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LABOR SUPPLY ESTIMATES FOR PUBLIC POLICY EVALUATION*

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Abstract

In recent years, the study of labor supply has occupied the attention of a large number of economists. With the growth in interest in the topic and with the inevitable diversity of economic models and statistical methods proposed by new entrants in the field, the literature has developed its own folklore. The principal legend is that the empirical estimates of the same parameters obtained from the set of available studies display such diversity that they are of little use to policy makers. This paper disputes the folklore. We claim that there is more agreement than disagreement once a few reasonable criteria based on recent theoretical work are used to eliminate certain studies from consideration, and once we are careful about posing the question we seek the estimates to address.

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LABOR SUPPLY ESTIMATES FOR PUBLIC POLICY EVALUATION

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In recent years, the study of labor supply has occupied the attention of a great number of economists. Both the availability of new sources of data, and the recurring desire of policy makers to be able to estimate the labor supply response to alternative programs of welfare reform have combined to stimulate an immense volume of theoretical and empirical work on the topic. Elaborate "experiments" have been conducted in order to provide estimates of responses to social programs.

With the growth in interest in the topic and with the inevitable diversity of economic models and statistical methods proposed by new entrants in the field, the literature has developed its own folklore. The principal legend is that the empirical estimates of the same parameters obtained from the set of available studies display such diversity that they are of little use to policy makers. This argument was advanced in defense of the enormous cost of collecting "experimental" data to resolve the apparent ambiguity in the empirical evidence on labor supply behavior. The fact that analyses of experimental data have produced a range of estimates comparable in width to those produced by more traditional data sources apparently only serves to strengthen the original observation.

This paper disputes the folklore. We claim that there is more agreement than disagreement once a few reasonable criteria based on recent theoretical work are used to eliminate certain studies from consideration, and once we are careful about posing the question we seek the estimates to address.

I. The Use of Labor Supply Functions in Policy Analysis

The needs of policy analysis are very specific. The goal of the policy evaluator is to estimate the response to programs that have been proposed but have never been observed in operation. To make such estimates, it is necessary to adopt a model--either explicitly or implicitly--in order to predict likely policy impacts.

One way to justify the widespread appeal of the negative income tax experiments

was that they offered a "model free" approach to the evaluation of policy. If an "experiment" could be conducted that closely resembled a proposed program, no model building was required in order to assess the impact of the program. As is evident from the literature this hope was illusory.[1] It is now clear, especially in the work of Hausman and Wise[2], that experimental data require as much and possibly more care in their analysis than traditional data, in large part because of initial administrative decisions used to create samples and because of self-selection decisions by experimental participants. It is now widely recognized that the experiments did not and could not directly estimate the likely impact of a widespread long duration negative income tax.

Like it or not, we are stuck with the need for a model to interpret data and to make policy forecasts. However, no single model can be said to be universally accepted by economists who work on labor supply. Nonetheless, the theoretical model that underlies most interpretations of data in this field is the neoclassical theory of consumer choice under certainty. Most workers in the field adopt it as the starting point, and indeed, it seems that every paper now written on the subject demonstrates to the reader the point--already apparent to Hicks[3]--that the economics of time is a special case of the theory of consumer choice. This is not to say that the basic model has not been extended; indeed, there is a considerable volume of activity associated with such extensions. However, we stick to the old model--on which considerable empirical evidence has been accumulated--and do not shift to each new model that comes along until evidence is accumulated that a new model is a genuine empirical improvement on the old.

The working assumptions in the neoclassical model as <u>conventionally applied</u> in practice are (1) that consumers face an exogenous gross wage rate (thus, tax rates, equalizing differentials payments, and the like are ignored); (2) the appropriate theoretical time dimension for the analysis is (conveniently) the one at the analyst's disposal, e.g., annual hours of work, participation in a week, etc. (thus, life cycle

considerations are ignored, as are interrelationships among different dimensions of labor force activity); (3) labor supply behavior can be characterized by the classical theory of consumer choice, complete with its optimality conditions that set marginal benefits equal to marginal cost (thus, fixed costs, nonlinear budget sets, corner solutions, and the like are ignored, and unemployment and uncertainty are neglected).

All of these assumptions have been challenged in new papers in the literature. But because much of this work is so new, little consensus on empirical estimates from the new models has emerged. In our view, this lack of agreement is only a transitional phenomenon. Unknown to most practitioners in the field, a consensus in fact exists in the studies based on the conventional analysis.

It is by now well known that estimates from the neoclassical model can be used to predict the labor supply response to social programs. For example, Masters and Garfinkle[4] and Ashenfelter[5] demonstrate how estimates of the standard labor supply function can be used to predict (a) participation in a negative income tax program, and (b) the reduction in hours worked by participants. In view of this work, we spare the reader a restatement of this point. Instead we ask the question "which of the available estimates should be used in such simulations?" In this regard, it is helpful to focus attention on a few studies for prime age males enshrined in a survey by Cain and Watts[6] that are based on the traditional model. The labor supply response of prime age males is of central concern to policy makers and has received the most attention in the empirical literature. For these reasons, as well as for the sake of brevity, we focus attention on this group. The Cain-Watts estimates are presented in Table 1. All of these studies share the following features in common: (a) they are based on cross section survey data; (b) they are for prime age males (the Fleisher, Parsons and Porter study is based on older males 45-59, while the other studies are more broadly based); (c) "cross effects" of wife's labor force activity on husband's hours of work are ignored; (d) all studies focus on an annual measure of labor supply.

The diversity in the estimates is enormous. But how many of these estimates would

TABLE 1

LABOR SUPPLY ELASTICITIES FOR "PRIME AGE" MALES[a]

Author	Uncompensated Wage Elasticity	Compensated Substitution Elasticity	Total Income[b] Elasticity
1. Ashenfelter-Heckman (in Cain-Watts)	-.15	.12	-.27
2. Boskin	-.07	.10	-.17
3. Fleisher-Parsons-Porter[c]	-.19	.04	-.23
4. Greenberg-Kosters	-.09	.20	-.29
5. Hall	-.18 to -.45	.06	-.24 to -.51
6. Hill	-.21 to -.34	.47 to .52	-.68 to -.86
7. Kalachek-Raines	.55	.86 to .96	-.31 to -.33
8. Masters-Garfinkle[d]	.01 to -.11	-.04 to .06	-.06 to -.12
9. Rosen-Welch[e]	-.27	.14	-.41

[a]Adapted from Cain and Watts, Table 9.1, pp. 332-333. For references and descriptions of the papers see Cain and Watts.

[b]This is defined as $W \cdot (\partial h/\partial Y)$ where W is the wage rate, $(\partial h/\partial Y)$ is the effect of a change in unearned income on hours worked. See Cain and Watts.

[c]These estimates (reported by Cain and Watts) are only one of the many estimates reported in this study.

[d]This study replaces Garfinkle's analysis in the Cain and Watts volume with Garfinkle's latest estimate of these effects reported in Masters and Garfinkle, Table 5.7, p. 95.

[e]This is the estimate for urban workers using hourly wage rates and annual hours worked (Table 1, Col. 1 of Rosen and Welch), rather than the estimate reported by Cain and Watts which was based on a regression of annual hours on weekly earnings. As Rosen and Welch point out, this latter procedure leads to an upward bias in the estimated wage effect.

we judge to be of interest in the light of recent work? To answer this question we first introduce some new results from the literature.

II. A Summary of Recent Results

The neoclassical theory establishes a relationship between hours of work (h), wage rates (W) and unearned income (Y). This relationship can be written for a consumer who equates marginal benefits to marginal costs as:

$$(1) \qquad h = \alpha_0 + \alpha_1 W + \alpha_2 Y + \bar{\varepsilon}$$

where ε is a portmanteau variable of unobservables. Estimates of the parameters in this function are required to perform the policy simulations mentioned earlier. All of the studies listed in Table 1 purport to estimate α_1 and α_2. It is our contention that for three reasons not all studies, in fact, estimate these parameters.

(a) Sample Inclusion Criteria and the Choice of the Dependent Variable

All of the studies listed in Table 1 claim that "ε" is uncorrelated with W and Y. But in many studies this claim is untenable. Consider, for example, the study by Kalachek and Raines. In this study, households with income greater than twice the Social Security Administration's low cost budget income level were excluded. The rationale for this exclusion is that the authors seek to explore the labor supply behavior of poor people. The implicit notion behind this restriction is a "culture of poverty" concept--that poor people have different labor supply behavior than others, and that poverty is a static concept.[7] The work of McCall[8] belies the second argument--there is a lot of turnover in and out of poverty. The first argument may be correct, but the authors run the risk of manufacturing selection bias by using only a sample of poor men on which to estimate their functions.

To illustrate, suppose ε is a random variable with mean zero in a random sample of data. Restricting an empirical analysis to poverty samples, we select from a random sample of men with identical values of W and Y those men with a lower than average value of ε. This is so since poverty is defined by total income I,

$$I = Wh + Y = \alpha_0 W + \alpha_1 W^2 + \alpha_2 WY + Y + W\varepsilon$$

Holding W fixed and increasing Y, the average value of ε must decrease for the selected sample if consumption is a normal good. Moving across groups with the same Y, but higher values of W, implies that the average value of ε must decrease provided that certain empirically plausible conditions hold.[9] To summarize, a regression fit on a selected sample may be written as

$$(2) \qquad h = \alpha_0 + \alpha_1 W + \alpha_2 Y + E(\varepsilon | \text{selection rule}) + V$$

where "$E(\varepsilon|$selection rule$)$" is the expected value of the unobservables for the selected sample. This term decreases with Y and with W suggesting that estimates based on "poverty samples" will tend to produce downward biased estimates of α_1 and α_2 since the analyst ignores how the sample is generated and hence omits the term "$E(\varepsilon|$ selection rule$)$" from his equation. "V" is uncorrelated with the other right-hand side variables by construction.

The papers numbered [3], [4], [6], [7], and [9] in Table 1 present labor supply estimates based on low income samples, and hence suffer from this bias. Given the very high income cut off employed in studies [3] and [4] we feel that the income truncation problem is less pronounced in these studies. One cannot, a priori, say that there is no merit in stratifying samples. People who are poor have different labor supply behavior than others. Studies by Heckman[10] and Burtless and Hausman[11] reveal considerable dispersion in preferences for work. However, inducing selection bias can manufacture apparent differences in estimated labor supply parameters between rich and poor.

There is a related <u>inclusion bias</u> that affects the analysis in some of the other papers: the choice of dependent variable used in the empirical work. For example, Hall seeks to explain the labor supply of all workers. If a worker does not work any hours, he receives a zero in the Hall analysis and is pooled in the sample with workers. As noted by Lewis[12], Ben-Porath[13], and others, the participation function differs structurally from that of the hours of work decision. The probability that someone participates is P(W, Y). The effect of a change in W and Y on P is <u>not</u> the same as the effect of a change in W and Y on h given in equation (1).[14] The hours of work function for workers is given by equation (2) with the selection rule being "some

work in the survey period."

The Hall paper essentially estimates a regression approximation to

$$(3) \qquad h = P(W, Y)(\alpha_0 + \alpha_1 W + \alpha_2 Y + E(\varepsilon \mid \text{"some work"}) + V)$$

$$= \beta_0 + \beta_1 W + \beta_2 Y + V^*$$

Hall's estimates of β_1 and β_2 do <u>not</u> correspond to α_1 and α_2, respectively. His estimates confound parameters of the participation function with the parameters of the true structural hours of work function. It is the latter that are required for policy analysis.[15]

Superficially, it would seem that this consideration is more important for the labor supply of secondary workers than it is for the labor supply of prime age males who have high participation rates. However, DaVanzo, DeTray and Greenberg[16] demonstrate that in their sample of prime age males, adding in "zero hours worked" observations into the sample raises the estimated value of the wage effect on labor supply. Their evidence is consistent with the notion that participation probabilities are related to wage rates.

To summarize, there are two sources of bias: (a) "zero hours of work" observations do not lie on the structural labor supply function, and (b) restricting estimates to subsamples of individuals with positive hours of work may result in selection bias. The method proposed by Hall of pooling "zeros" with continuous observations has no analytical justification, and the evidence in DaVanzo et al suggests that estimates based on his procedure overstate the true value of the structural wage elasticity.

A related point about sample inclusion bias can also be made about other definitions of labor supply used in the literature. For example, the Masters and Garfinkle estimates reported in Table 1 are based on a labor supply measure obtained by adding hours worked to hours unemployed (or on strike) for labor force participants. This is an appropriate measure only if individuals are surprised by involuntary unemployment (or strikes). An alternative view of unemployment advanced by Lucas and Rapping[17] views unemployment time as another form of leisure activity so that only measured hours of work are relevant in

estimating labor supply functions. A third view of unemployment as search activity advanced by Burdett and Mortensen[18] suggests a separate equation for unemployment time. There is no convincing evidence on which of these three views is correct.[19]

If, in fact, the behavioral function that characterizes unemployment differs from that of hours worked, the Masters-Garfinkle estimates are a weighted average of the two functions. This combined function is of little structural interest and certainly is not a basis for providing estimates of equation (1). This point highlights a glaring omission in the theoretical model currently used to evaluate policy--it ignores unemployment. The model of Burdett and Mortensen provides the first step towards a framework that accommodates labor supply and turnover behavior.

There is another source of sample selection that is somewhat more subtle. Studies [4], [7], and [9] defined annual labor supply as annual weeks worked (or in the labor force), times hours worked in the week preceding the survey. As noted by DaVanzo et al (pp. 95-96), if a worker did not work in the week preceding the survey his annual hours are estimated to be zero. If higher wage workers are more likely to have worked in the survey week, this source of measurement error--or selection bias--results in an upward biased estimate of the effect of wages on labor supply. DaVanzo et al present evidence that this point is empirically relevant.[20]

(b) Use of Work Related Transfers as Unearned Income

Since the original observation by Mincer, economists have been warned against using transfer payments and income transfers that are a result of labor supply choices as a determinant of those choices. Such a procedure builds a spurious negative relationship between income (as measured) and labor supply. Studies [6] and [9] include welfare and unemployment payments in Y and not surprisingly estimate large negative income effects. The remaining studies are not entirely clean on this point either. Since measured unearned income is largely a consequence of past work effort, it is likely that it is correlated with error "ε" in equation (1). To purge this bias, Greenberg and Kosters (study [4]) and Ashenfelter and Heckman (study [1]) use instrumental variables in an attempt to correct for any bias that results from this source.

(c) Measurement Error in W and Y

Survey data are ridden with error. It is well known that data on unearned income is measured with error. Unless instrumental variable methods are used (or the measurement error is somehow corrected) as in studies [1] and [4], the estimated value of the income term is biased toward zero.

Studies [6], [7], [8], and [9] define the wage rate as the ratio of earnings (in a time unit, usually a year) to labor supply. If labor suply is measured with error, the effect of the use of this measure is to bias the estimate of α_1 downward.

Other studies ([1], [2], [4], [5]) based on the Survey of Economic Opportunity (SEO) data are not entirely free of measurement error either. The wage measure used in these studies is constructed by dividing "normal" weekly earnings by actual hours worked in the survey week. Apart from the fact that this variable is not available for workers who supply no hours during the survey week, error is induced by transitory fluctuations in hours of work in the survey week, biasing α_1 toward zero.

In order to circumvent the measurement error bias in wage rates, and to predict missing values of the wage, some studies ([2], [5]) predict wages by running a regression on the sample of workers for whom wage data are available to predict a wage for observations with missing values and for the balance of the sample as well. This procedure seems attractive because it appears to solve two problems at once: (a) a missing data problem, and (b) an error in variables problem. However, this claim is quite misleading. For two reasons, such "instrumental" variable estimates may, in fact, be an important source of error. First, the wage data are missing nonrandomly. Low wage individuals are the ones more likely to be missing from wage samples and hence the imputation procedure overstates the missing wage. If hours of work are correctly measured, the imputation procedure biases the estimate wage effect downward. Second, if the imputed wage is divided into the earnings to estimate labor supply (as in [2] and [5]), any error in measuring wages is transmitted to the dependent variable and hence the estimated wage coefficient has an additional downward bias.[21]

III. Our Choice of Estimates: Uncertainty Reduced

In our judgment, the evidence is sufficiently clear that studies [6] and [9] should be eliminated from consideration as a source of estimates for policy analysis. Both studies are based on "poverty samples" with the income inclusion criterion much more stringent than that used in the other studies. Moreover, they both define wage rates by dividing earnings by labor supply and hence induce a negative bias in the estimated wage effect. Finally, both also include work determined transfers in the measure of unearned income.

The study by Kalachek and Raines is unusual for its high estimated substitution elasticity. This elasticity is based on a definition of labor supply that combines the wage effect of participation with the wage effect on hours. For reasons discussed earlier, this estimate is of little interest in policy evaluation. Nonetheless, it is much higher than that estimated by Boskin who also presents an estimated substitution elasticity that combines the participation and hours decision. In our judgment, this difference arises, in large part, from the stringent low income criterion employed by Kalachek and Raines (and not employed by Boskin). Their criterion results in a sample with a lower than average participation rate. Since much evidence suggests that the participation-wage relationship is nonlinear (becoming virtually flat at very high wage rates), it is not surprising to find a much greater estimated wage-participation relationship in their analysis than in that of Boskin. These arguments lead us to drop the Kalachek-Raines estimates from further consideration.

Eliminating these three studies greatly reduces the range in the estimates. The study by Hall [6] appears discrepant. The most surprising comparison is the contrast between his estimates and those presented by Boskin [2] which are based on almost exactly the same data set. We believe this discrepancy arises from the manner in which Cain and Watts chose to summarize the Hall study. They represent an evaluation of wage effects for a given level of ("whole") income and family composition. The Boskin estimates should be interpreted as a simple summary of Hall's estimates for the full

sample, in a format comparable to the other studies in the table. This observation further narrows the range of uncertainty.

The Boskin study is based on a pooled sample of workers and non-workers. Thus it estimates an equation like (3), combining participation and structural parameters. However, in view of his virtually inelastic participation function, it is not surprising to find that his estimates are closely in accord with the estimates for studies [1], [3], and [4] that are essentially based on samples of participants. As expected, his estimated wage effect is slightly more positive than that found in the three studies just cited, but the difference is too slight to be taken seriously.

The only truly discrepant study remaining in the table is that of Masters and Garfinkle. Their estimates are derived from two data sources: the SEO and the Panel Survey of Income Dynamics (PSID). The estimates based on the PSID are dominated by one extreme observation which when removed from the sample leads to agreement in the estimates from the two samples. The SEO based estimated income elasticity (-.06) and uncompensated wage elasticity (.01) are larger than the remaining estimates in the table. Much of this discrepancy can be traced to Masters and Garfinkle's definition of labor supply as the product of weeks in the labor force times 40 if the observation normally works "full time" or would like to work full time, or 20 if the individual voluntarily works part time. This treatment of hours worked per week flattens the estimated relationship between hours per week and wage rates--a relationship known to be negative for the wage measures used by Masters and Garfinkle (see DaVanzo et al)--and hence results in an upward bias in their estimated wage effect. Further, their measure gives greater play to the weeks-wage relationship which is known to be positive. Similarly, if unearned income reduces hours worked per week--as the theory predicts--their rather unusual treatment of hours worked per week leads to an understatement of the wealth effect. For these reasons, we drop the Masters-Garfinkle results from further consideration.

When this is done, the agreement in estimated elasticities is much closer than is assumed to be the case. The range in uncompensated wage elasticities is from -.19

to -.07. The range for income elasticities is -.29 to -.17. These estimates imply that the effect of a negative income tax of a $2400 income (1966 dollars) guarantee and a 50 percent tax rate on covered male labor supply would be to reduce male labor supply by 8 to 15 percent in covered families.

IV. Summary, Conclusions and Qualifications

This paper has demonstrated that independent estimates of prime age male labor supply functions based on cross-section data display less diversity in the estimated coefficients than is commonly assumed to be the case once a few reasonable criteria are applied to evaluate existing studies.

We have focused our attention on the labor supply behavior of the group most frequently analyzed--prime age males--and have deliberately kept to the traditional model most often utilized to interpret labor supply behavior. In choosing this demographic group, and the most elementary model of labor supply, we have abstracted from a host of problems discussed extensively elsewhere.[22]

Nonetheless, we find that the agreement among the reasonable estimates recorded in Table 1 is remarkable especially in view of different samples used, treatment of taxes, and control variables employed in the surviving studies. In our judgment, the range of admissible estimates could be and will be further eliminated as the data, theory, and empirical technique improve.

However, it is important to note that each study in our table can be faulted. We have pointed out these flaws, and have eliminated the most flagrantly biased estimates. We have not used all the criteria outlined in Section II to eliminate the studies under consideration. The agreement in the remaining studies may arise either from the lack of practical importance of the potential defects we have mentioned, or because of a happy coincidence of offsetting errors.

FOOTNOTES

*University of California, Santa Barbara; and University of Chicago and National Bureau of Economic Research, respectively. In preparing this paper we have benefitted greatly from three references: (a) the excellent general discussion by G. Cain and H. Watts, Chapter 9; (b) an unpublished simulation study by J. DaVanzo, D. DeTray and D. Greenberg; (c) a forthcoming survey by M. Killingsworth (references to each of these studies is given in the text). This research was supported by a NSF Grant SOC 77-27136 to the National Bureau of Economic Research.

1. See J. Pechman and M. Timpane, Work Incentives and Income Guarantees (Washington, D.C.: The Brookings Institution, 1975).

2. J. Hausman and D. Wise, "Stratification on Endogenous Variables and Estimation: The Gary Income Maintenance Experiment" (Massachusetts Institute of Technology, 1977).

3. J. Hicks, Value and Capital, (Cambridge: Oxford University Press, 1939), p. 313.

4. S. Masters and I. Garfinkle, Estimating the Labor Supply Effects of Income-Maintenance Experiments (New York: Academic Press, 1977), p. 217 ff.

5. O. Ashenfelter, "The Labor Supply Response of Male Wage Earners in the Rural Negative Income Tax Experiment," in Welfare in Rural Areas, eds. J. Palmer and J. Pechman, (Washington, D.C.: The Brookings Institution, 1978).

6. G. Cain and H. Watts, Income Maintenance and Labor Supply (Chicago: Rand McNally, 1973), Chapter 9.

7. Ibid, p. 341.

8. J. McCall, Income Mobility, Racial Discrimination and Economic Growth (Lexington, Mass.: Lexington-Heath, 1973).

9. More precisely it is required that

$$\varepsilon < (C-Y)/W - \alpha_0 - \alpha_1 W - \alpha_2 Y$$

where C is the cut off on earnings, $C-Y \geq 0$ for most observations. If $\alpha_1 > 0$, the statement in the text is correct. Otherwise, it need not be. For the studies considered in this paper, the statement is plausibly correct. In the Kalachek-Raines study, C≃ $8500

and mean Y is less than $1000. If α_1 is as small as -250 (a very small number in view of the estimates presented in Table 1) the statement in the text is valid for wage rates up to (roughly) $5.50 per hour--a high wage in the 1966 sample used by them. For wage rates in excess of this amount, the effect of an increase in W is to increase the mean value of ε in the selected sample.

10. J. Heckman, "Effects of Child Care Programs on Women's Work Effort," Journal of Political Economy (March/April 1974), pp. S136-S163.

11. G. Burtless and J. Hausman, "The Effect of Taxation on Labor Supply: Evaluating the Gary Negative Income Tax Experiment" (Massachusetss Institute of Technology, 1977).

12. H.G. Lewis, "On Income and Substitution Effects in Labor Force Participation," (University of Chicago, 1967).

13. Y. Ben-Porath, "Labor Force Participation Rates and the Supply of Labor," Journal of Political Economy, (June 1973), pp. 697-704.

14. J. Mincer, "Labor Force Participation of Married Women: A Study of Labor Supply," in Aspects of Labor Economics, ed. H. G. Lewis (Princeton: Princeton University Press, 1962), has derived a model in which the wage and income parameters for P are the same as for h. However, his model requires unacceptably strong assumptions. For a more detailed discussion, see J. Heckman, "A Partial Survey of Recent Research on the Labor Supply of Women," American Economic Review (May 1978), pp. 200-212.

15. Boskin estimates equation (2) and P(W, Y) separately. His first equation thus suffers from selection bias. The second does not yield policy parameters and his combined estimates are of little structural (and hence policy) interest. Ashenfelter and Heckman only report estimates of labor supply for individuals who work and thus commit the same error as Boskin.

16. J. DaVanzo, D. DeTray and D. Greenberg, "Estimating Labor Supply Response: A Sensitivity Analysis," (Rand Corporation, R-1372-OEO, December 1973).

17. R. Lucas and L. Rapping, "Real Wages, Employment and Inflation," in The Microfoundations of Employment and Inflation Theory, ed. E. Phelps (New York: Norton, 1969)

18. K. Burdett and D. Mortensen, "Labor Supply Under Uncertainty," (Northwestern University, 1977).

19. Recent work by F. Kalachek, W. Mellow and F. Raines, "The Male Labor Supply Function Reconsidered," Industrial and Labor Relations Review (March 1978) presents some preliminary tests between the first two models and offers evidence in support of both.

20. Some additional discussion of this problem is available in S. Sandell and P. Koenig, "Measurement Error and Its Consequences: The Case of Annual Hours of Work," (Ohio State University, 1978).

21. The labor supply function literature provides some of the best examples of the misuse of the instrumental variables method available. It is alleged to be "ego tu absolvo" of all error and empirical sin. As Cain and Watts note, application of the method requires that certain variables that determine wage rates do not determine labor supply. Often the exclusion criterion is quite arbitrary. Certain studies ([2], [5]), exclude education from the supply function in order to use it as an instrument for wage rates. One of the main findings in the DaVanzo et al analysis is that excluding education from the hours of work function and using it as an instrument to predict wage rates leads to an upward revision in the estimated wage coefficient.

22. M. Killingsworth, "Labor Supply: A Survey," (Princeton University, 1978).