Labour Supply Analysis of an Income Maintenance Experiment:

Results from Mincome

Derek Hum and Wayne Simpson

Department of Economics

University of Manitoba

Winnipeg, Manitoba

R3T 2N2

CANADA

We wish to thank the Economic Council of Canada and the Social Sciences and Humanities Research Council of Canada for financial assistance, Robert Amano, Gobind Ganga and Carolyn Hill for research assistance, and seminar participants at the Economic Council of Canada, McMaster University and the University of Manitoba. We accept all responsibility for remaining errors.

1. Introduction

Empirical studies of labour supply behaviour have been prominent over the last quarter century, encouraged by the numerous microdata sets suitable for labour market analysis and a variety of policy applications. The microdata sets arise from two sources: nonexperimental data from large household surveys and data from five large-scale income maintenance experiments. Four of these experiments (the New Jersey, Rural, Gary, and Seattle-Denver experiments) occurred in the United States and have now been analyzed and reported. The fifth, the Manitoba Basic Annual Income Experiment or Mincome, was conducted in Winnipeg, Canada between 1974 and 1977. Because of a variety of administrative and financial problems, analysis of labour supply response in Mincome has been delayed. This paper provides an initial analysis of the labour supply response to the Mincome program.

The delays in analyzing Mincome, while frustrating at the time, now provide some unique opportunities. We are able to assess the methodology used to analyze previous experimental evidence. In this paper we review what has been learned about the analysis of labour supply with experimental evidence. We also assess the evidence from the experiments in the context of recent research using nonexperimental data. The inconsistency of labour supply results from nonexperimental data, particularly for married women, has been reconciled in recent years (Mroz, 1987). We consider the lessons of this reconciliation from an experimental perspective. Finally, we offer specific results from the Mincome experiment, both results comparable to previous experiments in the United States and new results for cross-wage elasticities and the role of children in the labour supply of married women.

In the next section we consider two approaches to the analysis of experimental panel data, the ANOVA model and the structural labour supply model. We evaluate these two approaches in terms of their contributions to the empirical evidence in the literature. Section 3 uses these models to analyze the Mincome data and to compare its results with other evidence from experimental and nonexperimental data. Section 4 provides a summary and concluding remarks.

2. Analysis of Labour Supply with Experimental Panel Data

Panel surveys provide data of the form

0

for **N** individuals in **T** time periods. One of the advantages of panel data is that we are able to consider individual-specific effects, $\mathbf{á}_i$, and time-specific effects, $\mathbf{ç}_t$, as well as a standard disturbance term, $\mathbf{î}_{it}$, to explain $\mathbf{y}_{it}$ which, in our case, is labour supplied (annual hours worked):

0

The unobserved effects may be analyzed either as fixed effects for each time period in the sample or as random effects drawn from the population of individual effects (Hsiao, 1986, 41-43). If we analyze them as fixed effects we use the within-groups estimates derived from the regression:

0

where

0

Under the usual assumptions about $\mathbf{x}_{it}$ and $\hat{\mathbf{\imath}}_{it}$ within-group or fixed-effect estimates, [$\hat{\mathbf{a}}_w$,$\tilde{\mathbf{a}}_w$], can be obtained from Ordinary Least Squares regression of equation (3) where the fixed time effects, $\tilde{\mathbf{a}}_w$, are estimated from dummy variables.[1]

We can also obtain the between-groups estimates from the regression model:

(4) 0

where time effects are now eliminated. Note, however, that the between-groups estimator, $\hat{\mathbf{a}}_b$, is biased if

0

This bias arises from correlation between unobservable individual-specific effects and observable determinants of labour supply and was an early concern (Garfinkel, 1973; Greenberg and Kosters, 1973). Random-effect estimates can then be obtained as a weighted average of the within-groups and between-groups estimates obtained from a regression model of the form:

[1] Most panel surveys follow a large number of individuals over a few time periods so that it is practical to estimate the fixed time effects, but not the fixed individual effects, by dummy variables.

0

(Hausman and Taylor, 1981). Unless **è**=0, however, the random effects estimator, **â**$_{r}$, will be biased because of condition (5). Thus only the within-groups or fixed-effects estimator from equation (3) will be unbiased in general.

2.1 ANOVA Models

The simplest and most direct method to estimate the effects of an income maintenance experiment on labour supply is to specify an analysis-of-variance (ANOVA) model involving one or more dummy variables to distinguish the treatment group(s) from the control group. The model would then be

0

where

0

or, for several distinct plans,

0

This model is a common starting point for the income maintenance experiments, although most studies have simply let $\mathbf{y}_{i}$ be pre-experimental labour supply and $\mathbf{y}_{it}$ be average labour supply during a portion of the experiment, dropping the time effects **dã** (Hall, 1973, for New Jersey; Ashenfelter, 1978, for the rural experiment; Robins and West, 1978 and 1980, for Seattle-Denver).

A modest extension is to combine qualitative and quantitative variables to give an analysis-of-covariance (ANCOVA) model:

0

where $\mathbf{x}_{it}$ includes quantitative variables. An example is the spline series used to evaluate the New Jersey experiment (Watts and Rees, 1976):

0

where $(G_i - G_0)$ and $(t_i - t_0)$ are the differences between assigned and median guarantee levels and tax rates, respectively. Control variables are often introduced to capture differences in individual circumstances that determine labour supply response. For example, if experimental assignment insures that the treatment, T_i, is randomly allocated and hence uncorrelated with other determinants of labour supply response, then inclusion of such control variables is unnecessary (Hausman and Wise, 1985). But since treatments were not assigned randomly, control variables may be needed to obtain unbiased estimates of labour supply response (Keeley and Robins, 1978). We return to this issue in the next section.

2.2 Structural Labour Supply Models

One problem with results from ANOVA models is that they cannot be easily generalized for social policy analysis because the estimates refer to the specific effect of experimental programs whose features are unlikely to correspond to actual proposals (Burtless, 1986). An approach which overcomes this difficulty estimates a specific structural model of labour supply whose results can be generalized to a variety of policy applications (Keeley, 1981).

The conventional starting point is a set of labour supply functions for two working adults in the household:

0

where h_1 and h_2 are hours worked and w_1 and w_2 are the wages of each adult and y is unearned household income. Cross-sectional data from a variety of microdata sets, both experimental and nonexperimental, have been used to estimate linearized versions of equation (9):

0

(See Keeley, 1981, Killingsworth, 1983, Pencavel, 1986, Heckman and Killingsworth, 1986, and Hum and Simpson, 1991, for surveys of this literature.)

Several problems arise in estimating equation (9a). Initially, attention concentrated on the treatment of nonworkers and sample selection bias (da Vanzo et al, 1976; Heckman, Heckman, 1976 and 1980) as well as nonlinear budget constraints (Hausman, 1981, Hausman and Ruud, 1984), particularly for married women. It now appears that the most serious problems involve the treatment of unobserved person-specific fixed effects (Mroz, 1987). That is, we should specify

0

corresponding to equation (4) for the between-groups estimator. Bias then arises under condition (5).

With panel data, however, these fixed effects can be eliminated by examining changes in individual labour supply, the equivalent of the within groups estimator (2) for structural labour supply models. The Slutsky decomposition of equation (9) into a compensated wage, or substitution, effect and an income effect is

0

and the corresponding decomposition for the cross-wage effect is

0

These decompositions can be used to decompose the total differential of the labour supply function:

0

where $\mathbf{dw_a}$ is the change in the adult's wage rate arising from a tax change and $\mathbf{dy}$ is the change in household income from a change in the tax-transfer system. The crucial behavioural parameters are then easily estimated--namely, the compensated wage elasticity,

0

the income elasticity,

0

and the compensated cross-wage elasticity,

0

Equation (11) is essentially the household version of the labour supply model developed by Keeley et al (1978) to analyze the Seattle-Denver experiment.

Estimation of equation (11) with experimental data merely requires us to know the change in wages and household income induced by experimental treatments.[2] Then, with estimates of these elasticities, predictions of labour supply behaviour merely require knowledge of the effect of any proposed program on wages and household income. For a negative income tax program involving an income guarantee, $\mathbf{G}$, based on family size and composition, and a marginal tax rate, $\mathbf{t}$, these calculations are particularly straightforward. If $\mathbf{y_0}$ is the level of household income and $\mathbf{t_0}$ is the tax rate before the program, then

0

Of course, labour supply response can only be measured if there is sufficient variation in after-tax wage rates and unearned income. The absence of estimates of the conventional labour supply parameters from nonexperimental panel data reflects the lack of variation in these variables in such data. What variation there is in after-tax wages and unearned income is largely anticipated variation arising from life cycle behaviour, which is not suitable for estimating static labour supply elasticities (Killingsworth, 1983, 214-20) nor, therefore, for assessing the labour supply response to unanticipated changes in tax-transfer policy. The income maintenance experiments, by introducing large unanticipated changes in tax rates and income guarantee levels to selected participants, provide a unique source of data to overcome some of the serious problems facing the estimation of static labour supply behaviour.

3. Empirical Evidence

Experimental data provides a "strong test of the robustness of the results" from nonexperimental data (Mroz, 1987, 795). But accompanying this opportunity are special problems arising from social experimentation and from the design of the income maintenance experiments. In this section we first consider these special problems before turning to the experimental results using both ANOVA and structural labour supply models.

The analysis of experimental data is still new, but two potentially important problems have been identified with respect to the income maintenance experiments: nonrandom selection and nonparticipation. Nonrandom selection of the treatment group arises because the assignment process favours inexpensive observations over expensive ones to improve estimation reliability when the budget for payments to participants is limited (Conlisk and Watts, 1969). Since the cost of a treatment depends on household income, however, bias arises because labour supply is

---

[2] If treatments are allocated nonrandomly, then the model may also need to contain control variables to correct for nonrandom assignment of experimental treatments as discussed in the previous section.

correlated with this assignment variable. Families supplying little labour prior to the experiment are likely to have low family income and are therefore less likely to be allocated to generous plans because of the expense involved.

Consider this argument for the simplest ANOVA model represented by equation (7) for two time periods. Keeley and Robins (1978) specify the nonrandom assignment to depend directly on labour supply as follows:

0

so that the treatment, $\mathbf{T}_i$, is correlated with the error term, **î**$_{i1}$, to produce bias in the estimated effect of the treatment on labour supply in equation (14). Further examination of equation (14) reveals an additional potential source of bias ignored by Keeley and Robins. Since pre-experimental labour supply depends upon the unobserved individual effect, **á**$_i$, experimental assignment will also depend upon this effect. Thus nonrandom assignment, as represented by the model in equation (14), reintroduces the bias associated with the correlation between this unobservable effect and observable determinants of labour supply (in this case, $\mathbf{T}_i$). Recent evidence suggests that this source of bias may be quite serious (Mroz, 1987) so that control for nonrandom assignment takes on added importance.

Keeley and Robins (1978) and Keeley (1981, 124-5) argue that the only way to correct, at least partially, for nonrandom assignment bias is to include all assignment variables in the model:

0

Since assignment varies according to family size and composition as well as pre-experimental income, the number of assignment categories will be large, seriously impairing the ability of the model to generate statistically reliable estimates (Keeley, 1981, 130-1). Thus some compromise between running a separate ANOVA model for each assignment category, as in equation (15), and ignoring assignment effects completely must be struck.

Individuals may also decide not to participate in the experiment because it is not sufficiently attractive. The payment is determined by the treatment which consists of the income guarantee and tax rate:

0

so that payments depend upon pre-experimental household income, **Y**, relative to the treatment break-even level, **B**. Families with incomes below **B** will face different tax-transfer system during the experiment with respect to pre-experimental labour supply decisions, the classic case for marginal analysis of behavioural response to a shifting budget constraint. Families with incomes above **B** for prevailing (pre-experimental) labour supply, however, will not be affected by the experimental treatment at the margin. Thus we should attempt to distinguish between the labour supply response of families above and below the break-even level at the beginning of the experiment.

Ashenfelter (1980) shows that an approximate condition for participation for household member a is

0

where ç_a^s is the compensated wage effect defined in equation (12a). This condition is always satisfied for families below the break-even, as expected, but condition (17) also indicates that families above the break-even point may participate, particularly when they are near the break-even point such that **y-B** is small.

The analysis suggests two simple measures of break-even status to be used as control variables in assessing labour supply response (Keeley et al, 1978):

0

Keeley (1981, 131) argues that the issue of differential response by break-even status is complicated by nonrandom assignment, however, because both break-even status and labour supply depend upon pre-experimental income and family size, which affect the assignment. Thus break-even status should be interacted with the experimental treatment variables in the same manner as other assignment categories in equation (15). In practice, however, the evidence suggests that participation is determined by break-even status rather than choice (labour supply response) because labour supply response is small (Ashenfelter, 1980, for Seattle-Denver; Sabourin, 1985, for Mincome). Thus, if $\text{ç}_a^s = 0$ in equation (17) then only individuals in households below the break-even level participate in the experiment and break-even status has no effect on labour supply response.

One additional aspect of nonparticipation is attrition. Decisions to leave the experiment may depend upon the financial incentives to remain (the payment). From equation (16) we see that the payment is directly related to break-even status, **B-Y**, and the tax rate:

0
so that attrition may be correlated with labour supply response. The only evidence on this question by Robins and West (1978) for the Seattle-Denver experiment suggests that attrition declines as payments increase, as expected, and that larger substitution effects and smaller income effects are obtained when attrition bias is corrected.

3.1 The Mincome Data

The Mincome experiment was confined to residents of Manitoba. The original design called for samples drawn from the city of Winnipeg (the urban dispersed sample), several smaller communities (the rural dispersed sample), and Dauphin, where all families were eligible for one specific program (the saturation site sample). Our analysis concentrates on the largest sample, the intact[3] urban dispersed sample, which corresponds most closely to the classical experimental design model followed in the U.S. experiments. Problems with the rural dispersed sample eventually led to its abandonment, while the saturation site sample presents special evaluation problems (such as macro or community effects) not encountered in the U.S. experiments.

The Mincome experiment limited payments to three years, 1975-77. The intact urban dispersed sample consists of 1,187 households, of which 650 (55%) are double-headed, 364 (31%) are female single-headed, and 173 (15%) are male single-headed. Thus the experiment is similar in scale to the early U.S. experiments (New Jersey and Gary) but smaller than the Seattle-Denver experiment.[4] The households were assigned to the control group (612 or 52%) or to one of eight plans as shown in Table 1. The guarantee levels indicated for each plan are for a double-headed family of four; the guarantee level was adjusted for family size. Only a flat tax rate was offered to treatment groups, as in the early U.S. experiments.

[Table 1 about here]

The guarantee levels in Mincome are comparable to the New Jersey and Gary experiments. The most generous guarantee of $5800 for a family of four corresponds to the low-income cutoff calculated by Statistics Canada (Podoluk, 1968) adjusted for

---

[3] The intact sample excludes families whose marital status changed during the experiment. For an analysis of the effect of the experiment on marital status, see Choudhury (1989).

[4] Note, however, that the experiment was conducted over a more homogeneous population so that stratification by race was not deemed to be necessary to evaluate labour supply response. In the Seattle-Denver experiment, racial differences (white, black, and Chicano) were extensively explored, which reduced sample size.

inflation while the least generous guarantee of $3800 for a family of four corresponds to two-thirds of the low-income cutoff. Eligibility criteria are also similar to the American experiments. Only households with able-bodied heads under 58 years of age with household incomes below $13,000 (for a family of four) were admitted.[5] One notable difference is that Mincome was the only experiment to include single individuals, both male and female.

Variations in labour supply, represented by hours worked on an annual basis, are presented in Table 2 for three groups: males, females in double-headed families (wives), and females in single-headed families (single female heads). These are the main sex/marital status categories studied in the labour supply literature. The evidence in Table 2 is difficult to interpret because there are several different plans, which should have different effects on labour supply, and because there is very likely substantial noise in the data. The data in Table 2 do show declining hours worked from the first (pre-experimental or baseline) year to the other years for most plans, but the same pattern is also evident for the control group. Thus, it is difficult to determine from Table 2 whether the pattern in hours worked is affected by the experimental treatment.

[Table 2 about here]

3.2 ANOVA Models

The basic evidence in Table 2 can be evaluated in various ways using simple ANOVA models for panel data described in section 2.1, in particular equations (7, 7a, 7b) and (8, 8a). The ANOVA model results are presented in Table 3 for men, wives, and single female heads. The first column of Table 3 for each group simply compares the mean difference in annual hours worked between the experimental and control groups. The difference is negative, as expected, and statistically significant (at the 5% level) for males indicating a decline in work effort as a result of income maintenance support.[6] Males in the experimental group work 92 hours less per year on average. The experimental effect is also negative for wives, indicating a statistically insignificant reduction of 25 hours per year, and for single female heads, indicating a significant reduction of 100 hours per year.

[Table 3 about here]

---

[5] For a review of the eligibility criteria for the American experiments, see Basilevsky and Hum (1984) or Burtless (1986).

[6] The unambiguous prediction of a decline in hours worked from income maintenance experimental programs assumes that the experimental plan raises the income guarantee and raises the tax rate of all participants (which is true on average, but certainly not for all, Mincome participants), and that the income effect is negative and the compensated wage effect is positive as in standard consumer theory.

The experimental effect remains negative but declines and becomes insignificant when fixed-time effects are included in the second column of Table 3. Accounting for separate plan effects with dummy variables (equation (7b)) or by a spline series yields generally insignificant experimental effects provided that the time effects are included. Hence the simple evidence from ANOVA models in Table 3 suggests that the Mincome experiment reduced annual hours worked for all groups, but that the effects were small and not statistically significant.

In Table 4 we introduce additional explanatory variables to control for nonrandom assignment and participation behaviour. We include the five normal income cells and the family size index to account for assignment decisions based on normal income and family size and composition as in Keeley and Robins (1978). These assignment variables are allowed to interact with the treatment variable, as in equation (15). We also include the variables **FABOVE** and **EARNABV** defined in equation (18) to account for the effects of break-even status on participation. To account explicitly for any effects of attrition on labour supply response we include the observations on those households who left the experiment and include dummy variables, **ATT3** and **ATT4**, to denote when they left.

[Table 4 about here]

The results in Table 4 are similar, in terms of the experimental effect, to those in Table 3. The first two columns for each group (men, wives, and single female heads) include the assignment and break-even status variables but exclude those who left the experiment. The first column follows equation (15), interacting the assignment and break-even status variables with the treatment variable, but the assignment and break-even variables and the experimental effect are all insignificant.[7] In the second column, the assignment and break-even status variables are introduced independently to facilitate comparison of the experimental effect with Table 3. Hours worked for males decline by 15 hours annually in both Tables 3 and 4 when time effects are included. Hours worked for wives decline by 15 hours in Table 4 and 13 hours in Table 3. Hours worked for single female heads decline by 49 hours in Table 4 and 47 hours in Table 3. Experimental effects remain statistically insignificant.

The third column for each group in Table 4 includes participants who left the experiment after the pre-experimental year.[8] The experimental effect remains insignificant, although

---

[7] The assignment and break-even variables are insignificant individually and as a group. Note that the small positive coefficient on s_1 cannot be interpreted as the experimental effect because of the presence of interaction terms.

[8] Participants who left the experiment before the first experimental observation could not be included because at least

somewhat larger than in the first three columns. The attrition variables are insignificant for females in all cases and for males except attrition after the second year.

Our results are compared with several published summaries of the results from the American experiments in Table 5. Labour supply response is generally smaller in Mincome, especially for women, although the response is similar to the results from the Gary experiment. Although our results are not strictly comparable to the American experiments because of the inclusion of unattached individuals[9] as well as slightly different eligibility criteria, they do not differ markedly from the lower responses obtained in the other experiments.

[Table 5 about here]

3.3 Structural Models

In this section we present estimates of structural labour supply models repesented by equations (9) and (9a).

Reviews of the empirical literature identify three major sources of bias in the estimation of labour supply models: unobserved person-specific fixed effects, nonlinear tax regimes, and sample selection bias (eg., Killingsworth, 1983). Experimental data allows us to address two of these problems, the unobserved person-specific effects and complex tax regimes. Since the problem of selection bias, arising from the exclusion of nonworkers, has been studied extensively in the last decade, experimental data provides an excellent opportunity to correct for the three major sources of bias identified in the literature over the past twenty years.

The problem of unobserved, and hence omitted, individual characteristics can be addressed by using the equivalent of the within-groups estimator for the neoclassical labour supply model, as set out in equation (11). The differential changes in equation (11) are then interpreted as finite differences between experimental panels. For single heads, of course, $\mathbf{dw_2=0}$, and the labour supply behaviour of these heads is normally estimated separately from double-headed families.[10]

The problem of complex tax regimes is simplified by the presence of a single flat-rate tax change induced experimentally. Provided that the tax regime remains unchanged otherwise, as it did in Manitoba between 1974 and 1977, and provided that the structure of wages is stable, a matter that we consider below, the only variation in after-tax wages arises in the experimental group. Since this tax rate change generates a single marginal tax

---

two observations are required to estimate the within-groups model, equation (7) or (8).

[9] The sample sizes for individual males and females are too small to support separate analysis of these groups.

[10] In our case, single male heads are not analyzed because of the small sample size (n=56 for those completing all four panels).

rate for each household, bias arising from the dependence of the tax rate on hours worked is alleviated.[11] For nonexperimental (pre-experimental and control group) observations we linearize the budget constraint at the prevailing Manitoba marginal tax rate to compute virtual income, **y** (Hall, 1973). This is the most common method of dealing with nonlinear budgets (Killingsworth, 1983; Mroz, 1987).

We first estimated eight wage equations that varied according to specification (the inclusion of age or a direct measure of work experience and the inclusion of second-order terms for education and age) and estimation method (OLS versus Heckman's (1979) method to adjust for sample selection bias). The wage equations were estimated over all four years (the pre-experimental year, 1974, and the three experimental years, 1975-77) with dummy variables to capture time effects. There was evidence of selection bias for the OLS estimates in every case. A Chow test for the stability of the wage equations over time, performed for each specification, could not reject the hypothesis of a stable wage structure throughout the experiment.[12] This was a fortunate result because it suggested that after-tax wage changes in the data were primarily attributable to experimental tax changes and not changes in before-tax wages.[13]

To estimate the labour supply equations we use only the wage equations corrected for sample selection bias. The imputed wages from these four equations are used to estimate four structural labour supply models based on equation (11):

i) a strict version of equation (11) given by:

0

ii) a similar equation, common in analysis of the U.S. experiments (Hall, 1973; Ashenfelter, 1978; Robins and West, 1978 and 1980), which uses the pre-experimental survey as the basis for comparison instead of the average of all panels as above:

---

[11] In the New Jersey experiment there were changes in the state tax-transfer system during the experiment. In the Seattle-Denver experiment, a declining marginal tax rate was assigned to part of the sample. We are not aware of any careful investigation of the effect of these complications on labour supply behaviour.

[12] The F-test is not strictly valid for the wage equations adjusted for selection bias because the estimation procedure is known to generate heteroskedastic errors (Heckman, 1979). The uniformly low F-values across all equations give no indication, however, of temporal instability in wages.

[13] These auxiliary regression results are not reported here but are available from the authors.

0

where z_i is a set of control variables from the pre-experimental panel, to be delineated below. Note that these control variables do not appear in equation (21) because they have no effect in that specification (Hum and Simpson, 1991, chapter 8, note 3).

iii) a version of equation (11) restricted to comparisons between the pre-experimental year and the mid-year of the experiment:

0

Keeley et al (1978), for example, finds that the strongest, and most accurate, labour supply response in the Seattle-Denver experiment occurs in the mid-year. "Start up" and "wind down" effects in the other years may bias the estimates of permanent response. Initial estimates for Mincome (Simpson et al, 1987) agree with Keeley et al's results.

iv) a version of equation (11) using experimental hours as the dependent variable:

0

Keeley et al (1977, 1978) argue that equation (23) will generate biased estimates because the change in virtual income is measured using actual pre-experimental labour supply rather than permanent labour supply. This bias may be corrected by including pre-experimental labour supply, y_{i1}, as a control variable. They then estimate equation (24) by Tobit regression.

We have included this latter model because it has been quite influential both as a method of estimating structural labour supply models from experimental data and as a source of evidence on the labour supply behaviour of low-income households. We are not comfortable with the approach taken by Keeley et al, however, for several reasons. First, they do not provide a rigorous justification for their reformulation of the basic model given by equation (24). Their *ad hoc* adjustment may have other unintended effects on the estimates. Second, there is no justification for the use of the Tobit model. Pre-experimental labour supply could simply have been included as a control variable in equation (23) to meet their concerns, particularly since Mroz (1987) rejects the Tobit estimator for labour supply models for married women.[14]

In addition to the control variables from the pre-experimental survey, we add a variable to reflect changes in

[14] We recognize that, since the dependent variable in equation (23), or equations (21) or (22), is the difference between two limited variables ($y_{i3}\$0$, $y_{i1}\$0$), specification of the distribution of the error term and a consistent estimation technique may be difficult. We are not convinced, however, that Keeley et al's (1978) solution is better that simply ignoring this problem and using OLS in equations (21), (22), and (23). Mroz's (1987) rejection of the Tobit estimator reinforces our concerns.

family circumstances: **dchld** is the change in the number of pre-school children. There is consistent evidence (most recently Jakubson, 1988, for panel data) that the presence of pre-school children has a significant deterrent effect on labour supply response, particularly for wives. This variable has not been included in the evaluation of the other experiments to our knowledge.

The control variables fall into two groups:

i) those variables to deal with problems of nonrandom assignment and participation discussed earlier--namely, normal income cell, **FABOVE**, and **EARNABV**)--and

ii) control variables used in other studies (particularly, Keeley et al, 1978)--namely, pre-experimental hours, age, family size, number of preschool children, social assistance received, and the break-even level of the assigned plan--to facilitate comparison with earlier results.

These variables are defined in Table 6, which presents the results of the structural labour supply models represented by equations (21), (22), (23), and (24).

[Table 6 about here]

Estimates are provided for married men, wives, and single female heads in Table 6.[15] The compensated wage effects (the coefficients of $\mathbf{dw_a}$ and $\mathbf{dw_b}$ in Table 4) are often negative, particularly for women, contrary to expectations, as in the New Jersey experiment (Watts and Rees, 1976). As we move from equation (21) to equations (22), (23), and (24), the compensated wage effect becomes positive and significant, as expected, for married men but not for married or single women. Income effects are small, often insignificant, and generally negative as expected and as found in general in the empirical literature.

We also estimate the cross-wage effect in our model. The estimates are generally insignificant, although the estimate is positive and significant for equation (21) for wives.

Changes in the number of preschool children in the family, **dchld**, is consistently significant. Additional preschool children increase the labour supply of husbands and reduce the labour supply of wives. Indeed, the effects of children on labour supply is much stronger than the wage and income effects.[16]

---

[15] Single male heads are excluded because of the small sample size. Results for single male heads are provided in Hum and Simpson (1991).

[16] To the argument that the sample size of the experiment is too small to measure labour supply behaviour accurately, we respond that it is certainly large enough to measure family composition effects on labour supply. This suggests to us that the sample size is too small only in the sense that the labour supply response is quite weak and high variable.

Pre-experimental labour supply has a large and significant effect in equations (22), (23), and (24). Given our misgivings about the exact role of pre-experimental labour supply as a control variable, we place greater confidence in the results for equation (21), where the effects of pre-experimental control variables are properly eliminated. Note, for example, that the compensated wage effect for husbands changes dramatically as we move from equation (21) to equations (22), (23), and (24).

In Table 7, we present the labour supply elasticity results calculated from our estimates in Table 6 and from similar results not reported here that use alternative wage equations.[17] The previous results from experimental evidence have tended to produce smaller wage elasticity estimates for all groups relative to the nonexperimental evidence. Our results reinforce this conclusion but with one important difference. Our analysis indicates that the wage elasticity estimates remain sensitive to the specification of the wage and labour supply equations and the method of estimation. For husbands, the compensated wage elasticity varies from -0.4 for equation (21) to 0.3 for equations (23) and (24). For wives, they vary from -1.9 to -0.4 and, for single female heads, from -0.5 to 0.8. Many of the estimates are inexplicably negative, although negative compensated wage elasticities have been found in studies of the American experiments (Keeley, 1981). The income elasticity estimates are uniformly small and, at least for equation (21), more uniformly negative as expected.

We also provide unique estimates of the compensated cross-wage elasticity for husbands and wives. The estimates are generally insignificant for husbands, although they are generally positive and significant for equation (21). For wives, the estimates for equations (21) and (22) are large, positive and significant, but they are insignificant for equation (23) and (24).

Table 8 compares our results with published summaries of the results from the American experiments. The experiments, in contrast to the nonexperimental evidence, provide relatively low elasticity estimates of labour supply response. In particular, the labour supply estimates from Mincome are much smaller than corresponding evidence from the Mincome pre-experimental data alone (Prescott et al, 1986; Simpson et al, 1987). Yet our results suggest that precise estimates of labour supply response from experimental data remain elusive because of the sensitivity of the estimates to model specification and estimation method. Whether this imprecision could be solved by superior experimental design cannot, as far as we can see, be determined from the experimental evidence available.

[17] These results are available from the authors.

4. Conclusion

The Manitoba Basic Annual Income (Mincome) Experiment concluded a series of five large-scale income maintenance experiments conducted between 1968 and 1977. In this paper we have tried to summarize the methodologies that have evolved to analyze labour supply response in these experiments and to assess their contribution to our understanding of the experimental results. The methodologies fall roughly into two categories: classical ANOVA models to measure experimental response and structural labour supply models to measure substitution and income elasticities.

Our ANOVA results suggest that the labour supply response in Mincome was small, implying only a 1% reduction in hours worked for husbands, 3% for wives, and 7% for single female heads. These results are consistent with the smallest responses obtained in the experiments in the U.S. (Table 5). Our structural model results provide additional evidence of small substitution and income elasticities, consistent with results from the U.S. experiments (Table 8) and recent evidence from nonexperimental data (Mroz, 1987). One anomaly in our results is that we obtain negative substitution elasticities for women, particularly wives.

Our paper offers three new results. First, we examine the sensitivity of the results from our structural labour supply model to alternative plausible specifications of the wage rate and the labour supply model. We find considerable variation in the labour supply elasticity estimates, implying that precise estimation of labour supply response using structural models remains elusive. Second, we estimate cross-wage effects in a family labour supply model. The cross-wage elasticities are generally insignificant but, in our preferred model (Equation (21)), the elasticity is positive and significant for both husbands and wives for all specifications of the wage equation. Finally, we find that changing family circumstances during the experiment was an important factor in labour supply response. An increase in the number of preschool children in the family during the experiment reduced labour supply significantly for wives and increased it significantly for men. Indeed, this effect would seem to dominate any experimentally induced labour supply response and merit further careful examination.

References

Ashenfelter, Orley (1978) "The labor supply response of wage earners" in J. Palmer and J. Pechman (eds) *Welfare in Rural Areas*. Washington, D.C.: Brookings Institution.

Burtless, Gary (1986) "The work response to a guaranteed income: a survey of experimental evidence," in Alicia Munnell (ed.) *Lessons from the Income Maintenance Experiments*, Federal Reserve Bank of Boston and Brookings Institution, 22-52

Garfinkel, Irwin (1973) "On estimating the labour supply effects of a negative income tax" in Glen Cain and Harold Watts (eds) *Income Maintenance and Labor Supply*. New York: Academic Press, 205-64.

Greenberg, David, and Marvin Kosters (1973) "Income guarantees and the working poor: the effect of income-maintenance programs on the hours of work of male family heads" in Glen Cain and Harold Watts (eds) *Income Maintenance and Labor Supply*. New York: Academic Press, 14-101.

Hall, Robert (1973) "Wage, income, and hours of work in the U.S. labor force," in Glen Cain and Harold Watts (eds) *Income Maintenance and Labor Supply*. New York: Academic Press, 102-62.

Hausman, Jerry, and William Taylor (1981) "Panel data and unobservable individual effects," *Econometrica 49*, 1377-98.

Hausman, Jerry, and David Wise (1985) *Social Experimentation*. Chicago: University of Chicago Press.

Hsiao, Cheng (1986) *Analysis of Panel Data*. Cambridge, UK: Cambridge University Press.

Keeley, Michael, and Philip Robins (1978) "The design of social experiments: a critique of the Conlisk-Watts assignment model," Research Memorandum 57, Center for the Study of Welfare Policy, Stanford Research Institute, Menlo Park, California.

Mroz, Thomas (1987) "The sensitivity of an empirical model of married women's hours of work to economic and statistical assumptions," *Econometrica 55*, 765-99.

Robins, Philip, and Richard West (1978) "Participation in the Seattle and Denver income maintenance experiments and its effect on labor supply," Research Memorandum 53, Center for the Study of Welfare Policy, Stanford Research Institute, Menlo Park, California.

________________________________ (1980) "Program participation and labor supply response," *Journal of Human Resources 15*,499-523.

Watts, Harold, and Albert Rees (1976) *The New Jersey Income-Maintenance Experiment, Volume II: Labor-Supply Responses*. New York: Academic Press.

Table 1. Mincome Experimental Assignment.

Plans		Dbl-head hhlds	Single-head households		All households	
G[a]	t					
3800	0.35	29	21	19	50	48
4800	0.35	39	8	25	47	64
3800	0.50	41	17	43	58	84
4800	0.50	59	6	15	65	74
5800	0.50	35	9	21	44	56
3800	0.75	40	20	29	60	69
4800	0.75	24	7	15	31	39
5800	0.75	35	3	15	38	50
Total experimentals		302	91	182	393	484
Total controls		348	82	182	430	530
Total households		650	173	364	823	1,014

[a] Figures quoted are for a family of four (two adults and two children). Actual plan guarantees are adjusted according to family size.

Table 2. Mean Annual Hours Worked for Men, Women in Double-headed families (Wives) and Single Female Heads by Plan and Year.[a]

		1974	1975	1976	1977
Men:					
G	t				
3800	0.35	1,653.2	1,663.1	1,547.7	1,664.4
4800	0.35	1,627.3	1,437.5	1,412.4	1,557.8
3800	0.50	1,507.5	1,191.0	1,213.6	1,114.8
4800	0.50	1,888.5	1,681.9	1,577.0	1,489.4
5800	0.50	1,900.6	1,581.2	1,341.3	1,521.7
3800	0.75	1,499.2	1,409.9	1,506.1	1,532.3
4800	0.75	1,798.5	1,670.1	1,605.2	1,600.9
5800	0.75	1,771.4	1,658.8	1,513.9	1,570.5
Controls		1,720.5	1,679.3	1,567.9	1,582.0
All men		1,711.0	1,580.5	1,704.0	1,472.0
Wives:					
G	t				
3800	0.35	511.6	545.4	354.1	351.0
4800	0.35	502.3	559.4	438.0	332.8
3800	0.50	443.2	530.5	385.5	277.2
4800	0.50	415.2	529.7	416.5	529.8
5800	0.50	523.2	478.0	444.1	351.0
3800	0.75	794.8	442.9	454.5	370.8
4800	0.75	526.0	486.3	635.3	469.6
5800	0.75	526.0	593.1	460.0	703.3
Controls		561.0	625.7	587.0	682.6
All wives		542.9	559.1	489.8	516.2
Single Female Heads:					
G	t				
3800	0.35	1,012.1	1,058.7	876.4	969.5
4800	0.35	1,445.8	1,247.3	1,202.3	1,150.7
3800	0.50	1,492.2	1,285.0	1,017.8	1,006.9
4800	0.50	1,323.4	1,342.7	696.3	770.4
5800	0.50	1,532.5	1,033.9	1,241.9	1,167.3
3800	0.75	1,058.0	1,103.0	952.6	1,055.9
4800	0.75	1,510.7	1,522.1	1,340.6	1,571.0
5800	0.75	1,353.3	1,279.1	1,169.7	1,136.8
Controls		1,088.3	982.4	873.0	926.5
Total SFHs		1,221.9	1,153.8	1,002.4	1.027.8

[a] 1974 refers to the pre-experimental or baseline survey; 1975

consists of the second to fourth surveys generally conducted during that year at roughly four-month intervals; 1976 consists of the fifth to eighth surveys; 1977 consists of the ninth to eleventh surveys. Since interview dates vary for each respondent, data is prorated to a 365-day period to estimate annual hours worked.

Table 3. Regression Estimates of Annual Hours Worked for ANOVA Models (Equations 7, 7a, 7b, 8, and 8a).

Variables:	Men (n=1,284)	Wives (n=1,172)	Single female heads (n=584)
Eqn(7,7a)-no time effects:			
Intercept	46.1*	12.2	53.6*
s_1 = T	-92.2	-25.3	-100.4*
F, R^2	15.0*, 0.01	1.4, 0.001	7.7*, 0.01
Eqn(7,7a)- with time effects:			
Intercept	123.7*	25.1	99.4*
s_1	-14.6	-13.3	-47.3
d_1(1975)	-108.2*	-3.3	-64.2
d_2(1976)	-170.4*	-45.6	-121.7*
d_3(1977)	-187.1*	-25.8	-110.7
F, R^2	10.8*, 0.03	1.0, 0.003	3.2, 0.02
Eqn (7,7b):			
Intercept	123.7*	25.1	99.4*
P_1(G=3800,t=0.35)	-16.4	-28.0	6.8
P_2(G=4800,t=0.35)	13.3	1.6	-54.9
P_3(G=3800,t=0.50)	0.0	9.4	-88.2
P_4(G=4800,t=0.50)	-27.0	-0.2	-71.8
P_5(G=5800,t=0.50)	-100.7*	-43.5	-58.3
P_6(G=3800,t=0.75)	29.9	-57.0	-26.2
P_7(G=4800,t=0.75)	16.9	15.2	60.7
P_8(G=5800,t=0.75)	0.2	5.1	-47.7
d_1(1975)	-108.2*	-3.3	-64.2
d_2(1976)	-170.4*	-45.6	-121.7*
d_3(1977)	-187.1*	-25.8	-110.7
F, R^2	4.4*, 0.04	0.6, 0.005	1.36, 0.03
Equation (8,8a):			

Intercept	123.7*	25.1	99.4*
s_1	-12.7	-0.5	-95.2
s_2	0.0	0.0	0.0
s_3	-148.7	51.7	-311.2
s_4	-0.1	-0.0	0.1
s_5	213.0	-182.5	933.8
s_6	-0.1	-0.0	0.41
s_7	0.4	0.3	-1.2
d_1(1975)	-108.2*	-3.3	-64.2
d_2(1976)	-170.4*	-45.6	-121.7*
d_3(1977)	-187.1*	-25.8	-110.73
F, R^2	4.4*, 0.04	0.5, 0.004	1.6, 0.03

Table 4. Assignment and Annual Hours Worked in Mincome by Men, Wives, and Single Female Heads (Regression Method is OLS).

	Men			Wives			Single Female Heads		
Intercept	120.7	120.7	136.5	25.1	20.7	24.0	100.1	99.2	70.6
s_1 = T	2.4	-15.1	-28.0	-7.9	-15.7	-14.3	20.0	-49.9	-64.8
d_1(1975)	-105.5*	-104.1*	-96.5*	-3.3	-1.8	17.3	-64.6	-63.1	-16.7
d_2(1976)	-166.8*	-165.5*	-188.0*	-45.6	-44.1	-33.9	-122.5*	-121.0*	-85.1
d_3(1977)	-184.3*	-183.0*	-181.6*	-25.8	-24.3	-28.4	-111.5	-110.0	-53.0
NIC_2[a]		1.6	8.0		2.6	-10.8		6.4	6.5
NIC_3		-0.9	-0.4		0.5	-10.3		6.4	15.1
NIC_4		-0.1	1.3		1.0	-5.9		5.3	12.5
NIC_5		-1.5	-7.0		-0.2	-11.7		17.4	26.9
FABOVE		3.8	2.6		5.5	-12.6		24.2	42.2
EARNABV		0.0	0.0		0.0	0.0		0.0	0.0
FSI		-0.0	-0.1		0.0	0.0		-0.1	-0.1
T*FSI*NIC_2	-0.13			0.2			-1.2		
T*FSI*NIC_3	-0.35			0.3			-1.4		
T*FSI*NIC_4	-0.37			0.4			-1.1		
T*FSI*NIC5	-0.09			0.1			0.2		
T*FABOVE	150.8			-89.0			83.6		
ATT_3[b]			-71.2*			-35.3			-20.6
ATT_4			-24.9			-7.4			-6.8
no. obs.	1,280	1,280	1,806	1,172	1,172	1,508	580	580	815
R^2	0.04	0.03	0.03	0.01	0.00	0.0	0.03	0.02	0.02
F	4.6*	3.7*	4.8*	1.2	0.4	0.4	1.7	1.2	1.0

[a] Normal Income Cells, based on pre-experimental income, used for assignment of treatment.
[b] Attrition dummy variable: ATT_3=1 if attrition occurred in the second year of the experiment (1976); ATT_4=1 if attrition occurred in the final year of the experiment (1977).
* denotes statistical significance at the 5% level.

Table 5. Summary of Results from Five North American Income Maintenance Experiments for Annual Hours Worked (Mincome Results Based on Tables 3 and 4)

Experiment/ Author	Husbands	Wives	Single Female Heads
New Jersey:			
Keeley (1981)	-116 (7%)	-75 (33%)	
Robins (1985)	-34 (2%)	-56 (25%)	
Burtless (1986)	-21 (1%)	-56 (25%)	
Rural:			
Keeley	? (9%)	? (29%)	
Robins	-56 (3%)	-178 (28%)	
Burtless	-56 (3%)	-178 (28%)	
Seattle-Denver[a]:			
Keeley	-147 (8%)	-139 (21%)	-155 (15%)
Robins	-113 (7%)	-141 (21%)	-163 (16%)
Burtless	-144 (8%)	-107 (17%)	-85 (9%)
Gary:			
Keeley	-80 (5%)	-9 (3%)	-102 (28%)
Robins	-35 (2%)	-58 (20%)	-37 (10%)
Burtless	-114 (7%)	+14 (5%)	-112 (30%)
All U.S. Experiments:			
Robins	-89 (5%)	-117 (21%)	-123 (13%)
Burtless	-119 (7%)	-93 (17%)	-133 (17%)
Mincome:			
Tables 3 and 4	-17 (1%)[b]	-15 (3%)	-79 (7%)

Notes: [a] 3-year experiment only.
[b] includes single individuals (21% of all men in sample).

Table 6. Estimates of Annual Hours Worked From Structural Labour Supply Equations (21), (22), (23) and (24) [Regression Method is OLS for Equations (21), (22) and (23); Regression Method is Tobit for Equation (24)]

Variables	Husbands				Wives			
	Eqn(21)	Eqn(22)	Eqn(23)	Eqn(24)	Eqn(21)	Eqn(22)	Eqn(23)	Eqn(24)
Intercept	72.9	643.0*	749.8*	711.4*	117.1	624.1*	689.9*	671.5
d_1(1975)	-91.4				-29.0			
d_2(1976)	-107.2	-39.7			-186.9*	-32.6		
d_3(1977)	-92.8	-20.0			-252.4*	-28.7		
dw_{husbnd}[a]	-38.9	89.7*	110.7*	128.6*	-262.1*	-91.4	176.8	-431.3*
dw_{wife}	-28.5	-41.5	-99.6	-119.8	94.0*	-16.5	-44.7	-96.1
dv[b]	-0.01	-0.04*	-0.04*	-0.05*	-0.1*	0.0	0.0	0.0
dchld	105.0*	139.5*	140.2*	153.5*	-103.7*	-141.9*	-159.7*	-255.8*
h_p[c]		-0.7*	-0.8*	0.3*		-0.6*	-0.7*	0.4*
NIC_2		170.2*	410.7*	393.2*		-264.4*	-230.8	-548.7
NIC_3		294.7*	431.0*	429.7*		-140.6	7.9	-78.1
NIC_4		438.0*	491.1*	497.8*		-112.8	-65.1	-139.5
NIC_5		444.5*	517.7*	526.6*		-112.4	-0.8	-69.3
EARNABV		-0.0	-0.0	-0.0		0.0	-0.0	-0.0
FABOVE		401.5*	367.6*	399.9*		-352.0*	-265.2	-286.0
BREAK[d]		-0.0*	-0.0*	-0.0*		-0.0	-0.0	-0.0
AGE		2.2	1.0	0.7		-5.4	-6.9	-13.1
FAMSIZE		43.7*	87.8*	99.3*		28.8	35.2	67.4
CHILDREN		11.9	-21.1	-20.7		-141.1*	-168.5*	-306.8*
WELFARE		-0.0	0.0	0.0		-0.2	-0.0	-0.1
BOTH PTC		-119.5*	-134.0	-135.7		319.9*	306.2*	630.8*
n	852	762	230	230	884	769	230	230
R^2	0.08	0.46	0.59		0.07	0.41	0.53	
F	3.6*	32.9*	17.8*		3.15*	26.9	14.2*	

[a] The wage equation for these results include education, education squared, experience, experience squared, age, age squared, age*education, d_1, d_2, d_3, and is adjusted for selection bias.
[b] $dv=h_{husband}dw_{husband}+h_{wife}dw_{wife}+dy$ represents the change in virtual family income.
[c] h_p represents pre-experimental hours (annual hours worked in 1974).
[d] BREAK is the breakeven level assigned (G/t) as in Keeley et al (1979); AGE, FAMSIZE (family size), CHILDREN (number of children), WELFARE (welfare payments received in 1974), and BOTH PTC (both participated in the labour market in 1974) are also pre-experimental control variables defined in Keeley et al (1974).

Table 6 (continued)

Variables	Single Female Heads			
	Eqn(21)	Eqn(22)	Eqn(23)	Eqn(24)
Intercept	182.9	611.4*	377.3	164.3
d_1(1975)	-106.2			
d_2(1976)	-263.5*	-187.5*		
d_3(1977)	-361.9*	-202.2*		
dw[a]	313.3*	103.0	-113.1	-73.0
dv[b]	-0.1*	-0.0	0.0	0.0
dchld	-171.7*	-289.3*	-385.9*	49.4
h_p[c]		-0.5*	-0.6*	0.6*
NIC_2		222.1	391.7	
NIC_3		267.6*	341.6	
NIC_4		436.0*	520.8*	
NIC_5		54.7	253.9	
EARNABV		-0.0	-0.0	
FABOVE		n.a.	n.a.	
BREAK[d]		0.0	0.0	
AGE		-7.8*	-0.9	
FAMSIZE		-32.0	20.1	
CHILDREN		-225.7*	-377.1	
WELFARE		-0.1	-0.1	
n	468	474	148	148
R^2	0.10	0.37	0.45	
F	2.97*	16.4	7.65*	

[a] The wage equation for these results include education, education squared, experience, experience squared, age, age squared, age*education, d_1, d_2, d_3, and is adjusted for selection bias.
[b] dv=h.dw+dy represents the change in virtual family income.
[c] h_p represents pre-experimental hours (annual hours worked in 1974).
[d] BREAK is the breakeven level assigned (G/t) as in Keeley et al (1979); AGE, FAMSIZE (family size), CHILDREN (number of children), and WELFARE (welfare payments received in 1974).

Table 7. Summary of Labour Supply Elasticity Estimates for Husbands, Wives and Single Female Heads from Mincome.

	Husbands				Wives			
Elasticity/Wage	Eqn(21)	Eqn(22)	Eqn(23)	Eqn(24)	Eqn(21)	Eqn(22)	Eqn(23)	Eqn(24)
Compensated wage								
w_1[a]	-0.4*	0.1	0.1	0.1	-1.9*	-0.8*	-0.8*	-0.7*
w_2[b]	-0.3*	0.1	0.1	0.1	-1.8*	-0.6*	-0.7*	-0.5*
w_3[c]	-0.3*	0.2*	0.3*	0.3*	-1.9*	-0.7*	-0.5	-0.4
w4[d]	-0.1	0.2*	0.2*	0.2*	-1.3*	-0.4	-0.8	-0.9*
Income								
w_1	-0.1*	-0.1*	-0.0	-0.0	-0.1*	0.1	0.0	-0.0
w_2	-0.1*	-0.1*	-0.0	-0.0	-0.1*	-0.0	-0.0	-0.0
w_3	-0.1	-0.1*	-0.1*	-0.1*	-0.1*	-0.0	0.0	0.0
w4	-0.0	-0.1*	-0.1*	-0.1*	-0.2*	0.0	0.0	0.1
Uncompensated wg								
w_1	-0.4	-0.0	0.1	0.1	-2.0	-0.9	-0.8	-0.8
w_2	-0.4	0.0	0.1	0.1	-1.9	-0.7	-0.7	-0.6
w_3	-0.4	0.1	0.2	0.2	-2.0	-0.7	-0.4	-0.4
w_4	-0.1	0.0	0.1	0.1	-1.5	-0.4	-0.7	-0.9
Compensated cross-wage								
w_1	0.3*	0.1	0	-0.1	2.2*	0.7*	0.5	0.4
w_2	0.3*	0.0	-0.0	-0.0	1.6*	0.6*	0.5	0.4
w_3	0.3*	-0.0	-0.2	-0.2	2.3*	0.6*	0.2	0.2
w_4	-0.0	-0.1	-0.1	-0.2	0.7*	-0.1	-0.2	-0.2

* denotes that the elasticity is determined from a statistically significant regression coefficient estimate.

[a] Wage equation includes education, experience (age-education-5), experience^2, d_1, d_2, and d_3, and is corrected for selection bias.
[b] Wage equation includes education, education^2, experience (age-education-5), experience^2, age*education, d_1, d_2, and d_3, and is corrected for selection bias.
[c] Wage equation includes education, experience (reported), experience^2, age, d_1, d_2, and d_3, and is corrected for selection bias.
[d] Wage equation includes education, education^2, experience (reported), experience^2, age, age^2, age*education, d_1, d_2, and d_3, and is corrected for selection bias. The labour supply results for this wage are reported in Table 6.

Table 7 (continued).

Elasticity/Wage	Single Female Heads			
Compensated Wage				
w_1[a]	-0.5*	-0.1*	-0.2*	-0.4*
w_2[b]	-0.2*	-0.1*	-0.2	-0.2*
w_3[c]	-0.2*	-0.1	-0.1	-0.2
w_4[d]	0.8*	0.2	-0.2	-0.1
Income				
w_1	-0.1	0.1	0.2	0.2*
w_2	-0.2*	0.0	0.1	-0.0
w3	-0.1	0.0	0.1	0.0
w_4	-0.3*	-0.1	0.1	0.0
Uncompensated Wage				
w_1	-0.6	-0.1	0.0	-0.1
w_2	-0.4	-0.1	-0.1	-0.2
w3	-0.3	-0.1	-0.0	-0.1
w_4	0.5	0.1	-0.1	-0.1

* denotes that the elasticity is determined from a statistically significant regression coefficient estimate.

[a] Wage equation includes education, experience (age-education-5), experience^2, d_1, d_2, and d_3, and is corrected for selection bias.
[b] Wage equation includes education, education^2, experience (age-education-5), experience^2, age*education, d_1, d_2, and d_3, and is corrected for selection bias.
[c] Wage equation includes education, experience (reported), experience^2, age, d_1, d_2, and d_3, and is corrected for selection bias.
[d] Wage equation includes education, education^2, experience (reported), experience^2, age, age^2, age*education, d_1, d_2, and d_3, and is corrected for selection bias. The labour supply results for this wage are reported in Table 6.

Table 8. Summary of Labour Supply Elasticity Estimates from Five North American Income Maintenance Experiments.

Experiment/ Group	Substitution Elasticity	Income Elasticity
Husbands:		
New Jersey	0.1	-0.0
Rural	0.1	0.0
Seattle-Denver	0.1	-0.1
Gary	0.1	-0.1
All U.S.	0.1	-0.1
Mincome Manitoba	0.1	-0.1
Wives:		
New Jersey	-0.1	-0.3
Rural	0.3	0.01
Seattle-Denver	0.1	-0.1
Gary	0.4	0.3
All U.S.	0.2	-0.1
Mincome Manitoba	-0.9	-0.0
Single Female Heads:		
New Jersey		
Rural		
Seattle-Denver	0.1	-0.2
Gary	0.1	-0.2
All U.S.	0.1	-0.2
Mincome Manitoba	-0.1	-0.0

Sources: Robins (1985) for the U.S. experiments; Table 7 for the Canadian experiment (simple average of all estimates).