The most efficient estimator of e may be that obtained from the combined evidence from the experimental and control groups and these results from the maximization of the combined likelihood function (11) are contained in column (3) of Table 2.^{15/} Although the estimated standard error of the estimate of e does decline slightly, the estimate of e does also. In neither case would the estimate of e be judged significantly different from zero at conventional test levels. Again, there is little evidence of a well determined effect of the tax rate on participant status.

It is, of course, possible to test whether it is sensible to combine the data for controls and experimentals by contrasting the maximized likelihood in column (3) against the sum of the maximized likelihoods in columns (1) and (2). This gives an estimated test statistic (twice the difference between the unconstrained and constrained likelihood values) of 24.6 to be compared against the tabulated χ^2 distribution with 10 degrees of freedom.

^{15/} A word about computational matters may be useful to others. Maximization was carried out numerically by first using an algorithm due to Davidon and to Fletcher and Powell and then by an algorithm due to Goldfeld, Quandt, and Trotter. A standard reference is Goldfeld and Quandt [9].

This comparison implies that the constraints would be rejected at the .01 significance level, but not at the .005 level. A comparison of the coefficients in columns (1) and (2) of Table 2 makes it clear why the constraints are rejected: The constants and coefficients of pre-experimental earnings are very different as between the experimental and control sample. The difference in constants is perhaps not very surprising since the assignment to experimental and control status was based, in part, on estimates of permanent income. It follows that experimental and control families may very well have been drawn from populations with different mean earnings levels. The difference in coefficients on pre-experimental earnings might likewise reflect differences in the correlation coefficient between earnings in adjacent periods between the two groups and could also be a result of the method of assignment to experimental and control group status.

The results in column (4) of Table 2 combine the data on controls and experimentals, but allow the constants and coefficients on pre-experimental earnings to differ as between the control and experimental groups. The χ^2 statistic comparing the sum of the likelihoods in columns (1) and (2) against the likelihood in column (4) now falls to 7.0 and would not be judged statistically significant at even the .10 level. Although the model in column (4) has a plausible a priori basis and obviously provides a satisfactory fit to the data, it is derived after an examination of the results in columns (1)-(3) of Table 2 and may simply be a case of over-fitting. It should be made clear, therefore, that it is reflecting the following fact in the data: Participant status is less highly (negatively) correlated with pre-experimental earnings than would have been predicted on the basis of data from the control group only. An alternative explanation, therefore, is that

"welfare stigma" is significantly negatively correlated with pre-experimental earnings. Still, as the comparison of the likelihood values in columns (3) and (4) indicates, these differences do very little to improve the predictions of participant status, so that the results in column (3) are perhaps still the most useful summary of the data.

B. Further Results

Column (1) of Table 3 contains the results for black families from the maximization of the combined likelihood function (11). As expected, the estimate of $1/\sigma$ is larger for black families than for white families, indicating the smaller variance in the log earnings of black families. The estimates of e and the coefficients on pre-experimental earnings are very similar as between black and white families, however. It seemed useful, therefore, to combine the data on both black and white families, duly allowing the estimates of $1/\sigma$ to differ between the two groups, in order to increase the precision of the estimate of the compensated labor supply elasticity e . These estimates for the full sample of 2,119 families are contained in column (2) of Table 3. As can be seen from the Table, the estimated standard error of the estimate of e is at its lowest there, although the estimate of e is still so imprecise that it would not be judged significantly different from zero in this Table either.

Finally, to this point little use has been made of the distinction between the different lengths of program to which experimental groups were offered access. In fact, to test whether the experiment's results depended on the length of the period over which benefits were promised experimental groups were assigned to programs with durations of both three and five years. In Table 2 a dummy variable indicating enrollment in a three-year program

Table 3

Additional Estimates^{a/} of the Determinants of the Receipt of
Payments in a Negative Income Tax Experiment (Estimated
Standard Errors in Parentheses)

	(1)	(2)	(3)
	Black Families	All Families	All Families
Estimates Of:			
1/σ for blacks	1.521 (.060)	1.491 (.060)	---
1/σ for whites	---	1.332 (.037)	---
1/σ	---	---	1.359 (.030)
e	.132 (.349)	.165 (.242)	.241 (.323)
Δe for 3 year program	---	---	.373 (.188)
Coefficient of Pre- experimental Earnings	.323 (.053)	.316 (.030)	.313 (.029)
Log Likelihood	-562.7	-1768.6	-1771.7
Number of observations	695	2119	2119

^{a/} Other variables included in these equations whose coefficients are not reported are a dummy variable for the Denver site, the normal income variables E0-E6 and a dummy variable indicating participation in a 3-year treatment.

was simply added as a control variable in all the equations fitted. In each set of estimates in Table 2 the coefficient of this variable is positive, but it is significantly different from zero only in the estimates of column (3). This, of course, indicates that receipt of payments was greater during the second year of the experiment among experimental groups offered a three year program than among experimental groups offered a five year program.

The economic analysis of program participation set out above offers one possible explanation for this finding. To see this consider a three period model of intertemporal labor supply. The three-year experimental group is offered participation in a negative income tax program for the first period only, while the five-year experimental group is offered participation for both the first and second periods. In considering whether the family is better off participating in the program during the first period, the families in the three year program have an incentive to substitute leisure from the second and future periods for leisure in the first period, while the families in the five year program have an incentive to substitute leisure only from future periods for leisure in the first period. In effect, a negative income tax program offers a fire sale on leisure, but there is more urgency in taking advantage of such a sale in its initial period if it is short.^{16/}

^{16/} This assumes that leisure time taken in adjacent periods is substitutable. If e_{11} and e_{22} are the own compensated elasticities of substitution in the first and second periods, then for the three year program the relevant substitution elasticity in equation (7) is e_{11} , while the relevant substitution elasticity for the five year program is $\theta e_{11} + (1-\theta)e_{22} + 2\theta e_{12}$, where e_{12} is the compensated elasticity of labor supply in the first period with respect to a change in the second period wage, and θ is the share of the discounted present value of earnings in the two periods received in the first period in the absence of program participation. If e_{11} and e_{22} are taken to be of similar magnitude, then $\Delta e = e_{11} - [\theta e_{11} + (1-\theta)e_{22} + 2\theta e_{12}] = -2\theta e_{12} > 0$ if $e_{12} < 0$ as would be the case when leisure is substitutable between periods.

To investigate this issue more systematically column (3) of Table 3 contains the results of estimating separate values of e for the three-year and five-year experimental groups. This is accomplished by entering both the tax rate into equation (7) and the tax rate multiplied by a dummy variable indicating enrollment in a three year program. As can be seen from Table 3, the estimate of e for the three year program is significantly larger than the estimate for the five year program, but both estimates are still very imprecise. This provides some indication of the presence of intertemporal substitution of leisure induced by the program, but it is hardly definitive. It also suggests that further research accounting for the time pattern of participation may be of considerable interest.

IV. Conclusion

The advantages of the econometric framework set out here for analysis of the discrete choice of participation in a negative income tax program are considerable. First, the purely mechanical fact that a more generous program will inevitably lead to greater receipt of program benefits is neatly separated from the behavioral responses the program may induce. The former behavior implies nothing more than that a family that is eligible for an income transfer will take it, while the latter emphasizes the importance of estimating the substitution effects that are the traditional objects of study by economists. Second, the estimation of the parameters necessary for predicting the extent of participation or receipt of benefits is straightforward and can be carried out with familiar computational methods. At an empirical level, the results from the Seattle/Denver Income Maintenance Experiment suggest that differences in participation across negative income

tax plans are due primarily to differences in program breakevens or generosity. Tax rate variations have only small additional effects on participation, although estimates of compensated elasticities of labor supply are certainly consistent with previous research.^{17/} Perhaps most important, the empirical results also suggest that in this Experiment the receipt of benefits was not affected by welfare stigma or other nonpecuniary program participation costs. This provides considerable evidence of the value of simple program participation estimates such as those in Table 1 that are based on uncontaminated estimates of the income distribution.

The analysis also opens up a considerable agenda for further research. First, it is natural to consider combining the discrete choice model of participation with an analysis of the actual labor supply responses of program participants. Program participation is clearly a choice variable and the analysis here provides a structural interpretation and method for treating participation and labor supply response as jointly determined endogenous variables. Likewise, the analysis can be generalized to deal with intertemporal choice and the time pattern of participation in a negative tax program. This will presumably lead to a specialized multivariate probit analysis like equation (7) that emphasizes both the serial correlation in the stochastic determination of log earnings and intertemporal labor supply elasticities.

Finally, the analysis provides a simple scheme for building the information from experiments into the simulation of the aggregate determination

^{17/} Although the estimated standard errors of e do not allow rejection of the hypothesis $e=0$, they nevertheless rule out values of e large enough to produce differences in predicted participation any larger than even the differences indicated in Table 1.

of program enrollment. Such projections must use information on the characteristics of the relevant aggregate population, and equation (7) shows just how estimates of the parameters of the appropriate group's earnings distribution can be used for this purpose. The same methods could well have wide applicability to any situation where budget constraints are kinked, ranging from block pricing for electricity to food or housing purchases under alternative subsidy arrangements.

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