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WORK INCENTIVES IN TRANSFER PROGRAMS (REVISITED)

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Work Incentives in Transfer Programs (Revisited)

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ABSTRACT

The introduction of a negative income tax--defined as a reduction in the tax rate of a transfer program, holding the guarantee constant--is widely supposed to have beneficial work incentives. This is not necessarily the case, for the net effect of a tax rate reduction is, in fact, ambiguous in sign. Neither the presently available econometric evidence, evidence from the negative income tax experiments, or the evidence from evaluations of federal AFDC legislation necessarily provides a reliable resolution of the ambiguity. A detailed historical study of the AFDC program, using simple reduced-form, nonstructural estimating equations, indicates that for female heads lower tax rates indeed increase labor supply. However, the evidence for men and married women is not so sanguine.

WORK INCENTIVES IN TRANSFER PROGRAMS (REVISITED)

Ever since Milton Friedman made the argument for a negative income tax (Friedman, 1962), it has been an article of faith among economists of all persuasions that work incentives in transfer programs can be provided by a low benefit-reduction rate, or tax rate, in such programs. Low tax rates allow recipients to "keep" a significant proportion of increased earnings by a less than dollar-for-dollar reduction in transfer benefits. Despite the failure of Congress to enact a negative income tax, the argument has played and continues to play an important role in welfare-reform discussions in Washington. For example, work incentives were the primary motivation behind 1967 federal legislation that lowered the tax rate in the Aid to Families with Dependent Children (AFDC) program from 100 percent to 67 percent, and the possible work disincentives of 1981 federal legislation that increased the tax rate back to 100 percent have been extensively discussed.

The volume of research spawned by the interest in the negative income tax and in work-incentive-inducing welfare reform has been impressive. From the early nonexperimental studies surveyed in Cain and Watts (1973) to the multiple reports of the negative income tax experiments (surveyed in Moffitt and Kehrer, 1981) to the general literature on labor supply, which is relevant inasmuch as it provides estimates of income and substitution elasticities (surveyed in Killingsworth, 1983), an enormous body of evidence has been generated. We now have, or should have, a reasonably solid basis upon which to say whether lower tax rates induce more work effort and, if so, how much.

In this paper it is argued that in one important respect we still have little knowledge of the effect on labor supply of lowering a welfare-program tax rate. It is pointed out in the first section of the paper that, because of the nonconvexity of the budget constraint created by a transfer program, the effect of tax rates on labor supply is theoretically ambiguous. The ambiguity has nothing to do with the relative magnitude of income and substitution effects but arises because of changes in participation in the program. While the econometric issues in modeling the response to such constraints have been extensively discussed in the labor-supply literature, the implications of such constraints for the original issue of how tax rates affect labor supply have not been fully drawn out. (Similar ambiguities arise for the effects of "notches" on labor supply; for example, the idea that a notch "no doubt discourages work" (Blinder and Rosen, 1985, p. 736) is not necessarily correct.) It is also argued in the first section of the paper that neither the econometric evidence available on income and substitution elasticities, nor the evidence from the negative income tax experiments, nor the evaluations of 1967 and 1981 AFDC legislative changes necessarily provides a reliable answer to the relevant question, namely, whether lowering the tax rate in a transfer program (holding the guarantee constant) will increase or decrease labor supply.

The focus of the paper is entirely on the labor supply effects of lowering the tax rate, but the more general redistributive context should be kept in mind. Redistribution per se takes place because the utility functions of donors contain the income levels of the poor, but those utility functions presumably also contain the labor supply levels of the poor. In the conventional theory of income redistribution it is assumed that an extra dollar of transfer income will reduce the labor supply of the poor, so a donor population will transfer dollars to the poor up to the point at which the marginal utility of increasing the income of the poor equals the marginal disutility of reducing its own income plus the marginal disutility of reducing labor supply among the poor. This paper is concerned with whether the conventional assumption of labor-supply disincentives is factually correct. If not, obviously the nature of the tradeoff is changed.

In the second section of the paper, new evidence on this labor-supply question is provided for the AFDC program. Simple reduced-form, nonstructural labor supply equations are estimated to obtain the sign of the partial correlation between hours of work and tax rates in the program. A fairly comprehensive examination is undertaken--cross-sectional as well as historical time-series variation is examined. The results indicate that, for female heads at least, higher tax rates do appear to have a net negative effect on labor supply. However, as discussed in the conclusion, the policy implications are not necessarily comforting.

I. LOWERING THE TAX RATE: THEORY AND EVIDENCE

Figure 1 shows the familiar labor-leisure diagram for a transfer program with a guarantee ($400/month) and a linear tax rate. The line $\overline{KLN}$, whose slope is the hourly wage rate, is the pretransfer constraint. If the tax rate were 100 percent the constraint would have the horizontal segment shown, while lowering the tax rate would pivot this segment upward to that along which points I, J, M, and O lie. That the labor-supply response to such a change is ambiguous is clear from an examination of the diagram. Although an initial nonworker, such as the individual at I, may increase hours of work to point J--this is the classic response--other individuals may not. Individuals newly covered by the program (such as at point L) and some individuals above the new breakeven point (such as at point N) will now become participants in the program and will reduce their labor supply. In addition, some individuals observed at points such as K--this rejection of an increase in disposable income can be explained by the existence of welfare stigma (Moffitt, 1983)--will choose, after the tax rate reduction, to join the program because the benefit has increased sufficiently to outweigh the stigma cost. Thus the sign of the net effect is ambiguous and will depend upon the relative magnitudes of the responses of the different individuals along the constraint and their relative numbers (i.e., the distribution of income).[1]

It should be noted that, of course, a utility-compensated reduction in the tax rate must necessarily increase labor supply. If, for example, the guarantee were reduced simultaneously with the tax rate reduction, labor supply would more likely increase. This would also occur if a balanced-budget tax rate reduction were implemented, for since the reduction in the tax rate

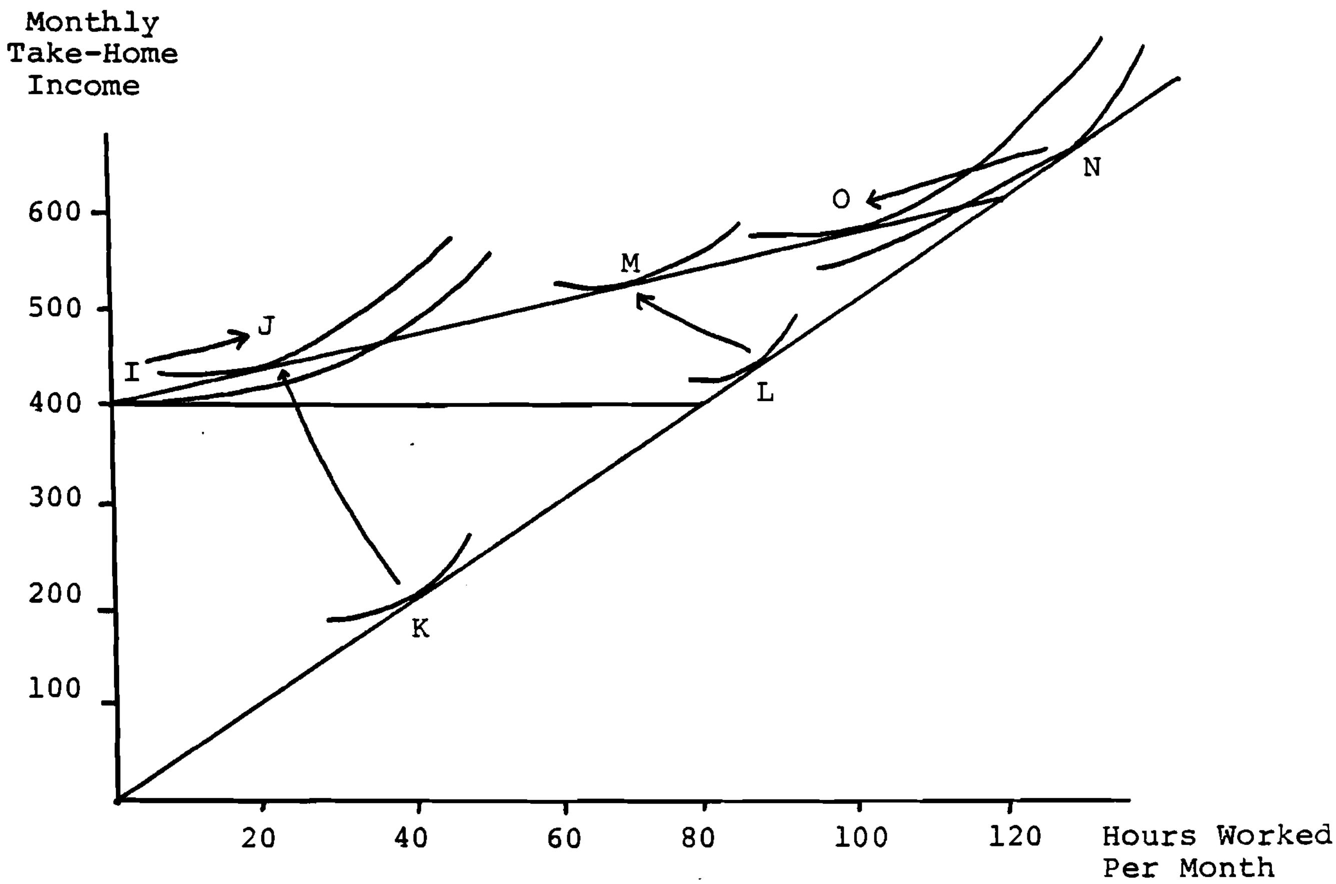

Figure 1. Effect of Tax Rate Reduction
on Labor Supply

increases welfare expenditures the guarantee would have to be reduced to hold them constant. The case we are concerned with here is, instead, that in which an outside donor population wishes to increase the aggregate transfer to the poor population.[2]

Knowledge of income and substitution elasticities is not by itself directly usable to resolve the labor supply ambiguity because such elasticities are defined for linear price and income changes. But because any pair of income and substitution elasticities implicitly defines a preference map, it can be used to simulate the labor supply response to changes in non-convex, piecewise-linear budget sets.

The results of one such simulation are shown in Table 1, which shows the work disincentives of transfer programs relative to no program at all. The survey articles on labor supply referenced at the beginning of the paper were used to form ranges of substitution and income elasticities from the literature, and these were used to calculate the labor supply disincentives of several transfer programs with various guarantees and tax rates. The simulations were performed on the Survey of Income and Education, a nationally representative data set.[3] The table indicates, for example, that a transfer program with a guarantee equal to 50 percent of the poverty line and with a 100 percent tax rate would lower male labor supply in the United States by .19 hours per person per week if a lower-bound set of elasticity estimates is used (relative to no program at all). The .19 value is fairly modest, but this is in large part because so few males choose to be below the breakeven level--values in the table are mean values for all males in the population regardless of their participation status in the program. The table also shows that lowering the tax rate but holding the guarantee constant at the same level

Table 1
Effect of an NIT on Mean Weekly Hours of Work in the U.S. Population[a]

	Tax Rate			
	1.0	0.75	0.50	0.25
MEN				
Low elasticities				
$G^b = 0.50$	-0.19	-0.07	-0.06	-0.10
G = 0.75	-0.69	-0.18	-0.16	-0.19
G = 1.00	-1.66	-0.37	-0.29	-0.28
High elasticities				
G = 0.50	-0.79	-0.80	-0.73	-1.25
G = 0.75	-2.45	-2.43	-2.10	-2.76
G = 1.00	-5.06	-4.91	-4.00	-4.24
MARRIED WOMEN				
Low elasticities				
G = 0.50	-0.10	-0.04	-0.05	-0.11
G = 0.75	-0.32	-0.14	-0.15	-0.32
G = 1.00	-0.61	-0.32	-0.32	-0.60
High elasticities				
G = 0.50	-0.26	-0.28	-0.29	-0.66
G = 0.75	-0.64	-0.72	-0.92	-1.92
G = 1.00	-1.11	-1.51	-1.90	-3.53
FEMALE HEADS				
Low elasticities				
G = 0.50	-0.81	-0.49	-0.35	-0.33
G = 0.75	-2.18	-1.08	-0.68	-0.50
G = 1.00	-4.02	-1.74	-1.00	-0.64
High elasticities				
G = 0.50	-2.06	-2.22	-2.02	-2.26
G = 0.75	-4.62	-4.99	-4.29	-3.87
G = 1.00	-7.34	-7.92	-6.50	-5.31

[a]Let $(\varepsilon_w, \varepsilon_y)$ represent a pair of wage and total-income elasticities. The low-elasticity pairs are (0.01, -0.01), (0.10, -0.05), (0.05, -0.02) for men, married women, and female heads, respectively. The corresponding high-elasticity pairs are (0.08, -0.20), (0.60, -0.30) and (0.20, -0.25).

[b]As a fraction of the poverty line for a family of four. In 1977 the weekly poverty line was $119.

Source: Moffitt (1985a).

reduces the labor supply disincentive--that is, males work longer hours--but only up to a point. Eventually a tax rate reduction reduces labor supply, simply because the upward movement in the breakeven level eventually reaches into the thick part of the male income distribution, thereby drawing proportionately more males into the program than are already on it. Many of the other entries in the table show the same perverse phenomenon--it occurs for males at sufficiently low tax rates for the two lower guarantees, regardless of the elasticities used, and it occurs at sufficiently low tax rates for married women's estimates at all guarantees and all elasticities. Indeed, at the high-elasticity estimates for married women, 100 percent tax rates provide the smallest work disincentives! The male-female difference is a result of the lower mean labor supply of women, which moves the thick part of the income distribution to a lower level than for males.

These econometric estimates hardly constitute conclusive evidence. The elasticities are drawn, for the most part, from estimates of wage rate and nonwage income elasticities, not from estimates of guarantee and tax effects. Most of these studies ignore welfare programs (and most ignore tax systems as well), and hence ignore nonlinear budget constraints in their estimation. Of the underlying labor supply studies, only two (Hausman, 1981, and Moffitt, 1983) were models of the AFDC program which incorporated nonlinear-constraint estimation methods. In addition, even if the wage-rate and nonwage-income elasticities are appropriate estimates of the response to shifts in linear constraints, an estimate of the effect of the response to shifts in nonlinear constraints requires an extra simulation, such as that reported in Table 1, which in turn requires additional assumptions (e.g., for the form of the utility function). Thus we have very few <u>direct</u> estimates of tax and guarantee effects. Partially as a result, for example, we also do not know if the indirect estimates provided in Table 1 are consistent with the actual experience in the AFDC program.

Ideally, the negative income tax experiments should be capable of providing more direct estimates. With randomly chosen experimental and control groups drawn from the entire U.S. population, and with some experimental families given different tax rates but the same guarantee, a simple comparison of mean hours of work across groups would provide a direct estimate of the effect of interest.

The conventional wisdom is that the experiments provided tax rate effects that were often weak and mixed in sign. Table 2, which shows the signs of the tax rate estimates in several studies, supports this generalization. To obtain the table, the one study in each of the experiments which comes closest to having estimated direct tax and guarantee effects (e.g., a simple regression of hours work on the tax rate and guarantees) was selected. As the table shows, the tax rate effects were positive about half the time and negative half the time. To determine whether the pattern of positive and negative effects is consistent with Table 1, the guarantee and tax rate levels in each study can be matched up with the Table 1 entries. As the last column of Table 2 indicates, there is quite a bit of consistency across the two tables. In fact, given the many differences between the experiments with their real-world complications and the pure form simulated in Table 1, the consistency is fairly strong.

The major difficulty with using these results arises from the truncation of the experimental samples. If any of the individuals above the sample truncation points were potential respondents to the plans tested, as Figure 1

Table 2
Experimental Evidence on Effect of NIT
on Mean Hours of Work[a]

	G, t levels	Sign of $\partial H/\partial t$	Consistency with Table 1
Men			
Watts and Horner (1977)[b]	$\bar{G} \mathrel{\hat{=}} .75$ $\bar{t} \mathrel{\hat{=}} .50$	$\partial H/\partial t>0$ for whites and blacks $\partial H/\partial t<0$ for Spanish-speaking	Roughly consistent
Bawden and Harrar (1977)	$\bar{G} \mathrel{\hat{=}} .75$ $\bar{t} \mathrel{\hat{=}} .50$	$\partial H/\partial t>0$ for Iowa $\partial H/\partial t<0$ for No.Carolina	Roughly consistent
Hausman and Wise (1979)	G = .75 or 1.0 t = .40 or .60	$\partial H/\partial t>0$ at G = .75 $\partial H/\partial t<0$ at G = 1.0	Completely consistent
Robins and West (1982)[c]	G = .90 to 1.3 t = .5 or .7	$\partial H/\partial t>0$	Inconsistent
Married Women			
Cain et al. (1977)[d]	G = .50 to 1.25 t = .30 to .70	$\partial H/\partial t>0$ for all	Consistent at high elasticity/Less so at low elasticity
Bawden and Harrar (1977)	$\bar{G} \mathrel{\hat{=}} .75$ $\bar{t} \mathrel{\hat{=}} .50$	$\partial H/\partial t<0$ for all	Inconsistent
Robins and West (1982)[c]	G = .90 to 1.3 t = .5 or .7	$\partial H/\partial t>0$	Consistent
Female Heads			
Robins and West (1982)[c]	G = .90 to 1.3 t = .5 or .7	$\partial H/\partial t<0$ in 2 of 3 cases	Partially consistent

Notes:

[a]All results are those on unconditional hours of work (i.e., including zeros). G is percent of poverty line.

[b]See Table 3.27, p. 110.

[c]See Table 2, p. 882. Numbers above show values of partial derivative of H w.r.t. t, including above-breakeven variables, all evaluated at means.

[d]See Table 4.7, p. 138.

indicates is possible, the measured effect in the experimental data would be biased. As may be seen from Figure 1, omitting some of the high-income individuals is likely to bias the hours-tax-rate correlation in a negative direction--that is, $\partial H/\partial t$ is more likely to be negative because high-income individuals have a positive $\partial H/\partial t$. This may be the reason for the most inconsistent entry in Table 2, that for rural wives (the rural had the lowest truncation point of all the experiments), but all the entries are potentially affected to some degree.

A final possibility for obtaining direct evidence on the effect of changing tax rates on labor supply might be obtainable from evaluations of federal legislative changes in the AFDC program. In 1967 the federal government lowered the tax rate in the program from 100 percent to 67 percent and in 1981 it was effectively raised back to 100 percent. Unfortunately, the existing evaluations of both of these changes are fraught with many problems. Only three crude studies of the 1967 tax rate reduction were conducted, in each case by a simple examination of whether the hours of work of AFDC recipients were higher or lower prior to 1967 than later, after the tax rate reduction was implemented. Hours of work among recipients was higher, of course, but only because the breakeven level was higher. Thus this evidence is of almost no value. Studies of the 1981 tax rate increase have likewise erred by only examining the hours of work of a group of AFDC recipients who were on the rolls both before and after the tax rate increase. Obviously hours of work must fall in this population because the breakeven level has fallen; women who have left the rolls and increased hours are excluded from the sample. These studies were also complicated by the simultaneous onset of the 1981 recession, creating traditional before-and-after inference problems.[4]

This relatively brief review of the evidence reveals that our knowledge of the effect of tax rates on labor supply is, even at this date, fairly shaky. However, the one major program for which direct estimates of such effects are in principle obtainable is the AFDC program, to whose evaluation I now turn.

II. EVIDENCE FROM THE AFDC PROGRAM

A. General Issues

Despite the fact that the AFDC program was the major stimulus to Milton Friedman's negative-income-tax proposal and that the possible work disincentives of the AFDC program have been the primary focus of welfare reform discussions, there have been very few labor supply studies of the program. Most of those that have been done have been primarily concerned with the often difficult econometric issues involved in estimating labor supply functions in the presence of piecewise-linear budget sets, in an attempt to isolate pure income and substitution elasticities (i.e., those for a linear budget constraint).[5] Such econometric models are fairly difficult to estimate and require a number of restrictive assumptions. Moreover, if the primary object of interest is the sign of the partial correlation between hours of work and the tax rate in the program (holding the guarantee constant), such structural models are not necessary. Since the sign of this partial correlation is indeed the object of interest here, structural estimation is explicitly avoided. The goal instead is to obtain direct, nonparametric estimates of the partial correlation by estimation of reduced-form hours equations as a function of tax rates and guarantees.

The structural models that have been estimated for AFDC also have been estimated only on cross-sections of the female head population. But if the object of interest is the sign of the hours-tax-rate partial correlation, it makes sense to determine that sign in time series as well. In addition, of course, if the effects of the 1967 and 1981 legislations are of direct interest, a time-series dimension is required. Therefore, both cross-section and time-series correlations will be estimated below.

It is important in this type of analysis to obtain variation in AFDC benefit-formula parameters that is as fully exogenous as possible. There are two sources of exogenous variation in such parameters. The first and most important is the cross-sectional variation across states, for each state determines its own benefit formula within federal guidelines.[6] The second source of variation is time-series variation. Real guarantees have declined continuously in the U.S. since 1967 and tax rates went through major changes in 1967 and 1981. Tax rates have also drifted in other years as a result of state administrative actions (see Fraker et al., 1985).

The underlying structural model upon which the estimates will be based is a standard labor supply model. Let H_i be the hours of work for individual i; W_i be the hourly wage rate; N_i be nontransfer nonwage income; G_i be the AFDC guarantee; t_i be the AFDC tax rate; and P_i be an AFDC participation variable equal to one if the individual is on AFDC and zero if not. Then a simple structural labor-supply model incorporating AFDC is:

$$H_i = f[W_i(1-t_iP_i),\ N_i + G_iP_i;\ \varepsilon_i] \tag{1}$$

$$P_i = \begin{cases} 1 & \text{if} \quad P_i^* > 0 \\ 0 & \text{if} \quad P_i^* < 0 \end{cases} \tag{2}$$

$$\begin{aligned} P_i^* = {} & V[W_i(1-t_i),\ N_i + G_i;\ \varepsilon_i] \\ & - V[W_i,\ N_i;\ \varepsilon_i] + u_i \end{aligned} \tag{3}$$

where ε_i and u_i are error terms and V is the indirect utility function. Equation (1) is a conventional labor supply equation conditional upon AFDC participation, with an error ε_i that is to be interpreted as incorporating unobserved tastes for work (variations in ε_i spread individuals over the constraint). Equation (3) represents the utility gain from participation, which is a function of tastes for work (ε_i) as well as "tastes" for welfare, u_i (which incorporates stigma). Participation may not take place even if the difference in the V functions is positive, if u_i is sufficiently negative (Moffitt, 1983). Individual K in Figure 1 is an example of such a case. Since tastes for work and welfare are likely to be correlated, ε_i and u_i are assumed to be correlated.

Equation (1) can be rewritten as follows:

$$H_i = P_i \ f[W_i(1-t_i),\ N_i + G_i;\ \varepsilon_i] + (1-P_i)\ f[W_i,\ N_i;\ \varepsilon_i] \qquad (4)$$

A first-order Taylor series expansion around the means of t_i, G_i, W_i, and N_i, and around the means of ε_i and u_i (i.e., zero), gives the following:

$$\begin{aligned} H_i \doteq \overline{H}_i &+ \overline{\frac{\partial H_i}{\partial t_i}}\,(t_i - \overline{t}) + \overline{\frac{\partial H_i}{\partial G_i}}\,(G_i - \overline{G}) \\ &+ \overline{\frac{\partial H_i}{\partial W_i}}\,(W_i - \overline{W}) + \overline{\frac{\partial H_i}{\partial N_i}}\,(N_i - \overline{N}) \qquad (5) \\ &+ \overline{\frac{\partial H_i}{\partial \varepsilon_i}}\,(\varepsilon_i) + \overline{\frac{\partial H_i}{\partial u_i}}\,(u_i) \end{aligned}$$

where a bar above a variable denotes a term evaluated at the means of the four variables and two error terms. The approximate mean hours of work in the population (taken over all ε_i and u_i) is therefore:

$$E(H_i | t_i, G_i, W_i, N_i) = \alpha_1 t_i + \alpha_2 G_i + \alpha_3 W_i + \alpha_4 N_i + X_i \beta \qquad (6)$$

where each α represents the relevant partial derivative in (5) and where a vector of variables (X_i) and coefficients (β) has been substituted for the remaining terms (H_i minus the products of mean partials and variables).

The estimate of α_1 will provide an estimate of the effect of a unit change in the tax rate at the means of the variables in the population. As stressed above, the sign of α_1 is theoretically ambiguous, for it summarizes the net effect of a change in the tax rate on both the probability of being a recipient as well as on hours of work if a recipient. This reduced-form coefficient is the best estimate (statistically speaking) of the effect of AFDC tax rates on labor supply. Of course, because α_1 is not a structural parameter, tax-rate effects at values of the variables other than their means cannot be determined. That is, because the "true" equation (4) is so nonlinear in the variables, a linear extrapolation is not warranted. However, the magnitude of the estimate of α_1 is important because, by comparison with the entries in Table 1 that correspond to U.S. tax and guarantee means, it provides an indirect indication of the consistency of estimated income and substitution elasticities in the labor supply literature with the hours-worked and AFDC tax rate data in the U.S. If the two are similar, this implies that the simulations are consistent with the data and (hence) we can use those simulations with more confidence in predicting the effects of tax rate changes away from the mean. If they are very different, it implies that the income and substitution elasticities in the literature are inconsistent with the hours-tax-rate correlations for the AFDC program. This result would presumably suggest additional research, including structural estimation, to determine more precisely the cause of the difference.

Regardless of the consistency of the estimate of α_1 with Table 1, the estimates are of interest for other reasons. First, it is of interest to know whether the α_1 estimates are similar in time-series and cross-section; if they are, this obviously will strengthen our confidence in the estimates. If they are consistent in both time-series and cross-section <u>and</u> consistent with Table 1, our certainty should be even greater. Second, answers to specific historical questions concerning the effect of 1967 and 1981 legislations can be obtained from estimates of α_1 on appropriate time-series data; estimates of the sign of α_1 will tell us whether those tax rate changes increased or decreased labor supply.[7]

B. <u>Data</u>

To conduct an analysis of the comprehensiveness desired, data on AFDC tax rates and guarantees as well as data on hours of work, wages rates, and the other variables are needed for the states in the U.S. for several years in time. The binding constraint in assembling such a data base is the availability of AFDC tax rates and guarantees, which are only available by state for nine years--1967 to 1981 at two-year intervals, and 1982.[8] These are consequently the years used for the analysis. To obtain data on hours of work and the other labor-supply variables, the microdata files from each March Current Population Survey (CPS) corresponding to a year of the AFDC parameters are used. From each of the nine CPS files all female heads with at least one child under 18 (the primary demographic eligibility characteristic for AFDC) are obtained. About three thousand such women are in each CPS. The data contain information on hours of work in the March survey week and information that can be used to construct hourly wage rates and nonwage income.[9] It is not known whether or not each woman is on AFDC in the survey week but this is not required for estimation.[10]

The primary difficulty in using these data is the sheer size of the data set (approximately twenty-seven thousand observations). One alternative to this problem would simply be to subsample the observations to reduce the data set to a manageable size. However, here instead the data are grouped by state in each year, and state means are constructed for all the variables. This procedure has several advantages. First, because the number of observations in smaller states are often sparse in many of the years, subsampling would eliminate many of the states. This would be a disadvantage because the equation will be estimated separately by year and because both random effects and fixed effects estimators will be used (see later). Second, the state is the natural unit of observation for a study of the AFDC program, since guarantees and taxes are constant within states. Third, this grouping allows us to employ panel-data techniques more easily, for the set of state means over time constitute a panel data set to which such techniques can be applied. Their application to the individual data would be more difficult.

Because of the grouping, all analyses below are weighted.[11] The means of the variables used in the analysis are shown in the Appendix.

C. Estimating Techniques

As just noted, the grouping by state has the advantage of forming a panel data set consisting of a time-series of state cross-sections. Panel estimation techniques can consequently be applied even though the underlying data are drawn from a set of independent cross-sections, not from a panel.[12] Panel data are particularly useful here because they allow the estimation of hours-tax-rate correlations from a variety of different combinations of cross-sectional and time-series sources.

To exploit this variety of sources of variation, a number of different estimators are used. They can all be discussed in the context of a conventional model of individual behavior that assumes both an individual-specific effect and a random effect:

$$H_{it} = Z_{it}\delta + \mu_i + \varepsilon_{it}, \qquad i = 1, \ldots, N_t \qquad t \in A_i \tag{7}$$

where N_t is the number of state means available in each year, A_i is the set of time periods for which data for state i are available, and where Z_{it} and δ contain the variables and coefficients, respectively, in equation (6). The error terms are assumed to have zero means and variances σ^2_μ and σ^2_ε, and are assumed to be distributed independently of one another. An estimator which utilizes only the cross-sectional variation in the data is the <u>between</u> estimator, δ_b, which is obtained by estimating the equation:

$$H_{i\cdot} = Z_{i\cdot}\delta_b + e_i, \qquad i = 1, \ldots, N \tag{8}$$

where $N = \operatorname{Max}_t (N_t)$. The between estimator relies only on between-unit variation and is consistent if $E(Z_{it}\ \mu_i) = 0$. However, it is not efficient. A variant of the between estimator can be obtained by estimating (8) separately by time period.

An estimator which utilizes primarily time-series variation is the <u>within</u> estimator, which is obtained by estimating the equation:

$$(H_{it} - H_{i\cdot}) = (Z_{it} - Z_{i\cdot})\,\delta_w + e_{it}, \qquad i = 1, \ldots, N_t \qquad (9)$$
$$t \in A_i.$$

The within estimator utilizes only the over-time variation within each state. It still, of course, utilizes some cross-sectional variation, for the estimator is based upon differences across states in the over-time variation of the variables. The within estimator is consistent and also efficient if μ_i is treated as a fixed effect and potentially correlated with Z_{it}.

An estimator that utilizes cross-sectional as well as time-series variation is the <u>random effects</u> estimator, which is obtained by estimating the pooled equation (6) with a GLS adjustment for the correlation of the error terms for the same individual unit over time. A simple method of obtaining the random effects estimator is by estimating the equation (Hausman and Taylor, 1981):

$$H_{it} - \hat{\rho}_i H_{i\cdot} = (Z_{it} - \hat{\rho}_i Z_{i\cdot})\,\delta_r + e_{it} \qquad i = 1, \ldots, N_t \qquad (10)$$
$$t \in A_i$$

where[13]

$$\hat{\rho}_i = 1 - \frac{\hat{\sigma}_\varepsilon}{\sqrt{\hat{\sigma}^2_\varepsilon + T_i\,\hat{\sigma}^2_\mu}} \qquad (11)$$

The quasi-first-difference equation (10) eliminates part of the cross-sectional variation but not all of it so long as $\hat{\rho} < 1$. Under the assumption that μ_i is a random effect distributed independently of Z_{it}, δ_r is a consistent and efficient estimator of δ. The estimator is an optimal weighted average of the pure cross-sectional, between estimator and the over-time, within estimator.

All the estimators discussed thus far can be estimated with or without dummies for the nine time periods in the panel. If time dummies are included, part of whatever time-series variation is in the sample is effectively eliminated as a determinant of the δ estimator.

A final estimator is the "between-time-periods" estimator, commonly called the <u>time-series</u> estimator. It is the mirror image of the between estimator because only the means within each time period, instead of within each unit, generate the variation in the variables:

$$H_{\cdot t} = Z_{\cdot t}\delta_s + e_t, \qquad t = 1, \ldots, 9 \tag{12}$$

The disadvantage of this estimator is that it can only be based upon nine observations. Compared to the within estimator, the time-series estimator eliminates all, not only part, of the cross-sectional variation in the sample. A related time-series estimator is that obtained by first-differencing:

$$\Delta H_{\cdot t} = \Delta Z_{\cdot t}\delta'_s + e_t, \qquad t = 1, \ldots, 9 \tag{13}$$

D. Results

Table 3 shows the results of estimating various forms of the random effects, fixed effects (within) estimator, and the between estimator. Column (1) shows the result of including only the guarantee and tax rate as regressors. The guarantee

Table 3
Estimates of the Hours-Worked Model

	Random Effects			Fixed Effects	Between
	(1)	(2)	(3)	(4)	(5)
Tax Rate	0.682 (1.234)	0.541 (1.147)	-1.520 (1.65)	-1.820 (1.57)	-3.17 (3.45)
Guarantee[a]	-0.996** (0.280)	-0.925** (0.324)	-0.850** (0.366)	0.863* (0.485)	-2.42** (0.476)
Hourly Wage	-	-0.021 (0.252)	-0.004 (0.264)	0.186 (0.240)	-0.025 (0.610)
Other Income (N)[a]	-	0.350 (0.515)	0.286 (0.518)	0.440 (0.497)	1.61* (0.948)
Age	-	0.089 (0.121)	0.032 (0.127)	0.317** (0.117)	-0.797** (0.319)
Education	-	1.673** (0.377)	1.66** (0.413)	1.37** (0.418)	2.40** (0.756)
Race	-	3.210* (1.731)	2.740 (1.85)	5.72** (2.54)	-4.67* (2.43)
No. Children	-	-3.458** (0.830)	-3.40** (1.02)	-2.22** (0.952)	-8.32** (2.09)
South	-	1.332 (0.867)	1.24 (0.897)	-[b]	-0.498 (0.874)
U	-	-0.574** (0.080)	-0.597** (0.127)	-0.321** (0.127)	-1.10** (0.260)
Time Dummies[c]					
1969	-	-	-0.298 (0.621)	0.116 (0.515)	5.74 (12.4)
1971	-	-	-0.490 (0.688)	-0.546 (0.604)	-4.99 (10.3)
1973	-	-	-1.34* (0.709)	-0.448 (0.650)	-1.90 (3.96)
1975	-	-	-0.495 (0.951)	-0.223 (0.902)	-5.10 (4.33)
1977	-	-	-1.030 (0.855)	-0.420 (0.779)	-2.12 (4.31)
1979	-	-	-0.183 (0.906)	1.50* (0.830)	-7.03 (5.60)
1981	-	-	-0.366 (1.12)	1.62 (1.05)	-3.29 (5.10)
1982	-	-	0.559 (1.54)	1.93 (1.44)	0.998 (5.37)
Constant	21.295	7.738	11.2	-[b]	57.5
Standard Error	2.78	3.003	1.95	1.59	1.50
Rho	0.540	0.367	0.342	-	-
R-Squared	0.380	0.601	0.615	0.390	0.777

Notes: U = Department of Labor Unemployment Rate.
Standard errors in parentheses.
*: Significant at the 10 percent level.
**: Significant at the 5 percent level.
[a]Divided by 100.
[b]Coefficients on variables that are constant over time cannot be estimated.
[c]1967 omitted.

coefficient is significantly negative but the coefficient on the tax rate is positive. However, it has an extremely large standard error. Column (2) shows the result of adding the wage rate and nonwage income as well as a set of socioeconomic variables. The guarantee and tax-rate coefficients are close in both magnitude and significance to those in the previous equation. This is an indication of a lack of overall correlation between the two AFDC parameters and the other variables added to the equation.

The other variables show a mixed pattern of effects. Neither the coefficient on the wage rate nor on other income is of the expected sign, but neither is significant. Race (proportion non-white), the number of children, the level of education, and the unemployment rate are all significant. Each is of the sign that should be expected from the past labor-supply literature, with the possible exception of race, but it is not infrequently found that black women work longer hours than white women.

Interestingly, however, the inclusion of time dummies in column (3) changes the tax-rate effect considerably, for it now becomes negative with a much larger t-statistic (though still below significance at conventional levels). This result is an indirect indication that a positive hours-tax-rate correlation exists in the time-series variation in the data. The cross-sectional partial correlation appears to be negative.

Columns (4) and (5) show the estimates of the fixed effects and the between models. The tax-rate coefficients are negative in both cases and, rather remarkably, very close in magnitude in the random effects and fixed effects models. This is unusual, for most data sets show quite different results when both of these estimators are obtained. Note too that the fixed effects estimator is not a pure time-series estimator and hence the positive hours-tax-rate effect mentioned just previously is not necessarily to be expected here. The between estimator, which utilizes entirely cross-sectional variation (but with the smallest sample size of any of the models in the table) gives the largest negative tax-rate effect. This is further confirmation that the negative tax-rate effects are stronger in cross-section than in time series.[14]

Table 4 shows the result of estimating the separate cross-sections by year. Here the tax rate coefficient bounces around considerably from year to year, as should be expected from the reduction in sample size per regression, but the coefficient is negative in all cases but one.

Table 5 shows the results of the pure time-series estimates. Since only nine observations are available for these regressions, they should be considered only suggestive. But they provide information on the previous results, for the tax rate effects in columns (1) and (2)--which differ only by whether a time trend is included--are positive. This confirms the suspicion mentioned above. However, a first difference of the hours equation (all equations here are adjusted for trend and cycle) gives a negative tax rate coefficient. To the extent that an aggregate fixed effects model is plausible, this equation should be given more credence than the level equations. Note too that this result shows why the "time-series" within estimator in Table 2, which is closely related to a first-difference estimator, did not evidence a positive hours-tax-rate correlation.

The source of the positive hours tax-rate effect in the level time-series regression can be seen in Table 6, which shows the values of the variables along with the level of the AFDC participation rate by year. The table shows that

Table 4
Guarantee and Tax Rate Coefficient by Year

	1967	1969	1971	1973	1975	1977	1979	1981	1982[b]
Tax Rate	-4.25	-0.619	-11.5	-4.04	0.658	-3.39	-6.10	-8.27	-16.5
	(5.70)	(7.45)	(6.16)	(6.33)	(9.42)	(4.64)	(4.26)	(7.99)	(10.9)
Guarantee	-1.87*	-2.19	-1.89**	-0.230	-1.80**	-1.80**	-1.80	-3.04	-0.045*
	(0.976)	(1.54)	(0.935)	(1.32)	(1.14)	(0.645)	(1.03)	(1.62)	(0.026)
Sample Size	29	26	27	22	20	46	37	29	9

Notes:
Variables in regressions 1967-1981 include all those in Table 2 except for year dummies.
Standard errors are in parentheses.
*: Significant at the 10 percent level.
**: Significant at the 5 percent level.
[a]Divided by 100.
[b]Includes education and number of children only.

Table 5
Time-Series Regressions

	H (1)	H (2)	ΔH (3)	H (4)	ΔH (5)
Tax Rate	0.76 (2.05)	0.46 (1.66)	-	-2.12 (2.20)	-
Guarantee	-0.07 (0.05)	-0.09 (0.19)	-	-0.09 (0.10)	-
ΔTax Rate	-	-	-0.18 (2.04)	-	-2.87 (1.89)
ΔGuarantee	-	-	-0.05 (0.71)	-	0.04 (0.06)
Year	0.06 (0.20)	-	-	0.07 (0.15)	-
U	-0.59* (0.22)	-0.55* (0.17)	-	-0.32 (0.22)	-
ΔU	-	-	-0.41* (0.19)	-	-0.47* (0.14)
D75[a]	-	-	-	-	1.55* (0.70)
D75*(Year-75)	-	-	-	0.99* (0.52)	-
Intercept	27.8	34.5	0.04	1.31	-.23
R-Squared	.82	.82	.61	.92	.85

Notes:
Standard errors in parentheses.
*: Significant at 10 percent level.
[a]D75 = 1 if Year > 75, 0 if not.

Table 6
Time-Series Trends in the Variables

	1967	1969	1971	1973	1975	1977	1979	1981	1982
Hours of Work	18.8	18.6	17.6	17.7	17.2	18.2	20.6	20.4	19.3
Effective AFDC Tax Rate (%)	41.0	42.0	23.0	22.0	30.0	33.0	32.0	24.0	70.0
AFDC Guarantee[a] (Monthly)	161.0	156.0	153.0	149.0	147.0	141.0	131.0	113.0	111.0
Unemployment Rate	3.8	3.5	5.9	4.9	8.5	7.0	5.8	7.6	9.7
AFDC Participation Rate (%)	28.0	38.0	47.0	49.0	48.0	49.0	48.0	42.0	35.0

[a]For a family of four in 1967 dollars.

hours of work of female heads in the U.S. followed a quadratic pattern from 1967 to 1981, falling in the early years and rising in the later years. Neither the unemployment rate nor the AFDC guarantee followed such a pattern (the guarantee in fact fell continuously). However, the AFDC tax rate followed the quadratic pattern closely--falling at first and rising in the later years.[15] Hence, the source of the positive hours-tax-rate coefficient above is obvious.

The data on AFDC participation rates in the table provide a suggestion of a possible explanation of the result. Participation rates in AFDC rose into the mid-1970s and fell thereafter, showing again a quadratic pattern. Since the participation rates in the table are those for the entire population of female heads, not just eligibles, this pattern of participation rates may just be a result of movements in the breakeven level associated with the pattern of tax-rate movements. But it suggests that participation-rate changes may be related to the hours movements, for it has been shown in other work (Boland, 1973; Michel, 1980) that participation rates of female heads eligible for the AFDC program (i.e., with income below the breakeven level) rose dramatically in the late 1960s and early 1970s and peaked in the mid-1970s--the same time that hours stopped falling. This pattern has generally been ascribed to changing tastes for welfare (reduction of stigma, etc.),[16] but what it implies for present purposes is that there were movements onto the AFDC rolls in the early 1970s that may have had little to do with the tax rate reductions taking place at the same time. An exogenous upward shift in participation propensities will reduce hours of work in the population even if the tax rate does not change.

An admittedly weak test of this hypothesis is shown in the last two columns of Table 5, where the time trend is splined at 1975. The results are consistent with the hypothesis--when this spline is introduced, the tax rate coefficient becomes negative. Moreover, it is quite close (2.12 to 2.87) in the level and first-difference versions of the equation. Even more surprising, these values are quite close to the random effects, fixed effects, and between estimators shown in Table 2, which enclose these time-series estimates. Thus, although the time-series evidence here must be regarded as relatively weak by its nature, it is possible to reconcile all four of the hours-tax-rate estimates from the different estimators.

Given the marked differences in the sources of variation used in the different estimators, the band of tax-rate coefficients provided by the four is remarkably narrow (-1.52 to -3.17). Most important is that all are negative, indicating fairly strongly that the net effect on labor supply of increasing the tax rate in the AFDC program would be negative and of decreasing the tax rate would be positive. It is also interesting to note that the magnitudes of the coefficients are reasonably consistent with the simulations based upon structural estimates provided in Table 1. At the U.S. mean guarantee and tax rate (about 75 percent of the poverty level and 50 percent, respectively), Table 1 implies that a .25 reduction in the tax rate would have a positive effect on hours of work ranging from .18 to .70 hours of work per week for female heads (using both the .75 to .50 and .50 to .25 entries in the table). The range predicted by the tax rate coefficients that have been obtained from the four estimators is from .38 to .79 hours of work per week. In light of the very different origins of the two sets of estimates, they are surprisingly close. This also gives, by implication, a strong measure of support to the structural based simulations in Table 1.[17]

III. CONCLUSIONS

The goal of this paper has been, to a large extent, to perform basic spadework on work incentives in the U.S. AFDC system: is the sign of the partial correlation between hours of work of female heads and the tax rate in the system positive or negative? The argument of the paper has been that the answer to this question is more ambiguous than heretofore realized, and that such spadework is informative on the question. From a fairly comprehensive analysis of both cross-sectional and time-series variation in the hours of work and tax rates of female heads in the U.S., it can be reasonably concluded that the sign of the partial correlation is indeed negative, at least at current tax-rate and guarantee levels. In addition, the magnitude of the effect is well within the range predicted by structural estimates of income and substitution elasticities in the labor supply literature, elasticities only occasionally based upon estimates of AFDC tax rate effects or transfer-program effects of any kind.

The conformity of the estimates here and the structural estimates are in one sense less comforting for welfare-reform policy, however. If the elasticity estimates for men and married women are as reliable as they apparently are for female heads (and they are probably more reliable), the possibility of perverse tax-rate effects are eminently likely. Suppose, for example, that a transfer program were under consideration for husband-wife families in the U.S. and it were desired to set the tax rate so as to minimize work disincentives. The results in this paper imply that the tax rate for men should not be reduced below about 50 percent and that the tax rate for women should probably be set at 100 percent. Of course, work incentives are not the sole criterion for setting the tax rate--different tax rates distribute a given aggregate transfer differently across the population, for example--but the force of this finding is surely to suggest that the tax rate be set at a higher level than it would have been otherwise.

The theoretical ambiguity, though not necessarily the empirical results found here, applies to almost all other transfer programs. Housing allowance programs, medical subsidy programs, and other transfer programs for which eligibility is based upon low income or low consumption of the good in question lead to ambiguous changes in the relevant good when the benefit-reduction rate is altered. Likewise, "notches" in transfer programs, which arise when the benefit is reduced by more than 100 percent or when eligibility is suddenly lost by a small increment in income, have ambiguous effects on labor supply. There would seem to be a good deal of research necessary to empirically resolve the ambiguities in these various programs as has been done here for AFDC.

APPENDIX

Means of the Variables Used in the Regressions[a]

	All Years	1967	1969	1971	1973	1975	1977	1979	1981	1982
Hours worked per week	19.2	18.9	18.7	18.8	17.8	17.1	19.1	21.1	20.7	19.6
Guarantee[b]	275.3	311.3	298.9	289.9	290.6	290.2	276.7	256.0	212.3	252.4
Tax Rate	0.269	0.313	0.282	0.176	0.196	0.268	0.264	0.291	0.247	0.571
Age	36.4	37.7	38.1	37.5	36.4	35.5	35.7	36.0	35.5	34.1
Education	10.9	10.2	10.2	10.6	10.7	10.8	11.2	11.2	11.3	11.7
Race	0.654	0.671	0.674	0.678	0.614	0.606	0.665	0.659	0.635	0.663
No. children	2.11	2.39	2.32	2.30	2.24	2.12	2.03	1.91	1.84	1.80
Hourly wage	3.99	4.14	3.62	4.13	4.16	4.29	4.13	3.89	3.64	3.90
Other income	133.6	148.6	151.4	142.1	131.7	118.0	135.5	133.8	108.9	116.3
South	0.420	0.448	0.462	0.444	0.409	0.450	0.370	0.432	0.448	0.222
U	6.00	3.66	3.47	5.65	4.90	8.77	6.65	5.72	7.70	10.9

[a] All dollar amounts in 1977 dollars.

[b] For a family of four.

NOTES

1. This ambiguity is a general result of the fact that, if the budget set is nonconvex, most of the comparative statics of demand disappear--price effects need not be negative and income effects need not be positive. For welfare programs this was first pointed out explicitly by Levy (1979). See also Moffitt (1985a).

2. For this same reason the financing of the increased transfer is ignored. If the increased transfer is financed by an increase in taxes, there may be work disincentives elsewhere in the population. Here it is assumed that the donor population is aware of this but still wishes to increase aggregate transfers, i.e., it is not yet at its optimum. Note also that the application of this model requires that the recipient population be a small part of the tax base relative to the donor population. The AFDC fits this model because the female-head population in the U.S. contributes little to the tax base.

3. See Moffitt (1985a) for details on the simulation.

4. For references to the 1967 studies, see the background paper to this one (Moffitt, 1985c). For a review of the 1981 studies and for some additional time-series evidence, see Moffitt (1985b, 1985d).

5. Four such studies are those by Hausman (1981), Levy (1979), Masters and Garfinkel (1977), and Moffitt (1983). There are some other studies, but most are concerned only with the work-no-work decision (rather than with hours of work) or only with the AFDC participation decision. The paucity of studies on the AFDC program is in part a result of the work on the NIT experiments, which tended to crowd out research on the existing welfare system.

6. The guarantee level is almost entirely at the discretion of the state but the tax rate must be set within federal guidelines. The federal government sets a nominal tax rate (either 67 percent or 100 percent in the past) but states can use deductions, maximum-grant constraints, and other devices to alter the effective tax rate. As a result, effective tax rates have considerable variance across states at a given point in time. However, all tax rates change more or less in tandem when the federal government alters the nominal tax rate. See Fraker et al. (1985).

7. It is interesting to note that reduced-form equations of the type in (6) were estimated frequently in the NIT experimental literature (indeed, we are implicitly assuming that the variation in G and t across states and over time is independent variation, i.e., that a "natural" experiment has taken place). In the experiments, structural labor supply equations were generally estimated subsequent to the estimation of such reduced-form equations in order to determine if the reduced-form coefficients were consistent with the theory of labor supply and to obtain structural coefficients for prediction away from the means. Here, the order is somewhat in reverse: reduced-form equations are being estimated (for the first time, at least for the AFDC program) to determine whether they are consistent with previously estimated structural parameters.

8. They are presented in Fraker et al. (1985). These are the only years that AFDC program data are available with which tax rates and guarantees can be estimated.

9. Unfortunately, the usual procedures of dividing last year's earnings by last year's hours must be used to calculate wage rates, and of using last year's nonwage income, must be employed. The calculated values are inflated by a wage index to correspond to March of the following year.

10. But this does imply that the structural model in (4) could not be estimated with these data in any case. The difficulty in the data is that it is only known whether the individual was on AFDC in the previous year. Hours of work in the prior year are also known, but hours of work at the time of AFDC receipt is not known. This problem is no less severe in the Panel Study of Income Dynamics and the National Longitudinal Surveys.

11. In the early years of the CPS some of the smaller states were not identified separately but were instead grouped together. For these state groupings means taken over all the states in the group must be used. Weighted averages of the individual state AFDC tax and guarantee parameters are calculated for these groups. It should also be noted that some of the states have small female-head sample sizes in the CPS. This will be discussed further below.

12. There is an errors-in-variables problem here that is ignored. The variable means for each state are treated as if they are exact means from the same population over time when in fact they are only sample means from that population. See Deaton (forthcoming).

13. Estimators for the variances can be obtained from an analysis of variance of the OLS residuals (see, e.g., Johnston, 1984). A slight modification of the standard formulas is required here because of the unbalanced nature of the panel.

14. Oddly, the guarantee coefficient in column (4) is positive and significant. Some investigation of the cause of this result was undertaken, and it appears to be partly a result of noise in the year-to-year guarantee differences and partly a result of two or three year-to-year differences with unusually positive hours-guarantee correlations. For example, when the model is estimated on only the large states--thus increasing the precision of the estimates (in the smaller states there were few CPS observations), the guarantee coefficient falls to insignificance. Also, when the fixed effects model is estimated separately by year (excluding 1982), five of the eight guarantee coefficients are negative but three are positive.

15. The decline in tax rates in the early period was a result of the 1967 federal AFDC legislation. The increase in the later period was a result of increasing state restrictiveness in granting deductions and in allowing benefits for workers.

16. This is the conventional wisdom but it has never been shown formally. An alternative explanation is that the benefit-wage-rate ratio was rising over the same period.

17. The effects obtained in this study are not simply replications of the mean effects obtained in the structural models underlying Table 1. Some of the female-head studies underlying Table 1 were on an NIT, not AFDC, and those that were on AFDC (see n. 3) used the Michigan Study on Income Dynamics not the CPS, used only a single cross-section (both in 1975), and used highly structured estimating equations.

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