# LABOUR SUPPLY ESTIMATION AND PUBLIC POLICY

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**Abstract.** Economists devote considerable energies towards refining their econometric techniques to overcome difficulties connected with conducting empirical research. Despite advances in technique. it is not clear whether further refinement in this direction is worthwhile for policy purposes. It may be that no further amount of statistical adjustment of inadequate data will increase understanding, and that better data is simply necessary to add to our knowledge. But rarely is sufficient credit paid to new forms of data. In short, econometric technique is emphasized to the neglect of data innovation, as if new data were merely lying about waiting for an ingenious suggestion for use. This paper surveys advances of the last twenty five years in estimating labour supply for policy purposes with a view towards appreciating the relative contribution of both improvements in econometric technique as well as developments of new data.

After briefly detailing the key parameters which economists have sought to estimate, we describe the early 'first generation' research (circa 1970), which is plagued by problems of unobservable variables, measurement errors, truncation and selectivity bias, and non linear budget constraints. 'Second generation' research constitute attempts to resolve one or more of these difficulties, and the respective contribution of econometric technique and new data is acknowledged and assessed, including the contribution of data generated by large scale social experiments in which participants are randomly assigned to different guaranteed income plans and their labour supply behaviour measured.

**Keywords.** labour supply estimation; income maintenance experiments; econometrics; experimental panel data; public policy.

## 1. Introduction

The uneasy alliance between economic theory and empirical studies in assisting public policy is a perennial controversy. Some feel that theoretical understanding of human behaviour is more helpful since unambiguous empirical results in economics are rarely possible, at least in the sense of devising definitive testing of theories under controlled experimental conditions. In the final analysis, public policy must therefore rely on informed judgment. Others hold that 'getting the numbers' is more useful for public policy purposes since economic theories are usually too simple and general to be relevant. Policy development without the hard facts would be irresponsible. It is likely that most of the profession will hold an intermediate position; that progress in understanding economic behaviour requires a judicious blend of theoretical and empirical analysis with each informing the other.

0950–0804/94/01 0057–25 JOURNAL OF ECONOMIC SURVEYS Vol. 8, No. 1

This conventional methodological stance leads to two further observations. Introductory textbooks in economics usually proclaim in the first pages on economic method that theirs is a social science, and that testing of alternative theories cannot employ the vaunted experimental methods of the natural and physical sciences. Yet experimental data, even large-scale economic experiments, are no longer impossible to find. A second related point is that economists have devoted considerable energy and ingenuity toward refinement of econometric techniques to overcome a variety of difficulties associated with conducting empirical economic research. Despite these acclaimed advances in technique and computing methods, it is not clear whether further refinement in this direction is worthwhile for policy purposes, since a statistical cathedral is often being erected on sandy data foundations. It may be that no further amount of statistical adjustment of inadequate data will yield deeper understanding, and that better data is simply necessary to add to our knowledge. Very rarely, in our opinion, is sufficient credit paid to new forms of data when charting the progress of economic research on a given subject, and all too often attention is drawn to either a novel estimator or computing algorithm. In short, statistical technique has been emphasized to the neglect of data innovation, as if new data were merely lying about waiting for an ingenious suggestion for its use.

In this paper we shall survey one area that illustrates these methodological issues. We consider recent empirical research in labour supply, particularly as it relates to income maintenance policy development and the search for both new econometric techniques and data sources. In our view, labour supply is a particularly useful area to consider such issues for at least four reasons. First, conventional labour supply analysis is based upon the well understood theory of consumer behaviour and hence provides a very clear theoretical underpinning for empirical research. Secondly, labour supply research has direct practical application and policy focus. In particular, there is enduring interest in the work incentive or labour supply effects of alternative income maintenance programs, literally a textbook example of the application of labour supply analysis to public policy debate.[1] Thirdly, numerous household surveys have been developed during the last two decades to enhance empirical research on labour supply behaviour. These include non-experimental data from extensive cross-sectional and panel surveys as well as special site-specific experimental panel data. Finally, labour economics has fostered a strong empirical tradition (Stafford, 1986). This tradition produces empirical and theoretical research of high quality and nowhere is this more evident than labour supply studies. For these reasons labour supply research, perhaps more than any other area of economics, can teach us a great deal about blending theory and evidence for policy application.

The next section sets the stage by considering the basic theory of labour supply behaviour and the crucial parameters which need to be estimated. We then consider the policy application of the labour supply response to a negative income tax or guaranteed annual income program in section 3. We next describe and characterize the early empirical research in section 4, emphasizing those problems which become the focus of subsequent research. The fifth section

considers advances in econometric research and data collection and their respective roles in resolving these problems in the last fifteen years. The concluding section summarizes some lessons learned from labour supply research and relates them to ongoing controversies in other areas of economics.

## 2. Basic theory of labour supply

We begin our discussion by briefly outlining the conventional static theory of labour supply. Since this theory is fairly well known, our treatment will concentrate on model development as the basis for policy evaluation and empirical research. Various theoretical developments, including life-cycle models, that have been the subject of much empirical research in recent years are reserved for later discussion.

Consider a household with two adults and unearned income $y$ facing market wages $w_1$ and $w_2$. The household chooses hours of work $h_1 \geqslant 0$ and $h_2 \geqslant 0$ and a consumption level $g > 0$ that maximize a family utility function (Becker,1974; Killingsworth and Heckman, 1986)

$$U(g, h_1, h_2), \quad \frac{\partial U}{\partial g} > 0, \quad \frac{\partial U}{\partial h_1} < 0, \quad \frac{\partial U}{\partial h_2} < 0, \tag{1}$$

subject to the budget constraint

$$w_1 h_1 + w_2 h_2 + y = g$$

where the price of the consumption good is normalized so that $w_1$ and $w_2$ are real wage rates. The solution from first order conditions gives us labour supply functions of the form

$$\begin{aligned} h_1 &= f_1(w_1, w_2, y) > 0 \quad \text{if } s_1 = w_1 \\ &= 0 \quad \text{if } s_1 > w_1 \end{aligned} \tag{2}$$

$$\begin{aligned} h_2 &= f_2(w_2, w_2, y) > 0 \quad \text{if } s_2 = w_2 \\ &= 0 \quad \text{if } s_2 > w_2 \end{aligned} \tag{3}$$

where $s_1 = -(\partial U/\partial h_1)/m$ and $s_2 = -(\partial U/\partial h_2)/m$ are the shadow wage rates, representing the opportunity cost of time spent in the labour market, and $m = \partial U/\partial g$ is the marginal utility of income.

The Slutsky decomposition of equations (2) and (3) gives

$$\frac{\partial h_i}{\partial w_i} = \frac{\partial f_i^s}{\partial w_i} + h_i \frac{\partial f_i}{\partial y} \quad i = 1, 2 \tag{4}$$

where $\partial f_i^s/\partial w_i$ is the compensated wage or substitution effect, indicating the change in labour supply arising from a change in the wage rate if income is held constant, and $hi\ \partial f_i/\partial y$ is the income effect. The decomposition can also be carried out for the cross-wage effect arising from a change in the wage of the other adult in the household:

$$\frac{\partial h_i}{\partial w_j} = \frac{\partial f_i^s}{\partial w_j} + h_j \frac{\partial f_i}{\partial y} \quad i, j = 1, 2;\ i \neq j \tag{5}$$

Standard consumer theory predicts that the compensated wage effects will be positive, that the income effects will be negative, and that

$$\frac{\partial f_1^s}{\partial w_2} = \frac{\partial f_2^s}{\partial w_1} \tag{6}$$

Equation (6) indicates that the compensated cross-wage effects must be equal, although this does not restrict the sign nor size of the uncompensated cross-wage effects.

Applied economists are particularly interested in estimates of three magnitudes:

(a) the compensated wage elasticity,

$$\eta_i^s \equiv \frac{\partial f_i^s}{\partial w_i} \frac{w_i}{h_i} \tag{7}$$

(b) the income elasticity,

$$\eta_y \equiv \frac{\partial f_i}{\partial y} \frac{y}{h_i} = w_i \frac{\partial f_i}{\partial y} \tag{8}$$

and (c) the compensated cross-wage elasticity,

$$\xi_i^s \equiv \frac{\partial f_i^s}{\partial w_j} \frac{w_j}{h_i} \tag{9}$$

With estimates of these three elasticities, economists can then predict the mean labour supply response to a variety of policy initiatives that alter the incentives to work through the tax-transfer system. Applications of the approach are widespread in the literature.[2] In this paper, we focus on one particularly clear application, the work incentive effects of income maintenance programs.

## 3. Labour supply and income maintenance policy

Labour supply research has addressed many issues associated with the work incentive effects of taxes and transfers. In order to focus on the link between economic research and policy, we shall confine our discussion to the contentious issue of income maintenance and work incentives; in particular, the response to a negative income tax or guaranteed annual income program.

The choice of policy issue is based on a number of considerations. First, the issue has an enduring social interest. In the past three decades since Friedman's (1963) initial proposal of a negative income tax, there has been widespread interest in welfare reform revolving around a guaranteed annual income program. The tantalizing possibility of replacing several income transfer programs with a single negative income tax never disappears from policy discussions. In recent years, the issue has been widely discussed in the U.K. (eg., Britten and Webb, 1990) and elsewhere in Europe in the context of a new social charter for the European Community. Moreover, the single most worrisome issue concerning a guaranteed income program has been work incentives or,

more generally, the labour supply response of recipients. Indeed, there have been five large-scale social experiments conducted in Canada and the United States during the past twenty-five years to examine the effects of a guaranteed annual income program, and labour supply response has been the dominant concern in every case.

Secondly, the links between economic research and policy application are clear and direct. One of the most attractive features of a guaranteed annual income program for both administration and policy analysis is its simplicity. The program replaces a complex system of cash and in-kind transfers and entitlement formulae (tax-back rates) with a direct tax and transfer scheme. Each household is provided with an income guarantee, $G$, based upon family size and composition. Each working member of the family faces (for simplicity of exposition) a single, uniform tax rate, $t$. Thus net payments, $P$, are

$$P = G - tY > 0 \quad \text{if } Y < B, \tag{10}$$

where

$$Y = w_1h_1 + w_2h_2 + y \text{ and } B = \frac{G}{t}$$

where $y$ represents unearned income. Payments gradually decline under the program until household income reaches the break-even point, $B$.

In economic terms, the budget constraint faced by individuals under such a program is well-defined and well-behaved (continuous and convex), facilitating analysis of behavioural response. In particular, we can write the total differential for the labour supply function of, say, the first adult in equation (2) as

$$\mathrm{d}h_1 = \frac{\partial h_1}{\partial w_1}\,\mathrm{d}w_1 + \frac{\partial h_1}{\partial w_2}\,\mathrm{d}w_2 + \frac{\partial h_1}{\partial y}\,\mathrm{d}y \tag{11}$$

and use the Slutsky equations (4) and (5) to obtain

$$\mathrm{d}h_1 = \frac{\partial f_1^s}{\partial w_1}\,\mathrm{d}w_1 + \frac{\partial f_1^s}{\partial w_2}\,\mathrm{d}w_2 + (h_1\,\mathrm{d}w_1 + h_2\,\mathrm{d}w_2 + \mathrm{d}y)\,\frac{\partial f_1}{\partial y} \tag{12}$$

where $\mathrm{d}w_1$ is the change in the first adult's wage rate resulting from the tax change and $dw_2$ is the change in the spouse's wage rate from the tax change. Of course, $dw_2 = 0$ for single individuals and single parents. The income effect then depends upon the change in unearned income, $\mathrm{d}y$, caused by the change in income guarantee plus the income effects associated with the change in after-tax earnings for each adult evaluated at hours worked prior to the policy shift, $h_1$ and $h_2$. To determine relative changes in labour supply, equation (12) can be written

$$\frac{\mathrm{d}h_1}{h_1} = \eta_1^s\,\frac{\mathrm{d}w_1}{w_1} + \xi_1^s\,\frac{\mathrm{d}w_2}{w_2} + \eta_y\left(\frac{\mathrm{d}w_1}{w_1} + \frac{\mathrm{d}w_2}{w_2} + \frac{\mathrm{d}y}{y}\right) \tag{13}$$

This means that the relevant policy parameters ($t$ and $G$) and estimates of the labour supply elasticities will be sufficient to predict labour supply response for

individuals in relative terms. Consequently, economic research and policy analysis are simply and clearly linked.

Thirdly, research interest in the issue has been consistently strong. As noted, the issue generated five large-scale social experiments and an impressive array of economic research, using both this experimental evidence and the rapidly expanding nonexperimental data available from household surveys of labour market activity. Thus our examination of the policy relevance of labour supply research can draw on a rich variety of analyses both in terms of econometric innovations and in terms of the type of evidence used. Indeed, few other areas of economic research can boast of a more varied or investigated data set. These three factors provide an excellent, perhaps unique, basis for considering the contribution of conventional economic methodology to policy issues.

## 4. The early 'first generation' research

Since our interest in labour supply research is not historical, we adopt a pragmatic starting point *circa* 1965. This starting point reflects both the advent of serious policy discussion concerning the feasibility of a guaranteed annual income program and the early stages of modern applied econometric research using the first readily available household microdata bases. Also, it is at this point that computing technology begins to permit the estimation, testing, and evaluation of economic models and their policy application (Stafford, 1986).

The growing interest in income maintenance and the quantification of its labour supply effects is reflected in the landmark volume by Cain and Watts (1973). In fact, the authors provide an excellent summary of the state of the art in empirical labour supply research with nonexperimental, cross-sectional household microdata, just on the eve of the advent of social experimentation on this question.

The contributors to this volume identify four important and enduring empirical issues: unobservable variables, measurement error, truncation and selection bias, and nonlinear budget constraints. The potential importance of unobservable, and hence omitted, person-specific differences such as 'preferences' or 'ambition' is recognized by several authors (Greenberg and Kosters, 1973; Fleisher, Parsons and Porter, 1973). Omission of these variables will introduce bias into estimates of labour supply equations (2) and (3) because such variables are correlated with labour supply, wages and unearned income. Attempts to address this difficulty by including household wealth as a proxy variable are unsatisfactory because wealth is endogenous, and hence itself a source of bias in a properly specified life-cycle model of labour supply. Others have attempted to reduce the influence of this unobserved 'worker heterogeneity' by restricting the sample on the basis of income, employment status, residence, age, race or marital status. These *ad hoc* restrictions not only reduce wage and income variation, and hence estimation efficiency, but also complicate comparisons across studies and render predictions of labour supply for the general population very hazardous. And, of course, the extent to which sample

restrictions succeed in reducing the bias in labour supply estimates from unobserved heterogeneity remains unknown.

Secondly, Cain and Watts (1973, 350–351) recognize the measurement problems associated with certain crucial variables. For example, they note that the 1967 Survey of Economic Opportunity provides fourteen different measures of labour supply for the contributors to their volume. Although annual hours worked seems to have become the standard, there are many alternative measures and no clear theoretical basis for choosing among them. It seems clear that annual hours worked is superior to such partial measures as labour force participation, weeks worked, and hours worked in a short reference period, but hours spent looking for work and hours spent in school might also be included as part of labour supply. Moreover, Cain and Watts identify errors-in-variables bias arising from the problem of measuring 'normal' or 'permanent' wages and incomes from the snapshot provided by cross-sectional data and biases arising from the inclusion of any transfer income, such as welfare payments, conditioned on labour supply.

The third set of issues concerns sample truncation and selection bias. Hall (1973) argues that labour supply equations (2) and (3) should specifically include non-workers despite the absence of an observed wage His solution has become standard practice in the estimation of labour supply models: Estimate a wage regression for the sample of workers and use the regression to impute the wages of both workers and non-workers for the labour supply regression. As we shall discuss later, however, the issue of selection bias arising from non-workers is decidedly more complex than this two-step procedure.

Truncation bias may also arise if the sample is chosen on the basis of current earnings or total income, which both depend upon labour supply (Cain and Watts, 1973, 340–348). Choice of a low-income sample to investigate the labour supply effects of income maintenance will exclude some households that are normally poor and include some households that are normally not poor because of income fluctuations over time. This will bias regression estimates of labour supply parameters toward zero (Hausman and Wise, 1976 and 1977). In particular, the income maintenance experiments selected participants on the basis of a household income ceiling which introduces this type of bias (Keeley and Robins, 1978).

Finally, Hall (1973) also recognizes the problem of nonlinear budget constraints created by the tax-transfer structure. In the simplest case, a worker faces a nonlinear budget constraint because the marginal tax rate rises as earned income rises. This implies that the marginal tax rate, and therefore the after-tax wage, are unknown until earned income is known. Earned income, however, depends directly on labour supply, which is yet to be explained. Hall introduces the useful concept of 'virtual income' as the unearned income that would arise if the worker paid the marginal tax rate on all income earned. Under a progressive tax scheme, for example, a rising marginal tax rate implies a rising virtual income to reflect tax 'savings' on inframarginal earnings. This concept permits the nonlinear budget constraint to be represented by a linear constraint

in which the crucial labour supply variables — the after-tax wage and unearned income — are uniquely defined. Thus, the current marginal tax rate a worker faces, and the associated virtual income, define a 'linearized' budget constraint or the purposes of estimating labour supply. Additional complications arise, but these are left to the next section.

Most of the early labour supply results for the U.S. are provided by Cain and Watts and summarized in a number of surveys (Keeley, 1981; Killingsworth, 1983; Hum and Simpson, 1991). Although these results use only nonexperimental cross-sectional data and ordinary least squares regression techniques, they constituted the 'conventional wisdom' for a decade or more, and provided the basis for policy development. These results are quite easily summarized. The responsiveness of labour supply to wage rate changes, implied by the uncompensated wage elasticity in equation (4), is generally greater, and estimates have greater variation, for females than for males. This occurs because the compensated wage elasticity estimates are greater, and have greater variability, for females. Compensated wage elasticities are found to be as high as 2.9 for females by Hall (1973), and Hum and Simpson (1991) find a mean compensated wage elasticity of 0.9 for all published U.S. studies of this period. For males, on the other hand, the highest compensated wage elasticity found is 0.9 by Kalachek and Raines (1970) and the mean of all studies is only 0.1. Income elasticity estimates are much more uniform, except for Hall's inexplicable figure of 2.1, and the mean of all studies is 0.2 for both males and females. Thus the mean uncompensated wage elasticity is 0.8 for females but $-0.1$ for males from the early literature. These results are reflected in numerous policy simulation exercises (e.g., Rea, 1983). Most of the literature and policy research ignores cross-wage elasticities, and McElroy (1981, 58) concludes only that 'the cross-wage elasticities are poorly estimated in both the male and female supply equations ...'

Results for other countries for this period are principally from the U.K. and are reviewed in Killingsworth (1983). The mean compensated wage elasticity for men is 0.3, but the compensated wage elasticity estimates are predominantly negative, contrary to theoretical expectations, and the mean is only positive because of Ashworth and Ulph's (1981) very large estimate of 2.2 for married couples from the British Market Research Bureau Survey. Without Ashworth and Ulph's results, the mean compensated wage elasticity estimate is $-0.1$. Ashworth and Ulph's income elasticity estimate is also very large ($-2.1$) and accounts for mean income elasticity estimate of $-0.4$. Without their estimate, the mean income elasticity estimate is negligible. For women, the elasticity estimates are generally lower, contrary to U.S. results for the period, yielding a mean of 0.1 for each of the compensated wage and income elasticities and a negligible uncompensated wage elasticity.

We have already mentioned several econometric reasons to question the stylized elasticity estimates from the early literature. This doubt is reinforced by daVanzo *et al.* (1976) who find that the estimates of labour supply elasticities are sensitive both to the inclusion of nonworkers and to the inclusion of control

variables such as education and family net worth for white husbands aged 25 to 54. Since adult males are found to be the most stable group for labour supply analysis, these sensitivity tests cast considerable doubt on the reliability of the early literature in general.

## 5. Recent 'second generation' research

Killingsworth (1983) distinguishes between the early 'first-generation' research portrayed above and more recent 'second generation' research that directly addresses the econometric problems identified in the early research — particularly, unobservable person-specific effects, sample selection bias, and nonlinear budget constraints. We argue, however, that the economic literature has paid too much attention to econometric refinements in the analysis of labour supply and too little attention to data improvements. Progress in measuring labour supply response has been derived from the combination of econometric refinements and superior data, particularly panel and experimental data. Some of this data, particularly the experimental data, was, in fact, generated to answer specific questions concerning labour supply response that were deemed impossible to address through the analysis of cross-sectional microdata, however detailed. Thus, we divide this section in two, first considering econometric refinements and then data improvements.

### 5.1. *Second generation econometric refinements*

The treatment of nonworkers has received the most attention. Problems arise because the wage rate is unobserved and hours worked are truncated at zero for a nonworker. First generation studies typically excluded nonworkers, but this procedure introduces sample selection bias, since the participation decision is nonrandom. In particular, those who do not participate are likely to have lower wage offers and/or higher shadow wages at zero hours. Heckman (1976, 1979) and Lee (1978) show that this sample selection problem is equivalent to the omission of a relevant, but unobserved, variable (the inverse Mill's ratio) in the wage and labour supply models. A consistent estimate of this variable can be constructed from a consistent estimate of the participation decision.[3] Inclusion of this constructed variable in the wage and labour supply equations then renders OLS estimates of those equations consistent.[4] This method has been called the 'generalized Tobit' regression technique (Mroz, 1987) because some studies estimate labour supply equation (18) by Tobit regression (Tobin, 1958; Amemiya, 1973), a more restrictive method which assumes that the variables which determine participation and hours worked are identical (Heckman, 1979). In the presence of fixed employment costs for workers, this assumption may be inappropriate (Cogan, 1981) and the generalized Tobit estimator will be preferred.

A second important complication in the estimation of labour supply behaviour

arises because the budget constraint is nonlinear under a system of income-conditioned transfers and progressive marginal income tax rates. One of the crucial variables in labour supply analysis, the wage rate after tax, will then depend upon a tax rate which varies with earnings, and thus with the dependent variable, hours worked, thereby creating bias. One simple solution is to linearize the budget constraint by using instrumental variables for the after-tax wage rate (Johnson and Pencavel, 1984; Leuthold, 1985).

This approach is inadequate, however, if the budget constraint is kinked and/or nonconvex, as is demonstrably the case in many industrialized countries, especially for married women in low-income families (Hausman, 1980, for the U.S.; Stelcner and Smith, 1985, for Canada; Blomquist and Hansson-Brusewitz, 1990, for Sweden; Bourgignon and Magnac, 1990, for France; van Soest *et al.*, 1990, for the Netherlands; Blundell *et al.*, 1991, for the U.K.). Burtless and Hausman (1978) and Hausman (1980) specify an indirect utility function, corresponding to a specified labour supply function, to find the utility-maximizing hours worked across all segments of a piecewise-linear budget for each individual. The estimates of the parameters of the indirect utlllty function obtained from iterative maximum likelihood estimation are then converted to estimates of the corresponding labour supply parameters. These estimates typically imply higher substitution and lower income elasticities than other approaches (Hausman, 1985).

This approach has remained controversial, however. Killingsworth (1983, 177–179) warns that this 'complete budget constraint' approach is complicated, and even more so if preferences, hours worked, and the specified budget constraint all contain errors. Heckman (1981) also argues that the simpler 'linearized budget constraint' approach may prove to be more robust in practice. MaCurdy *et al.* (1990, 417) conclude that the estimation techniques used in the complete-budget-constraint approach 'require local satisfaction of the Slutsky condition over a wide range of wage-income combinations... It is no surprise that compensated effects ... are typically nonnegative ... since this nonnegativity constraint is essentially imposed by the procedure.' They show that the consequence of this procedure is to bias the compensated wage elasticity upward and the income elasticity downward, accounting for much if not all of the observed difference between estimates from this approach and other approaches. Moreover, the bias is potentially greater as the number of budget segments increases, so that more detailed and complex budget constraints likely generate greater bias. Blundell *et al.* (1991) specify and estimate a nonlinear labour supply function with a piecewise-llnear budget constraint to address the problem raised by MaCurdy *et al.* and find relatively small mean uncompensated wage elasticities (0.2) for lone parents in the U.K., although labour supply is more wage-elastic at low hours.

Other prominent problems, such as measurement error in critical variables and unobservable person-specific effects (or worker heterogeneity), require more than mere econometric refinements. Rather, treatment of these problems depends upon the development of panel microdata sets, to which we now turn.

### 5.2. *Data improvements*

The early econometric evidence cited in section 4 was derived from household survey microdata sets that were also at an early stage of development. Survey research has since addressed many early shortcomings of the data and now often produces more sophisticated and more reliable data for labour supply analysis. While these improvements have likely occurred in many subtle ways, we wish to focus on two more dramatic developments: Panel and experimental data sets. We believe that these data developments have been particularly important in advancing labour supply research during the past fifteen years.

Panel data is particularly useful in the treatment of unobservable person-specific effects. Reconsider the labour supply equation (from (2) (3)) to incorporate person-specific effects in the error term for individual $i$:

$$h_i = Z_i\gamma_i + u_i \gtrless 0 \qquad (14)$$

$$\text{where } u_i = \alpha_i + \xi_i$$

where the person-specific effect, $\alpha_i$, say 'motivation,' may be expected to be correlated with included regressors in $Z_i$, in particular such critical variables as wage rates and household income. The remaining error, $\xi_i$, may be assumed to be random noise uncorrelated with $Z_i$. From this perspective, conventional estimates of labour supply behaviour considered above will be biased. As mentioned earlier, proxy variables for $\alpha_i$, such as wealth, may not solve the problem since they typically introduce bias through their correlation with $\xi_i$. The preferred solution is to treat the person-specific effects as fixed effects which do not vary over time. Thus, with panel data for individuals at, say, time $t_0$ and time $t_1$ we may estimate an equation of the form:

$$\begin{aligned}\Delta h_i \equiv h_{i,t_1} - h_{i,t_0} &= [Z_{i,t_1} - Z_{i,t_0}]\gamma_i + [\alpha_{i,t_1} - \alpha_{i,t_0}] + [\xi_{i,t_1} - \xi_{i,t_0}] \qquad (15)\\ &= \Delta Z_i\gamma_i + \Delta\xi_i \text{ since } \alpha_{i,t_1} = \alpha_{i,t_0}\end{aligned}$$

to eliminate the fixed effect. Note that this equation is consistent with equation (13) to estimate compensated wage, compensated cross-wage and income elasticities with an appropriate redefinition of the variables.

From this perspective we first consider the analysis of nonexperimental panel data sets, such as the National Longitudinal Survey and the Panel Study of Income Dynamics in the United States. These data sets permit us to estimate equation (15), which has eliminated the bias arising from unobservable person-specific effects if we assume that these effects do not change during the period of observation. The problem, however, is that the critical variables in equation (15) — namely, after-tax wages of household members and unearned household income — also change very slowly and quite predictably in most nonexperimental circumstances. Thus estimates of the crucial labour supply parameters may be imprecise.

Indeed, this problem may partly explain the shift in attention in recent years to the estimation of intertemporal substitution effects arising from life-cycle

labour supply models (Killingsworth, 1983, 263–97; Altonji, 1986). Proponents of the life-cycle approach argue that static labour supply models confuse intertemporal substitution, arising from a wage change *anticipated* by the lifetime labour supply plan, with the compensated and uncompensated wage elasticities which arise from an *unanticipated* wage change. In principle, life-cycle labour supply models are superior because they permit us to distinguish between the effects of anticipated and unanticipated wage changes. In practical terms, however, the explanation of hours and wages in nonexperimental panel data is likely to be dominated by anticipated wage changes, reflecting human capital accumulation and other systematic processes, rather than unanticipated wage changes, consisting of individual effects (personal windfalls or tragedies), which are random, and societal effects (policy shocks), which are infrequent.[5] Hence, such data is normally more suited to the estimation of intertemporal substitution effects than static labour supply parameters.

This is a serious problem because the analysis of the effect of changes in tax-transfer policy concerns unanticipated wage and income changes. Static labour supply models remain a useful simplification to assess these effects directly, provided that the unanticipated (static) and anticipated (dynamic) effects may be differentiated. Differentiation of these effects requires the occurrence of unanticipated wage and income changes, which can be introduced into after-tax wage rates and income experimentally. Indeed, this was an important original rationale for the income maintenance experiments: Policy advisors lacked confidence in the estimates of labour supply behaviour provided by nonexperimental data. The dynamic interpretation of labour supply behaviour reinforces this rationale.

Experimental panel data can introduce dramatic, unanticipated changes in the budget constraints facing participants to facilitate estimation of the labour supply response to comparable policy reform. Hum and Simpson (1991) calculate for the Canadian income maintenance experiment (Mincome), for example, that the after-tax wage of husbands fell by an average of almost 50% and virtual income rose by an average of more than 150% for husbands in the treatment group of the experiment compared to husbands in the control group between 1974 and 1976.

Two other valuable features of the experimental data are worth noting. First, the experimental data generally used constant marginal tax rates, which reduces the potential problems arising from nonlinear budget constraints. Since the treatment of these problems is both complex and controversial, as discussed in section 5.1, this is a fortunate simplification. Secondly, bias may arise in the analysis of labour supply response in nonexperimental data because workers are constrained in the hours they work (Ham, 1988; van Soest *et al.*, 1990) and the probability of being constrained is correlated with determinants of labour supply. Analysis of this bias in nonexperimental data is complex, however, because any indicator of underemployment is likely to be endogenous and determined by the same variables that determine labour supply, ruling out any sort of instrumental variables estimation (Kniesner and Goldsmith, 1987). This

bias should be less serious for experimental data because the assignment of plans (tax rates and income guarantees) is unlikely to be correlated with the probability that an individual faces a limitation on hours worked at a given site.

Data innovations, like econometric innovations, bring their own new problems with them. Stafford (1986) identifies attrition of respondents and questionnaire inconsistencies over time as particular difficulties associated with panel data. To these problems must be added the nonrandom sampling procedures, sample truncation problems, and limited duration associated with the income maintenance experiments. The limited research to date on these issues indicates that attrition, nonrandom sampling, and experimental duration had little effect on labour supply response in the U.S. experiments (Ashenfelter, 1980, 1983; Robins and West, 1978; Robins, 1984). While awaiting further research on these issues, we examine the empirical results from second-generation econometric techniques and data to see what tentative conclusions can be drawn about methodological issues from labour supply research.

### 5.3. *Second-generation empirical results*

What have we learned about labour supply response, particularly the compensated wage and cross-wage elasticities and the income elasticity identified as crucial parameters in equation (13)? In this section we summarize the results of the last fifteen years of research as well as assess the relative importance of the various econometric refinements and data improvements outlined above.

The econometric refinements, like the early results discussed in section 4, have primarily been applied to cross-sectional microdata. It is both important and customary to distinguish between male and female labour supply results. We present a summary of the studies for men and women in Tables 1 and 2, respectively.

On the one hand, interest in labour supply research on men has been curtailed by first generation evidence that male labour supply is relatively unresponsive to wage and income changes and therefore less interesting than female labour supply. Recent studies of labour supply for males often ignore the econometric problems identified in section 5.1, reasoning that those problems are less serious for males. Consequently, studies in Table 1 are often able to examine other issues, such as taxes and uncertainty (MaCurdy, 1981 and 1983), older workers (Hanoch and Honig, 1983), interval data (Ham and Hsiao, 1984), and non-pecuniary compensation (Filer, 1986). One exception is Ehnes and Simpson (1983), who find that the inclusion of nonworkers in the sample and the treatment of sample selection by Tobit estimation of the labour supply equation increases the uncompensated wage elasticity from $-0.21$ to 0.27. Another exception is a series of studies for the U.K. (Ashworth and Ulph, 1981; Ruffell, 1981; Blundell and Walker, 1982) and other European countries (Blomquist and Hansson-Brusewitz, 1990; Bourgignon and Magnac, 1990; van Soest *et al.*, 1990) which incorporate nonlinear budget constraints. Elasticity estimates are uniformly small and, for the U.K. studies, income elasticity estimates dominate

**Table 1.** Estimates of Labour Supply Elasticities from Second-Generation Studies of Nonexperimental Data for Adult Men

| Author | Data | Study Characteristics | Estimation[1] | Elasticity Estimates: Comp. Wage | Income | Uncomp. Wage |
|---|---|---|---|---|---|---|
| Ashworth & Ulph (1981) | British Mkt Research Bureau (BMRB) | U.K. husbands; nonlinear budget | OLS | 0.03 | 0.23 | −0.20 |
| | | | ML | −0.30 | 0.14 | −0.50 |
| | | | ML – complete budget | −0.19 | 0.42 | −0.61 |
| Ruffell (1981) | BMRB | U.K. husbands; nl budget | OLS | 0.09 | −0.16 | −0.07 |
| | | | ML | 0.04 | −0.11 | −0.07 |
| Blundell & Walker (1982) | Family Expend. Survey | U.K. husbands, working wives. nl budget | ML-sel bias (working wives) | 0.13 | −0.36 | −0.23 |
| MaCurdy (1983) | Seattle-Denver IME 1972–5 | Working husbands; wages endogenous | 2SLS | 0.74 to 1.43 | −0.74 to −0.43 | — |
| Ehnes & Simpson (1983) | Mincome Baseline 1974 | Adults under 59; family l s model | OLS | 0.42 | −0.60 | 0.18 |
| | | | Tobit | 0.95 | −0.68 | 0.27 |
| Hanoch & Honig (1983) | Retirement History survey, 1969–75 | Husbands over 58 in 1969 | 3SLS on wages, hours, weeks | 0.19 | −0.02 | 0.17 |
| Hausman and Ruud (1984) | Panel Study of Income Dynamics 1976 | Married couples. n l budget | ML | 0.07 | −0.10 | −0.03 |
| Ham and Hsio (1984) | Census of Canada 1971 | 25–54, worked in 1970; interval data | ML | 0.12 | −0.01 | 0.11 |
| Filler (1986) | Quality of Empt Survey 1977 | Over 16, over 20 hrs per wk; includes nonpecuniary compensation | OLS (wage exog) | −0.10 | 0.00 | −0.10 |
| | | | 2SLS (wage endog) | 0.09 | 0.00 | 0.09 |
| Ransom (1987a, 1987b) | PSID 1977 | Families; husbands 30–50; working wives | ML-endog switch | −0.05 | −0.02 | −0.09 |
| | | | ML-simult. Tobit | 0.04 | −0.03 | −0.04 |
| Blomquist & Hansson-Brusewitz (1990) | 1981 Level of Living Survey | Swedish husbands 15–75 | ML- n l budget | 0.09 | −0.01 | 0.08 |

*Continued*

**Table 1.** (*Continued*)

| Author | Data | Study Characteristics | Estimation[1] | Elasticity Estimates | | |
|---|---|---|---|---|---|---|
| | | | | Comp. Wage | Income | Uncomp. Wage |
| Triest (1990) | 1984 PSID | Husbands 25–55 | IV, ML | 0.0 | 0.0 | 0.0 |
| van Soest *et al.* (1990) | 1985 Org Strategic Labor Mkt Research Survey | Dutch husbands | ML-n l budget | 0.13 | −0.01 | 0.12 |
| Bourgignon & Magnac (1990) | 1985 INSEE Labor Force Survey | French husbands 18–60 | ML-n 1 budget | <0.1 | 0.0 | <0.1 |

Notes: Estimates are an average if more than one similarly constructed point estimate is reported.
[1] Linearized budget constraint assumed unless otherwise stated.

compensated wage elasticity estimates to produce modestly backward bending supply schedules for men (a mean uncompensated wage elasticity of −0.2). Although the studies of male labour supply are difficult to compare, the results are generally consistent with the low wage and income elasticity estimates obtained in the early research.

For females, the story is suite different. There has been considerable research activity concentrating on the econometric problems outlined in section 5.1. Yet the results remain disappointing from the standpoint of estimation precision. The range of results in Table 2 is, if anything, even wider than that for the early results, causing Killingsworth and Heckman (1986) to conclude that almost any estimate will find a comparable result in the literature. In other words, there is little consensus for prediction and policy assessment.

Moreover, the influence of the econometric refinements on the results is difficult to determine. Heckman's (1976) early treatment of nonworkers and selection bias finds larger wage elasticity estimates in comparison with earlier (OLS) methodology, but later studies (Heckman, 1980; Nakamura *et al.*, 1979; Nakamura and Nakamura, 1981; Stelcner and Breslaw, 1985) reverse this result. Franz and Kawasaki (1981) find relatively large compensated and uncompensated wage elasticities (exceeding 1) for West German wives. Analyses incorporating nonlinear budget constraints (Hausman, 1981; Hausman and Ruud, 1984; Moffitt, 1984; Yatchew, 1985; Blomquist and Hansson-Brusewitz, 1990; Colombino and del Boca, 1990; Triest, 1990; van Soest *et al*, 1990) provide estimates that are at neither extreme of the range of results, although they contain a bias toward larger elasticity estimates (MaCurdy *et al.*, 1990) noted in section 5.1.

Estimates of the cross-wage elasticity are infrequent and imprecise. Results are restricted to the uncompensated cross-wage elasticity, which is generally negative

**Table 2.** Estimates of Labour Supply Elasticities from Second-Generation Studies of Nonexperimental Data for Adult Women

| Author | Data | Study Characteristics | Estimation[1] | Elasticity Estimates: Comp. Wage | Income | Uncomp. Wage |
|---|---|---|---|---|---|---|
| Heckman (1976) | 1966 Natl Long. Survey (NLS) | Married whites 30–44 yrs | OLS | 1.48 | −0.02 | 1.46 |
| | | | FIML/Tobit | 4.35 | −0,04 | 4.31 |
| Nakamura & Nakamura (1979) | 1971 Canadian Census | Married 30–44 (avg of 3 age groups) | Selection bias adjusted | −0.01 | −0.13 | −0.14 |
| Cogan (1980) | 1961 NLS | Married whites 30–44 yrs | OLS | 1.17 | −0.03 | 1.14 |
| | | | Tobit | 3.60 | −0.10 | 3.50 |
| | | | FIML/Tobit | 2.91 | −0.09 | 2.83 |
| Heckman (1980) | 1966 NLS | Married whites 30–44 yrs | OLS | 2.26 | 0.00 | 2.26 |
| | | | Sel bias | 1.47 | 0.00 | 1.47 |
| Ashworth & Ulph (1981) | BMRB | U.K. Wives, husband <65, n l budget | OLS | −0.14 | −0.02 | −0.12 |
| | | | ML | 0.28 | −0.24 | 0.04 |
| | | | ML: complete budget | 0.77 | −0.23 | 0.54 |
| Hausman (1981) | 1975 PSID | Household heads | ML, nonlinear budget | 0.68 | −0.18 | 0.50 |
| Cogan (1981) | 1967 NLS | Married whites 30–44, entry costs | FIML/Tobin | 2.64 | −0.19 | 2.45 |
| | | | ML | 0.68 | −0.03 | 0.65 |
| Franz & Kawasaki (1981) | Census, Statist. Bundesamt | W. German wives | Selection bias adjusted | 1.28 | −0.20 | 1.08 |
| Ruffell (1981) | BMRB | U.K. wives husband <65 | OLS | 0.04 | −0.04 | −0.00 |
| | | | ML | 0.51 | −0.08 | 0.43 |
| Nakamura & Nakamura (1981) | 1970 U.S. Census | Married 30–44 (avg 3 groups) | Selection bias adjusted | 0.06 | −0.27 | −0.22 |
| | 1971 Canadian Census | Married 25–59 (avg 7 groups) | | 0.07 | −0.16 | −0.10 |
| | | Married 30–44 | | 0.08 | −0.25 | −0.16 |
| | | Married 25–59 | | 0.10 | −0.25 | −0.15 |
| Ransom (1982) | 1976 PSID | Married, husbands 30–50 | ML | 0.48 | −0.07 | 0.41 |
| Blundell & Walker (1982) | Family Expend. survey | U.K. wives | ML-husband hrs not rationed | 0.33 | −0.22 | 0.11 |
| | | | ML-husband hrs rationed | 0.33 | −0.19 | 0.14 |

*Continued*

**Table 2.** (*Continued*)

| Author | Data | Study Characteristics | Estimation[1] | Elasticity Estimates: Comp. Wage | Income | Uncomp. Wage |
|---|---|---|---|---|---|---|
| Hanoch & Honig (1983) | 1969–75 Retirement History Survey | Single, 55+ in 1969, 4 panels | 3SLS for wages, hrs, wks; sel. bias | 0.21 | −0.06 | 0.15 |
| Hausman and Ruud (1984) | 1976 PSID | Married, husbands 25–55 | ML, nonlinear budget | 1.12 | −0.36 | 0.76 |
| Moffitt (1984) | 1972 NLS-Older Women | | ML | 0.71 | −0.28 | 0.43 |
| | | | ML, endog wages, n l budget | 0.39 | −0.18 | 0.21 |
| Smith & Stelcner (1985) | 1981 Canadian Census | Married 20–54 | Selection bias adjusted, tax endog | 0.22 | −0.13 | 0.09 |
| Stelcner & Breslaw (1985) | 1979 Survey of Consumer Finance | Quebec wives 20–54, tax illusion | sel. bias-OLS | 0.49 | −0.09 | 0.40 |
| | | | GLS | 0.50 | −0.09 | 0.41 |
| Yatchew (1985) | 1975 PSID | Married | ML, n l budget | 1.36 | −0.89 | 0.47 |
| Prescott *et al.* (1986) | 1974 Mincome baseline | Winnipeg, low income, single vs. married | Selection bias adjusted | 5.83 to 8.22 | −5.93 to −7.48 | 0.35 to 0.74 |
| Ransom (1987b) | 1976–8 PSID | Married, working husbands 30–50 | ML (simultaneous Tobit) | 0.73 | −0.16 | 0.57 |
| Blomquist & Hansson-Brusewitz (1990) | 1981 Level of Living Survey | Married or cohabiting Swedish women 15–75, n l budget | FIML (full sample) | 0.79 | −0.24 | 0.55 |
| Bourgignon & Magnac (1990) | 1985 INSEE Labor Force Survey | French wives 18–60, salaried jobs when working | Tobit | 0.3 | −0.2 | 0.1 |
| | | | ML-n l budget | 1.0 | −0.3 | 0.7 |
| Colombino & Del Boca (1990) | 1979 Survey of Turin | Wives in Turin with working husbands | ML-n l budget | 1.18 | −0.66 | 0.52 |
| Triest (1990) | 1984 PSID | Wives 25–55, n l budget | ML-working wives | 0.42 | −0.16 | 0.26 |
| | | | -all wives | 1.31 | −0.32 | 0.99 |

*Continued*

**Table 2.** (*Continued*)

| Author | Data | Study Characteristics | Estimation[1] | Elasticity Estimates | | |
|---|---|---|---|---|---|---|
| | | | | Comp. Wage | Income | Uncomp. Wage |
| van Soest *et al.* (1990) | 1985 Org of Strategic Labor Mkt Research Survey | Dutch wives-n l budget | ML-hrs constrained | 1.02 | −0.23 | 0.79 |
| | | | -hrs un-constrained | 0.79 | −0.20 | 0.59 |

Notes: Estimates are an average if more than one similarly constructed point estimate is reported.
[1]Linearized budget constraint assumed unless otherwise stated.

but insignificant for husbands and wives (Ransom, 1987a and 1987b; Prescott *et al.*, 1986; Baffoe-Bonnie, 1989). The exception is Hausman and Ruud (1984) who find a significant elasticity of −2.36 when they account for the nonlinear and nonconvex budget constraints facing wives. Bourgignon and Magnac (1990) estimate a family labour supply model for France, correcting for simultaneity, selectivity, and budget nonlinearities, but they conclude that their results are 'disappointing' because they yield negative compensated wage elasticities.

The data from the income maintenance experiments provide an alternative opportunity to investigate labour supply response, as discussed in section 5.2. One problem, however, is that structural labour supply models were not estimated in the first three U.S. experiments (New Jersey, the Rural Experiment, and Gary), only in the Seattle-Denver Experiment, so that wage and income elasticities have to be inferred from the results. Nevertheless, Robins' (1985) summary of the results seems quite conclusive: the labour supply elasticity estimates are uniformly quite low for males and females. Robins finds that the compensated wage elasticity varies from −0.09 to 0.19 for men with a mean of 0.08, from −0.08 to 0.42 for wives with a mean of 0.17, and varies from −0.04 to 0.29 for single mothers with a mean of 0.13. His income elasticity estimates vary from 0.06 to −0.30 with a mean of −0.10 for men, from 0.26 to −1.32 with a mean of −0.06 for wives, and vary from −0.07 to −0.46 with a mean of −0.16 for single mothers. Thus the mean uncompensated wage elasticity is −0.02 for males, 0.11 for wives, and −0.03 for single mothers. Keeley (1981) and Robins (1985) conclude that the experimental elasticity estimates are more uniform, more consistent with economic theory, and generally smaller for married women than the results from nonexperimental cross-sectional data, regardless of econometric vintage. Recent results from the Mincome experiment in Canada provide additional support for this conclusion, with compensated wave elasticities which never exceed 0.3 for men and 0.0 for wives and income elasticities which never lie below −0.01 for men and −0.2 for wives (Hum and Simpson, 1991). Hum and Simpson also find a small and insignificant compensated cross-wage elasticity for husbands, but a larger and often

significant elasticity with a mean of 0.9 for wives, a result which deserves further attention.

Why are the experimental results so much more conclusive than the nonexperimental results? Mroz (1987) provides one answer. He investigates the economic and statistical assumptions of the bulk of the nonexperimental research using the 1975 Panel Study of Income Dynamics for married women. His specification tests reject the assumption that the wife's wage rate is exogenous, likely because unobserved person-specific differences yield a spurious positive correlation between wages and labour supply. He also rejects the assumption that the wife's labour market experience can be used as an exogenous instrumental variable to correct the bias arising from the endogeneity of the wife's wage rate, because past labour market experience is similarly correlated with wages and current labour supply through person-specific differences. Hence numerous second-generation studies that use this procedure will produce biased estimates. Mroz also rejects the Tobit assumption used to incorporate nonworkers into the labour supply model, finding that Tobit estimates exaggerate both wage and income effects, a result supported by Ehnes and Slmpson (1983).

Ruling out any previous studies that either treat the wife's wage as exogenous, or use labour market experience as an instrumental variable to control for the wife's wage, or apply the Tobit model for labour supply, Mroz is left only with studies that find low elasticity estimates, such as Boskin (1973) or Nakamura and Nakamura (1981). Mroz's own results for acceptable specifications are also at the lower end of the range of second-generation results from nonexperimental data: 0.23 to 0.85 for the uncompensated wage elasticity and 0.03 to 0.11 for the income elasticity. He thus concludes that the range of estimates from previous studies using nonexperimental data is misleading because of common specification errors associated with person-specific unobservable effects, and that the labour supply response of wives is similar to the response estimated for males. Less detailed, but similar conclusions are also obtained by Jakubson (1988).

These results suggest one convincing explanation for the greater homogeneity of results in the experimental data. After-tax wage and income variation in the experiments do not arise from spurious correlation between these variables, person-specific differences and labour supply. Hence the estimates should be more reliable. In particular, structural labour supply models of the sort provided by equation (15) avoid the most serious specification errors found by Mroz. The rationale for the experiments then — to provide superior data and more reliable estimates of labour supply response for policy analysis — would seem to be justified in retrospect by the empirical literature to date. Mroz (1987, 794–795) views the income maintenance experiments as a 'strong test of the robustness of the results' from his study. He calls for further analysis of the experimental evidence, particularly the estimation of structural labour supply models like equation (23), as well as careful specification testing whenever nonexperimental data is used.

## 6. Concluding observations

We have argued that labour supply research provides a particularly useful area to study the effectiveness of conventional (positivist?) economic methodology and its contribution to policy development. Our assessment of the situation from the viewpoint of labour supply research is both optimistic and cautious. First, we are optimistic that careful empirical analysis, backed by rigorous economic theory, can produce strong tests of that theory as well as useful results for prediction and policy analysis. In the case of labour supply, a quarter century of research with improving econometric technology and data sets has significantly narrowed the range of estimates of the wage and income elasticities to the point where economists and policy analysts should have considerable confidence in the theory and its crucial parameter values. Unless policy design is acutely sensitive to the estimates of these parameters, there should now be considerable basis for agreement on the outcomes of tax and transfer policy initiatives in general and alternative income maintenance programs in particular, at least for low-income households in North America.

We are cautious, however, because there are many hidden pitfalls in the methodology. The academic literature on labour supply estimation is strewn with econometric refinements associated with what now appear to be seriously misspecified models. In particular, there are numerous dangers associated with the specification of models for nonexperimental data which, by its very nature, requires many underlying assumptions to relate the specified theory to the data available. One approach, of course, is to encourage, even demand, more and better specification testing, and several journals now instruct referees to consider the extent to which the submitted paper performs appropriate specification testing. We wonder, however, if this step alone will be effective. Another step is to consider carefully the data requirements of the problem at hand, something that economists rarely do. In the case of labour supply, consideration of the problem, particularly the treatment of person-specific unobservable effects, points toward panel data and experimental evidence as an important check on the reliability of nonexperimental results, however well the specification is tested. While experimental data is expensive and subject to its own unique problems of interpretation, it has proven to be a valuable check on labour supply results from nonexperimental data and has allowed researchers to identify crucial specification problems in nonexperimental research. Both economic and econometric theory are notoriously silent on the issue of data collection to improve policy decisions, but we see this issue as a fundamental one if real progress in understanding economic behaviour, as opposed to publishing articles in refereed journals as rapidly as possible, is the ultimate objective.

How well do these observations fit other areas of economic research? We believe that they fit quite well. In the evaluation of employment training, for example, there is also the growing realization that analysis of nonexperimental data is inadequate because of potential specification bias (Ashenfelter, 1987). This has led to proposals for randomized trials to provide credible tests of the

effectiveness of employment programs (Burtless and Orr, 1986; Ashenfelter, 1987). The point again is that conventional economic approaches contain many pitfalls in assessing the empirical relevance of theory and measuring behavioural response. These pitfalls may defy economic and econometric innovation and require careful assessment of the capability of existing data to provide reliable results. They may even require economists to become involved in the specification of the data requirements for policy design. This appears to be happening in the evaluation of employment training, as it has in the measurement of labour supply response, with considerable potential benefit to our understanding of how labour markets work.

## Notes

1. Hammermesh and Rees (1988), Gunderson and Riddell (1988), and Hoffman (1986) are recent textbook examples.
2. Keeley (1981) provides several applications. Interest in the evaluation of the effects of taxation on labour supply has been particularly keen in recent years. See, for example, Blundell *et al.* (1988) for the U.K., Blomquist and Hansson-Brusewitz (1990) for Sweden, Bourguignonand Magnac (1990) for France, Colombino and del Boca (1990) for Italy, Triest (1990) for the U.S., and van Soest *et al.* (1990) for the Netherlands. All but Blundell *et al.* (1988) are contained in a special issue of the *Journal of Human Resources* 25, summer, 1990.
3. If the errors are assumed to be normally distributed, for example, probit regression provides consistent estimates for the participation model.
4. The replacement of the inverse Mill's ratio by a proxy variable introduces heteroskedasticity, which is normally corrected as well. Alternatively, the method of maximum likelihood can be used to estimate the system of wage, labour supply and participation (sample selection) equations simultaneously to improve efficiency.
5. A rare exception is the 1986 Tax Reform Act in the U.S. For an overview of initial research on the effect of this shock, see Bosworth and Burtless (1992).

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