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ABSTRACT

The central issue of this report is the wide range of parameter estimates of labor supply responses produced by previous econometric studies based on cross sectional data. Considerable public and private debate as to how the existing welfare system should be reformed has fostered renewed study of how welfare related government policies affect people's behavior, particularly how different welfare reform proposals would alter the division of time between market work and other activities. The task undertaken here is a sensitivity analysis of the different methods and formulations by which researchers have previously estimated labor supply responses. The approach taken is a step-by-step exploration of alternative labor supply estimating equatios that attempts to identify the independent (marginal) effect of each particular change in the form of these equations. The sensitivity of response parameters to changes in the measure of market labor supply by an individual to changes in the way an individual's wage is measured, to how non-wage related income effects are estimated, and to who is included in the estimating sample is considered. By systematically exploring what difference each of these changes makes to the parameter estimates, it is possible to isolate which factors strongly affect estimated response parameters. (Author/JM)

ESTIMATING LABOR SUPPLY RESPONSE: A SENSITIVITY ANALYSIS

PREPARED FOR THE OFFICE OF ECONOMIC OPPORTUNITY

JULIE DaVANZO DENNIS N. DeTRAY
DAVID H. GREENBERG

R-1372-OEO
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PREFACE

This report is one of four reports written under Grant No. 9088D-72-01 from the Office of Economic Opportunity. At its initiation, the project was to develop and explore a general economic framework to assess the potential effects of various proposed welfare reform measures. When the grant began (July 1972), work was divided into three phases: (1) formulation of a conceptual framework, (2) specification of necessary econometric methodology, and (3) analysis and evaluation of existing data sources and future data needs. As the possibility for welfare reform became more remote toward the end of 1972, OEQ decided that analyzing specific income maintenance plans was not an optimal use of the remaining time and funds under the grant and the plan for the remaining research was altered from one that emphasized conceptual questions to one that explored the sources of differences among existing empirical estimates of the amount of time people supply to market work, a key factor in Congressional debate on welfare reform.

This report contains the results of the last phase of the project, the sensitivity analysis of estimated labor supply parameters. It is incomplete in the sense that there was not enough time to explore all possible sources of differences among recent labor supply studies, and because it was often not possible to indicate which of the specifications tested was "correct." Future work is planned to rectify these shortcomings; in the meantime, this study should provide a useful guide to some major reasons for the divergent results obtained in recent empirical studies of labor supply.

The other reports written under this grant are:

De Tray, D. N., *A General Economic Framework for Welfare Reform*, R-1346-OEO, The Rand Corporation, July 1973.

Smith, J. P., *Family Decisionmaking Over the Life Cycle: Some Implications for Estimating Labor Supply*, R-1121-EDA/OEO, The Rand Corporation, November 1973.

Smith, J. P., *Assets and Labor Supply Over the Life Cycle*, The Rand Corporation, forthcoming.

SUMMARY

Over the past several years there has been considerable public and private debate as to how the existing welfare system should be reformed. This debate, and the alternative proposals generated by it, has fostered renewed study of how welfare-related government policies affect people's behavior, particularly how different welfare reform proposals would alter the division of time between market work and other activities. The central issue of this report is the sources of the rather wide range of parameter estimates of labor supply responses yielded by previous econometric studies based on cross-sectional data. The task undertaken here is a sensitivity analysis of the different methods and formulations by which researchers have previously estimated labor supply responses; a step-by-step exploration of alternative labor supply estimating equations is used to identify the independent (marginal) effect of each particular change in the form of these equations. We consider the sensitivity of response parameters to changes in the measure of market labor supplied by an individual, to changes in the way an individual's wage is measured, to how nonwage-related income effects are estimated, and to who is included in the estimating sample. By systematically exploring what difference each of these changes makes to the parameter estimates, we are able to isolate which factors strongly affect estimated response parameters.

For purposes of the sensitivity tests the sample consisted of married white male heads of households, aged 25-54. In the first part of the analysis, the sample was further restricted by excluding all individuals who might present special estimation problems.

Our analysis on this restricted sample using *observed* measures of wages and net worth mainly confirms what past labor supply studies have indicated:

° A negative bias is introduced into the wage coefficient when the dependent variable (or some part thereof) is used to calculate the wage measure.

° There is some weak evidence of curvilinearity in the relationship between labor supply and wages, but only at very high and very low wage rates.

° Observed net worth and the amount of time individuals work are positively, not negatively, related; the size and significance of this relationship varies substantially among the different components of net worth.

It is sometimes argued that imputed measures of wages and assets overcome many of the shortcomings and biases that may be present in observed measures; our experiment with imputed wages and an imputed measure of net worth led us to conclude that:

° The coefficient of the imputed male wage is always more positive than that of the observed wage. In the equations explaining hours worked, this result is consistent with the negative bias expected for observed wages. However, as long as the hours equations include male education as an explanatory variable, the sign of the imputed wage coefficient is always negative and only slightly smaller in absolute magnitude than the coefficient of the observed wage.

° The size and sign of the imputed wage coefficient in equations explaining hours of work are extremely sensitive to whether male education is included as an explanatory variable in the labor supply regression.

° Adjusting wage rates by cost-of-living indexes has almost no effect on estimated wage parameters.

° In equations explaining hours of work, the sign of the net worth coefficient (for both imputed and actual measures of net worth) depends on whether the male wage variable is observed or imputed but not on whether net worth is observed or imputed. When the observed male wage is used in the regression, the sign of the net worth coefficient is usually positive; but when the imputed male wage enters the regression, the net worth coefficient is negative, at least for regressions based on the restricted sample.

° Adjusting imputed wages and imputed net worth for experience or age in general changes the respective coefficients in the "right" direction -- that is, in a positive direction for the imputed wage coefficient and in a negative direction for the imputed net worth coefficient.

° Changing the specification of the regression used to impute net worth can affect the sign of the net worth coefficient in the labor supply regression; when children and home status are left out of the net worth imputing regression, the relationship between imputed net worth and labor supplied changes from negative to positive.

° The relationships between imputed wages and the two components of labor supply, weeks worked per year and hours worked per week, are often of opposite sign, as are the relationships between imputed net worth and the two labor supply components.

° Neither the husband's wage nor the net worth coefficients are sensitive to whether wife's earnings, wife's observed wage, or wife's imputed wage is entered in the labor supply regression.

Our final tests concerned the effect of sample composition on estimated wage and net worth parameters. We added back to the sample the various groups that were excluded from the initial restricted sample because they presented special estimation problems and found that:

° Both the net worth and male wage coefficients become more positive as the sample becomes less restricted.

° The most dramatic positive changes in the male wage and net worth coefficients occur when men who did not work the week preceding the survey (especially those who did not work at all in 1966) are added to the sample.

° When dummies representing the reasons for initial exclusion (for example, disability and student dummies) are included in the regression equation, there is less difference between parameter estimates for the restricted and unrestricted samples, but the wage and net worth coefficients still become more positive as the sample becomes less restricted.

° The divergence in results between studies that use observed wages and those that use imputed wages are not only due to use of different wage measures, but to differences in sample composition as well. When observed and imputed wage coefficients are computed *for the same sample*, the latter is usually more positive than the former. This difference is accentuated because studies that use observed wages must restrict their samples to those who report wages, whereas studies that impute wages tend to be less restrictive in defining their samples, usually including nonworkers.

Major differences in estimated wage and income effects can often be attributed to one of three factors: (1) the measure of labor supply used, (2) the broadness of the sample used, especially the inclusion of individuals with zero values for the dependent variable, and (3) for those studies that used imputed wages, the inclusion or exclusion of education in the labor supply regression. The resolution of the differences resulting from these factors is beyond the scope of this report, but our findings should be useful guides for future theoretical and empirical research on labor supply.

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I. INTRODUCTION

Over the past several years there has been considerable public and private debate as to how the existing welfare system should be reformed. This debate, and the alternative proposals generated by it, has fostered renewed study of how welfare-related government policies affect people's behavior, particularly how different welfare reform proposals would alter the division of time between market work and other activities. These labor supply effects determine the distribution of transfer payments under a program, the work patterns of the recipient population, and the ultimate cost of the program. Thus, the potential labor supply effects of various welfare reform proposals have played and will continue to play a major role in determining which, if any, of those proposals eventually becomes law.

In attempting to predict the labor supply effect of income maintenance programs, economists have emphasized two features: the base level of support and the "tax rate." The base level of support defines the income guarantee and is the maximum transfer a household can receive under the program. Under almost all of the proposed reform programs, the amount of transfer is reduced as more income is received from other (nonprogram) sources; for example, for every dollar a family member earns, the family forgoes some fraction of a dollar of welfare payments. The rate at which welfare support is withdrawn as additional income from nonprogram sources comes into the family is commonly referred to as the program tax rate. According to conventional economic theory, both the income transfer and the tax rate of an income maintenance program will tend to reduce the amount of market work that family members want to supply. What economists have attempted to determine is the magnitude of the change in market labor supply under different levels of transfer and different rates of taxation.

The central issue of this report is the wide range of parameter estimates of labor supply responses produced by previous econometric studies based on cross-sectional data. As is well documented in a collection of such labor supply studies edited by Cain and Watts (1973), the range of parameter estimates is far too great to make these studies of much use to policymakers trying to choose among alternative income maintenance programs. Moreover, because the methodologies of these studies vary in several ways simultaneously, it is difficult to pinpoint the exact causes of divergence empirical results. The task we undertake here is a sensitivity analysis of the different methods and formulations by which researchers have previously estimated labor supply responses. Our approach is a step-by-step exploration of alternative labor supply estimating equations that attempts to identify the independent (marginal) effect of each particular change in the form of these equations. We consider the sensitivity of response parameters to changes in the measure of market labor supplied by an individual, to changes in the way an individual's wage is measured, to how nonwage-related income effects are estimated, and to who is and who is not included in the estimating sample. By systematically exploring what difference each of these changes makes to the parameter estimates, we are able to isolate which factors strongly affect estimated response parameters. Future research can then concentrate on resolving those methodological issues that do make a difference.

Although this study examines the sensitivity of estimated labor supply response parameters to many of the methodological choices made in the past, we make no claim to testing the sensitivity of the parameters to every possible methodological permutation. Time and resource constraints forced us to make certain arbitrary decisions about what to explore. We concentrate on approaches frequently used in previous research and in general do not attempt to advance the state of the art. Moreover, we do not explore the sensitivity of labor supply parameters to alternative data sets or to alternative estimation techniques. In keeping with most recent labor supply research, we use the 1967 Survey of Economic Opportunity and the ordinary least squares regression technique.

Section II of this report briefly surveys the alternative methodological approaches used in recent labor supply studies and examines their possible shortcomings. Our sensitivity tests of these different approaches are presented in Section III. Section IV contains our conclusions and recommendations for future work.

II. A COMPARISON OF RECENT LABOR SUPPLY STUDIES

In the introduction we noted that the prerequisites to predicting the effects of income maintenance programs on labor supply are estimates of the influence of changes in tax rates and income on the hours of work of potential program participants. These estimates may be obtained through controlled social experiments or through analysis of nonexperimental cross-sectional data. Given the amount of both time and money that social experimentation requires, many researchers have attempted to obtain the necessary estimates from existing cross-sectional data sets. In this section we review recent labor supply studies that use cross-sectional survey data to point out the substantial disparities among the estimated response parameters in these studies. We then describe some major problems that researchers face when they attempt to infer from cross-sectional survey data how individuals might react to an income maintenance program.

Although nonexperimental data have been used to examine the labor supply of a number of different demographic groups, probably the greatest attention--at least in recent studies concerned with the effects of income maintenance programs on labor supply--has focused on primary workers, particularly married male heads of households in the prime working-age category. This group is of particular interest both because it supplies a large portion of the total market work effort and because under many income maintenance proposals families headed by working males would be eligible for benefits for the first time. We focus on married male heads of households since this group represents a common denominator of so many recent studies.

Table 1 presents an overview of twelve recent labor supply studies. These studies are all considered in a recent review of the labor supply literature by Glen Cain and Harold Watts (1973). Many, in fact, appear as chapters in the Cain-Watts book. All of these studies consider prime-age married males in the analysis, although in several of the studies labor supply functions were estimated for other demographic groups as well.

Table 1

OVERVIEW OF LABOR SUPPLY STUDIES THAT INCLUDE PRIME-AGE MARRIED MALES

Author	Data Source-Year	Major Groups Included in Study by Age and Family Position[a]	Principle Estimation Technique	Uses an Inputed Wage Rate Variable	Uses an Imputed Variable to Control for Differences in Preferences
Greenberg-Kosters (1970)	SEO - 1967	Married males, 14-61	Ordinary least squares (OLS)	No	Yes[b]
Garfinkel (1971)	SEO - 1967	Married males, 25-61	OLS	No[c]	No
Boskin (1970	SEO - 1967	Married males, 21 and over[d] Married females, 21 and over Female heads of families, 21 and over Male teenagers, 14-21 Female teenagers, 14-21	OLS	Yes	No
Hall (1971)	SEO - 1967	Married males, 20 and over[d] Married females, 20 and over Female heads of families Single men, 20 and over Single women, 20 and over Male relatives of family head, 20 and over Female relatives of family head, other than wives, 20 and over Male teenagers, under 20 Female teenagers, under 20	OLS[e]	Yes	No
Cohen-Rea-Lerman (1970)	CPS -1967	Males, 22-54 Youths, 16-21 Women, 22 and over Males, 55 and over	OLS	No	No
Tella-Tella-Green (1971)	SEO - 1967	Male household heads - no other earners in household[f] Married females Married persons Female household heads - no other earners in household	Cross-tabulations[g]	Yes[h]	No
Ashenfelter-Heckman (1971)	100 SMSA aggregates in Census-1960	Married males, 25-54, and married females, 25-54[k,m]	Three-stage least squares[k]	Yes[m]	No
Ashenfelter-Heckman (1973)	SEO - 1967	Married males, 25-64[p]	OLS	No	Yes[q]
Rosen-Welch (1971)	1/1000 sample in Census-1960	Married males, 25-65[n]	OLS	No	No
Kalachek-Raines (1970)	CPS - 1966	Males, 14 and over Females, 14 and over	OLS	Yes	No
Hill (1970)	SEO - 1967	Married males, 25-54[d]	OLS	No	No
Fleisher-Parsons-Porter (1971)	National Longitudinal Survey, 1966-1967			No	Yes[b]

Notes
On following page.

Notes to Table 1

[a]Labor supply parameters were separately estimated for each of the groups identified in this column.

[b]Greenberg and Kosters (1970) developed an independent variable that is intended as a control for variations in preferences for asset accumulation. Fleisher, Parsons, and Porter use a somewhat similar variable that they interpret as representing the deviation between a household's observed assets and its "desired" assets.

[c]Through most of his analysis Garfinkel excluded nonparticipants in the labor force from his sample. However, in the few regressions he did compute with such persons included, each nonparticipant was assigned an imputed wage rate.

[d]For each of the population groups studied, separate regressions were estimated for blacks and whites.

[e]Individual data were grouped into cells on the basis of age, size of household, wage rates, income, and several other variables. Regressions were then run across the means of these cells.

[f]Some of these groups overlap. Separate analyses were conducted for wives, and for a group including both single men and married men whose wives did not work. In addition, there was an attempt to calculate male and female income and substitution effects jointly for a sample that included both husbands and wives. Persons without spouses were excluded from this latter group.

[g]Individual data were grouped into cells on the basis of wage rates and nonemployment income. Income and substitution effects were estimated by comparing adjacent cells of the resulting cross-tabulation.

[h]Only the wage rates of nonparticipants in the labor force were imputed. The reported wage of participants is used.

[k]Wage and substitution effects were estimated jointly for married men and married women, subject to the restriction that the cross-substitution effect of the men's wage on the women's labor supply would be equal to the effect of the women's wage on the men's labor supply.

[m]Aggregate data for SMSAs are used. That is, the regressions were run across the mean or median values for the 100 largest SMSAs. The labor supply measures are the proportion of *all* married males and married females, 25 to 54 years old, in each SMSA who participated in the labor force in the week preceding the 1960 Census. The wage variables are the median 1959 annual earnings of all males and all females in each SMSA *who worked 50-52 weeks in 1959*. By applying these wage measures to *all* residents of an SMSA 25 to 54 years old, a wage value is in effect imputed to each, including nonworkers. Moreover, at one

point in the analysis wages are treated as endogenous, which is equivalent to using an instrumental variables estimate of the wage (an imputed wage).

[n]Separate regressions were estimated for rural and urban residents.

[p]Married males with zero hours or with nonworking wives were excluded from this sample. Separate regressions were estimated for a sub-sample that only included whites.

[q]Ashenfelter and Heckman (1973) used an imputed measure of total income (but the actual wage rate) in their labor supply regressions in an attempt to control for variation in preferences for asset accumulation.

The table suggests several important similarities among many of these studies. Seven of them, for example, use data from the 1967 Survey of Economic Opportunity and all but two use ordinary least squares regression techniques. Despite such similarities, however, results from these studies are disturbingly varied. This is indicated by Table 2 (adapted from Cain and Watts, 1973), which presents point estimates of the income and substitution effects that were obtained in each of these studies. Note, for example, that Greenberg and Kosters (1970); Garfinkel (1971); and Boskin (1970) all used the same data base and roughly the same population group, but their estimates are sufficiently diverse to lead to considerably different policy conclusions.

Divergences among the results of previous attempts to estimate labor supply schedules from cross-sectional data could result from several sources: (1) differences in the measurement of the major variables whose relationships are to be estimated--labor supply, wage rates, and household income or wealth; (2) the techniques used to control for factors other than wage rates and income that influence labor supply; and (3) differences in the sample of households or individuals used in the studies. These three sources of difference among labor supply studies are highly interdependent. For example, whether certain observations can be included in the sample is contingent on whether the wage rate and wealth variables are constructed directly from observed data or imputed.

One consequence of such interdependency is that the methodologies used in previous nonexperimental studies have varied in several ways simultaneously, making it difficult to pinpoint the causes of divergence in their empirical results. In the empirical part of this study, we attempt to remedy this by proceeding in a step-by-step fashion, so that the sensitivity of the results to choosing one methodological alternative rather than another can be demonstrated.

Before presenting results from our various sensitivity tests (see Section III), we first discuss some major problems in constructing wage, wealth, and labor supply measures and in selecting a sample population for nonexperimental labor supply studies and indicate how previous

Table 2

POINT ESTIMATES OF INCOME AND SUBSTITUTION PARAMETERS FOR PRIME-AGE MALES

Author	Description of Group to Which Estimates Apply	Substitution Elasticity[a]	Total Income Elasticity[b]
Greenberg-Kosters (1970)	Married males in labor force under age 62	.02 to .20 (authors' preference: .20)	-.15 to -.34 (authors' preference: -.29)
Garfinkel (1971)	Able-bodied married males aged 25-61 in labor force	Near 0 (sometimes positive and sometimes negative)	Near 0 (sometimes positive and sometimes negative)
Boskin (1970)	Married males aged 20-59	.10 (white) neg. (black)	-.17 (white) -.06 (black)
Hall (1971)	Married males aged 20-59 with predicted wage < $3.00/hr.	-.20 to +.39[c] weighted average = .06 (white) -.68 to +.25[c] weighted average = -.10 (black)	-.24 to -.51 (white) -.12 to -.28 (black)
Cohen-Rea-Lerman (1970)	Males aged 22-54	Negative	-.08 to +.03
Tella-Tella-Green (1971)	Males head of household aged 18-64, wage < $3.00/hr.	.16 to .45 (authors' preference: .16 to .18)	-.11 to -.38 (authors' preference: -.11 to -.16)
Ashenfelter-Heckman (1971)	Married males in 100 SMSAs, 25-54	.06[d]	-.06[d]
Ashenfelter-Heckman (1973)	Married males, aged 25-64, wives not working	.12	-.27
Rosen-Welch (1971)	Employed married males aged 25-64, income < $10,000	(range: negative to .6) average = .2 to .3	.001 to -.15
Kalachek-Raines (1970)	Males aged 24-61, incomes < $8,500	.86 (white) .96 (nonwhite)	-.31 to -.33

Table 2, continued

Author	Description of Group to Which Estimates Apply	Substitution Elasticity[a]	Total Income Elasticity[b]
Hill (1970)	Males aged 25-54. Incomes below poverty line	.47 (white) .27 (black)	-.68 (white) -.35 (black)
	Income above poverty line	.52 (white) .56 (black)	-.86 (white) -.88 (black)
Fleisher-Parsons-Porter (1971)	Urban married males aged 45-59 with earnings in 1965 and 1966, income < $10,000 (excluding transfer payments)	-.19 or .04[e]	-.08 or -.23[e]

Source: Cain and Watts, Table 9.1.

[a]When the substitution elasticity is evaluated at the means, it is defined as $(\partial L^c/\partial W)(\bar{W}/\bar{L})$, where W = wage rate and L = labor supply (for example, hours worked per year). The superscript, c, on $\partial L^c/\partial W$ refers to the compensated effect of a change in wages as opposed to the uncompensated effect (written without the superscript). The two are related as follows: (1) $\partial L^c/\partial W = \partial L/\partial W - \partial L/\partial Y(L_0)$ where $\partial L/\partial Y$ is the income effect (income coefficient in a simple linear model) and L_0 is the "prechange" or initial equilibrium value of labor supply. Income = $Y = WL_0 + Y_n$, where Y_n = nonlabor income. The substitution elasticity is obtained by multiplying (1) by $\bar{W}/\bar{L}$, that is, $(\partial L^c/\partial W)(\bar{W}/\bar{L}) = \eta_w^c = \eta_w - (\partial L/\partial Y)(\bar{W}/\bar{L})\, L_0$. It is convenient to express the last term as a weighted income elasticity (see b). Thus, $\eta_w^c = \eta_w - \eta_y(\bar{W} \cdot L_0/\bar{Y})$. If the income effect is measured as $\partial L/\partial Y_n$, then the substitution elasticity is defined as:

$$\eta_w^c = \eta_w - \eta_{y_n} \frac{\bar{W}L_0}{\bar{Y}_n} \quad \text{where} \quad \eta_{y_n} = \frac{\partial L}{\partial Y_n}\frac{\bar{Y}_n}{\bar{L}} .$$

[b]The total income elasticity (income effect from a wage change expressed in elasticity terms) is defined as $\eta_y \bar{W}L_0/Y = (\partial L/\partial Y)(\bar{Y}/\bar{L})(\bar{W}L_0/Y)$, which equals the conventionally measured income elasticity weighted by the fraction that earnings is of the income variable used. By the conventional assumption that $L_0 = \bar{L}$ and $Y = \bar{Y}$, the total income elasticity is the same whether measured as $(\eta_{y_n}) \cdot (WL_0/Y_n)$ or $(\eta_y) \cdot (WL_0/Y)$, whereas the conventionally defined income elasticity, measured as η_y or η_{y_n}, will differ depending on the sizes of the following two two fractions:

Notes to Table 2, continued

$$\bar{Y}/\bar{W} = \left(\frac{\overline{WL} + \bar{Y}_n}{\bar{W}}\right) \text{ and } \bar{Y}_n/\bar{W} .$$

The use of a total income elasticity serves to make the income parameter estimates in the various studies comparable and permits a direct comparison with the observed uncompensated wage elasticity to determine whether the sign of the unobserved substitution elasticity is positive.

[c]See footnote 1, p. 109.

[d]These elasticities pertain only to labor force participation rates. In contrast, all other elasticity estimates in these tables pertain to annual hours.

[e]The first elasticities in each column were derived from a regression in which men who received any work-conditioned nonemployment income were excluded. The second elasticities were derived from a regression in which men with certain types of work-conditioned nonemployment income (welfare payments and unemployment insurance benefits) were excluded.

studies have varied in their approaches to these problems. Our comparison of how labor supply researchers have defined their variables and selected their samples is based on a detailed review of seven studies, the results of which are reported in tabular form. In constructing the sensitivity tests, however, we have attempted to emphasize broad, basic differences in past approaches, rather than to replicate the methodologies of specific studies. In this way the number of comparisons we make can be kept sufficiently small that the more important differences in methodologies can be assessed.

THE TRANSITION FROM THEORETICAL TO MEASURABLE VARIABLES[1]

The variables that are used in a theoretical model of household behavior are often not easily defined and constructed with available data. This subsection deals with the important transition from theoretically correct variables to their empirical counterparts and with some of the biases that may be introduced in the process.

Many of the methodological problems that researchers face in constructing the variables necessary to estimate labor supply responses stem from the influence of various life cycle effects. Over their lifetimes individuals allocate their time between market work and other activities in response to their expected lifetime pattern of wages and prices of goods (Ghez and Becker, 1972; Smith, 1973).[2] In any single period a person's labor supply decision is based not only on his characteristics (for example, wage rate) in that period, but also on these in other periods.

The economic theory underlying nearly all labor supply studies (for an exception, see Smith, 1972) is really only correct for relating *lifetime average* labor supply to lifetime average wages and wealth levels. However, although the theory requires lifetime averages, the

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[1]For a fuller treatment of many of the issues discussed in this subsection, see DaVanzo and Greenberg (1973) and Greenberg (1972).

[2]Other important factors that influence the lifetime allocation of time are rates of time preference, rates of interest, and tastes for consumption and work.

data used usually contain information on individuals at only a single point in time. Since these individuals are often at different points in their life cycles, in looking across families at a point in time, we risk confusing "true" differences in labor supply behavior (differences in lifetime averages) with differences related to life cycles.[1] If the true relationship between lifetime averages is to be accurately assessed we must in some way "purge" our independent and dependent variables of intertemporal or life-cycle components. One way of empirically implementing a one-period labor supply model is to assume that hours of work supplied by family members at any point in their life cycle consists of three components--a permanent (average) lifetime level of hours worked, a component related to the life-cycle position of the individual, and an error component. Similarly, observed wages (and income) at any point in the life cycle consist of three components: an exogenous permanent component, an endogenous life cycle component, and a measurement error component. We want ultimately to have an empirical framework in which we can estimate the response of permanent hours to changes in the permanent level of wages and income. This problem can be approached in at least two ways. On one hand, the dependent variable--the amount of labor supplied--may be purged of its age-related component by including age as an independent variable in the labor supply regression. On the other hand, the independent variables of interest can be purged of *their* age components by estimating those age components and then subtracting them from the variables. In general, if permanent and time-related components are uncorrelated, these two methods should yield the same parameter estimates for wage and income effects.[2] In Section III, we test the sensitivity of labor supply estimates to these two approaches.

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[1]For a detailed analysis of the potential independent effects on labor supply of such life cycle phenomena as interest rates, time preferences, and wage paths see Smith (1972, 1973).

[2]See Appendix A for a proof of this statement.

Consideration of these intertemporal factors in labor supply studies is a relatively recent phenomenon. Labor supply economists have, of course, faced numerous other serious estimation problems, many of which are closely related to life cycle considerations. It is to a discussion of these more traditional problems of labor supply estimation that we now turn. We first discuss the major methodological problems associated with defining the two principal explanatory variables--that is, with appropriately representing the changes income maintenance programs make in wage rates and income levels. Then difficulties in constructing the labor supply measure are considered. The techniques used in the first seven labor supply studies in Tables 1 and 2 to construct these three variables appear in Table 3.

Wage Rates

Since an income maintenance program presumably represents an unexpected change in families' environments, one must have an accurate estimate of how people respond to an *exogenous* change in their market wage rate to determine the labor supply effects of program taxes on earnings. To estimate the effects of a change in wages, one needs a measure of the amount of market goods and services that can be exchanged for an additional hour of market work. Since individuals are assumed to make this decision at the margin, the marginal wage rate should be used to provide an accurate estimate of the effects of a wage change. The best that most micro data sets supply, however, is an average wage uncorrected for such factors as federal, state, and local taxes; cost of living; and nonpecuniary returns to work time. As Table 3 indicates, several researchers (for example, Hall, 1971, and Boskin, 1970) have attempted to adjust wage measures for federal income taxes and cost-of-living differentials. In Section III we explore the sensitivity of estimated wage parameters to adjustments for cost-of-living differences across geographic regions.[1]

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[1]We chose this particular aspect of the wage adjustment for two reasons. First, the sensitivity of wage parameters to changes in federal tax rates has already been considered by T. Paul Schultz in a forthcoming labor supply study. Second, it is unclear just how

Table 3

VARIABLES USED TO MEASURE LABOR SUPPLY AND WAGE AND INCOME EFFECTS IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

Author	Labor Supply Measure: Preferred Measure	Labor Supply Measure: Alternative Measure(s)	Wage Variable
Greenberg-Kosters (1970)[a]	$WKLF_y \cdot HRWRK_w$	(1) $WRWRK_y \cdot HRWRK_w$ (2) $WKLF_y \cdot (HRLF_w)^b$	$E_w \div HRWRK_w$
Garfinkel (1971)	(1) $WKLF_y$ (2) $FTLF_y$ } m	(1a) $WKWRK_y$ (1b) $WKLF_y \cdot HRWRK_w$ (2a) $FTWK_y$ (2b) $HRWRK_w$	$E_w \div HRWRK_w$
Boskin (1970)	(1) $LFP(2)_y$ (2) $(E_y \div WG)^f$ } e		$\hat{WG}_{adj}{}^g$
Hall (1971)	$(E_y \div WG)^f$		$\hat{WG}_{adj}{}^g$
Cohen-Rea-Lerman (1970)	(1) $LFP(1)_y$ [or $LFP(1)_w$] (2) $[WKLF_y \cdot (HRLF_{w\&y})^p]^q$ } n		$\{E_y \div [WKLF_y \cdot (HRWRK_{w\&y})^q]\}$
Tella-Tella-Green (1971)	$WKWRK_y \cdot HRWRK_w$		For Labor Force Participants: $E_y \div (WKWRK_y \cdot HRWRK_w)$ For Non-Labor Force Participants: $\widehat{(E_w \div HRWRK_w)}^t$
Ashenfelter-Heckman (1971)	$LFPR_w$		$\widehat{(E_y)}^u$

Table 3 continued

VARIABLES USED TO MEASURE LABOR SUPPLY AND WAGE AND INCOME EFFECTS IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

Author	Income Variable[w]: Preferred Measure	Income Variable[w]: Alternative Measure	Tests for Non-Linearities
Greenberg-Kosters (1970)[a]	$.08(HV-HD) + [.08(RV-RD) \text{ or } RENT]^{c} + .08(BV-BD) + [.08(MI+MC+BOND) \text{ or } INT]^{c} - .08(DBTI+DBT) + (ANNU+CONT+SS+PEN)$	$.08\begin{Bmatrix} HV, \text{ if } HD=0 \\ .2HV, \text{ if } HD>0 \end{Bmatrix} + \left[\left(\frac{RV-RD}{RD}\right) \cdot RENT\right] + .08(CV-CD) + INT - (12.08 \cdot DBTPY) + (ANNU+CONT+SS+PEN)$	Uses basic wage measure plus a variable that took a zero value at wages < $2 per hour, but actual value of wage rate at wages > $2 per hour.
Garfinkel (1971)	$(INT+RENT+SS+ANNU+CONT)^{d}$	$(INT+RENT+SS+ANNU+CONT) + .08[HV-HD) + CV-CD) + (BV-BD) + (MI+MC) - (DBTI+DBT) + PV]$	A step function approach: uses a set of dummy variables created by breaking the continuous wage and income measures into intervals.
Boskin (1970)	$\}[(INT+PEN+SS+ANNU+CONT) + .06(HV+RV) + .12CV[_{adj}\}^{h}$		
Hall (1971)	$\{\}\sum_{a=1}^{n}(WG_a \cdot HRFT_a) + (PEN+SS+ANNU+CONT+INT) + .06[(HV-HD) + (RV-RD)] + .12(CV-CD) + .15(DBTI+DBT) + .33BI_{adj}\} \div n\}^{k}$		Uses step functions for the wage and income variables. (See discussion under Garfinkel.)
Cohen-Rea-Lerman (1970)	$INC_y - E_y$		Uses a step function for the wage and income variables. (See discussion under Garfinkel.) Also tested for interactions between the wage and income variables.
Tella-Tella-Green (1971)	(RENT+INT+PEN+ANNU+CONT)		Aggregates individuals into cells on the basis of wage rates and non-employment income and compares adjacent cells.
Ashenfelter-Heckman (1971)	$(\widehat{INC_y})^{v}$		

Key to Table 3

Subscripts

a	:	adult members of household where a = 1, 2, ..., n
adj	:	variable adjusted for geographic differences in prices and to net out payments made under the federal income tax system
w	:	data pertain to the week before the survey
y	:	data pertain to the year before the survey
^	:	predicted or imputed

Labor Supply

FTLF	:	1, if normally worked full-time or worked part-time because of slack work; 0, if worked part-time voluntarily
FTWK	:	1, if normally worked full-time; 0, if normally worked part-time
HRFT	:	time available for market work
HRLF	:	hours in labor force per week
HRWRK	:	hours working per week
LFP	:	labor force participation
LFP(1)	:	1, if in labor force at any time during time period; 0, if not in labor force during time period
LFP(2)	:	1, if in labor force at least 26 weeks; 0, if not in labor force at least 26 weeks
LFPR	:	labor force participation rate (applicable only to grouped or aggregated data); the proportion of the group that was in the labor force at any time during the time period
WKLF	:	weeks in labor force (weeks working plus weeks unemployed)
WKWRK	:	weeks working

Wages

E	:	own earnings
ES	:	spouse's earnings
$\hat{WG}$	:	antilogarithm of imputed logarithm of hourly wage = $e^{\widehat{\ln(E_w \div HRWRK_w)}}$

Income and Assets
(the following pertain to the household as a whole)

ANNU	:	regular payments from life insurance, annuities, royalties, trust funds, etc.
BD	:	debts owed by business or farm
BI	:	business or farm income
BOND	:	value of bonds, government bonds, stocks, etc.
BV	:	market value of business or farm
CD	:	debt owed on vehicles
COMP	:	workmen's compensation, illness, or accident benefits
CONT	:	regular contributions by persons outside the household
CV	:	market value of vehicles
DBT	:	noninterest debts (debts owed doctors, hospitals, other people, utility companies, etc.)
DBTI	:	debts owed stores, banks, and lending institutions
DBTPY	:	monthly payment against DBTI and DBT
HD	:	debt owed on own home
HV	:	market value of own home.
INC	:	total income from all sources
INT	:	interest and dividend income
LUMP	:	one-time-only money receipts (net profits from the sale of assets; lump sum payments from insurance policies, pensions, funds, etc.)
LV	:	personal loans to others
MC	:	money in checking accounts
MI	:	money in interest-bearing accounts
NEY	:	nonemployment income
PEN	:	pensions
PV	:	value of other personal property (excluding clothing, furniture, etc.)
RD	:	debt owed on real estate holdings
RENT	:	rental income receipts
RV	:	market value of real estate holdings
SS	:	Social Security or Railroad Retirement
UNPL	:	unemployment insurance receipts
VET	:	veteran's disability or compensation
WEL	.	welfare receipts

Notes to Table 3

[a]Greenberg and Kosters attempt to control for the possibility that individual preferences for asset accumulation jointly determine hours worked and the assets used in the construction of the income effect variable. They divide the unexplained residual from a supplemental regression of asset holdings on age, age squared, and wages by the worker's total human and physical net worth and then introduce this measure into the labor supply function as an additional explanatory variable.

[b]$HRLF_w$ calculated by assigning a value of 40 hours to involuntary part-time workers; a value of 24 hours to those working more than 24 hours who expressed a preference for part-time work; and the value of actual hours worked to all other persons.

[c]The larger of the values enclosed by the brackets is used.

[d]Garfinkel attempted to control for non-monetarized sources of nonemployment income by entering the following as independent variables: (BV - BD), (HV - HD), (CV - CD), (MI + MC), (DBTI + DBT), and PV.

[e]A two-stage procedure is used: first the probability that a potential worker being in the labor force is estimated; then the individual's hours of work, conditional on his participation, are estimated. Thus, $\widehat{LFP(2)}$ is treated as an estimate of the conditional probability of participation.

[f]Hall includes persons who are out of the labor force in his regressions that use $E_y \div \widehat{WG}$ as a dependent variable; Boskin excludes these individuals.

[g]Predicted or imputed wages were computed from the following wage equation, which was estimated by computing separate regressions for each race-sex group, with dummy variables for each of the other characteristics:

$$\ln(E_w \div HRWRK_w)_{i,j,k,l,m,n,q,r} = \hat{\alpha}_{i,j} + \hat{\beta}_{i,j,k} + \hat{\gamma}_{i,j,l} + \hat{\lambda}_{i,j,m,n} + \hat{\mu}_{i,j,n} + \hat{\theta}_{i,j,q} + \hat{\delta}_{i,j,r}$$

where

$i = 1,2$	for white and black
$j = 1,2$	for male and female

$k = 1, \ldots, 9$	for age groups
$l = 1, \ldots, 9$	for years of education groups
$n = 1, \ldots, 12$	for SMSAs (Hall)
$m = 1, \ldots, 40$	for geographical location of labor market (Boskin)
$n = 1,2$	for U.S. and foreign residence at age 16
$q = 1,2$	for nonmember or member of a union
$r = 1,2$	for no health effect on work or some effect

The antilogarithms of the wage rates that were estimated from this equation (WG) were adjusted for geographical differences in prices and for the federal income tax. Thus, the wage measure is purportedly an estimate of the real post-tax hourly wage rate.

[h]The measure of non-wage income that appears here for Boskin is the one he explicitly describes on his page 51. However, he also footnotes Hall at this point. Since in a number of respects Boskin utilizes Hall's research, which preceded his own, it is possible that his measure may actually be identical to Hall's, minus Hall's first term.

[k]Hall's measure is the *sum* of non-labor income (including unobserved flows imputed to assets) and the annual dollar value of the time *each* adult family member has available for work. He calls this measure "whole income." For most adults HRFT, the annual time available for work, was assumed to equal 2,000 hours. Individuals in school were assumed to have 500 hours available. "Individuals reporting physical disabilities that prevented work or limited their amount of work were assigned potential hours of work between 0 and 2,000 hours according to a formula that took account of the nature and length of the disability."

[m]Both measures were used in order to separately estimate the effects of wages and income on two distinguishable dimensions of labor supply: weeks per year and hours per week.

[n]Both measures were used in order to separately examine the determinants of labor force participation and annual hours. Since the wage variables could not be constructed for those who did not work during the year prior to the survey, it was not used in regressions that explain LFP_y or LFP_w.

[p]$HRLF_{w+y}$, average hours per week in the labor force, is based on interview questions on weekly hours during both the year and the week prior to the survey. The actual estimates are given in the following table:

Labor Force Status During Week Prior to Survey	Hours Usually Worked During Year Prior to Survey	
	Usually Full-time	Usually Part-time
Full-time	reported $HRWRK_w$	30 hours
Part-time:		
Slack work	40 hours	35 hours
Other reasons:		
Usually part-time	40 hours	reported $HRWRK_w$
Usually full-time	40 hours	30 hours
Not at work	40 hours	20 hours

[q]Persons who did not work at least one week during the year prior to the survey were excluded from regressions that use $WKLF_y \cdot HRLF_{w+y}$ as the dependent variable.

[r]$HRWRK_{w+y}$, average hours worked per week, is based on interview questions on weekly hours worked during both the year and the week prior to the survey. The estimates actually used are given in the following table:

Hours Worked During Week Prior to Survey	Hours Usually Worked During Year Prior to Survey	
	Usually Full-time	Usually Part-time
Full-time	reported $HRWRK_w$	25 hours
Part-time		
Usually part-time	35 hours	reported $HRWRK_w$
Usually full-time	40 hours	25 hours
Not at work	40 hours	20 hours

[s]For those working one to thirteen weeks during the year prior to the survey, Cohen, Rea, and Lerman used a wage derived from the occupational median income for full-time workers, instead of the formula reported in Table 3.

[t]Imputed or predicted wages for non-participants in the labor force were estimated by matching such persons to workers on the basis of the following six characteristics, with each characteristic broken down into two or more categories as shown: sex (male, female); age (18-24, 25-39, 40-54, 55-64); last grade completed in school (0-7, 8-11, 12-15, 16 and over); race (white, non-white), SMSA (in SMSA, not in SMSA); region (South, non-South). Each nonworker was assigned a potential wage rate based on the corresponding characteristics of workers.

[u] Variable used is the median value for each SMSA for those in the sample who worked during the year preceding the survey.

[v] Variable used is the median value for each SMSA for those in the sample who received some sort of income during the year preceding the survey.

[w] All the observed income components that appear in these two columns pertain to the year preceding the survey.

Although wages for workers are usually reported in data sets as averages rather than in the desired marginal form, observed wages for nonworkers are seldom available at all. One approach to this problem, which we use in the first part of our empirical work, is simply to exclude these persons from the sample. However, this approach may result in an underestimate of labor supply responsiveness to income maintenance programs by precluding analysis of withdrawal from the labor force.[1] Later in the empirical work, therefore, we examine the sensitivity of labor supply estimates to an alternative approach that allows nonworkers to be included in the sample. This approach, which has been used by Tella, Tella, and Green (1971); Schultz (forthcoming); Hall (1971); and others, involves imputing a "potential wage rate" for nonworkers on the basis of the wages observed for persons of similar characteristics for whom a wage is reported.

Although imputing wage rates does permit nonworkers to be considered in labor supply studies, the imputation process itself raises a number of serious questions. One difficulty is that underlying this approach is the assumption that for a given set of characteristics those who do not work face the same potential wage rate as those who do. Since the latter group works, but the former does not, either their actual potential wage rates differ or they entertain different options for productive nonmarket use of their time (see De Tray, 1973b). In addition, for groups with relatively low labor force participation rates, such as married women, the average wage of those who work may be an upward biased measure of the average wage offer for all members of

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weg should be corrected for taxes; some studies have indicated, for example, that, taking *all* taxes into account, the tax burden in the United States is approximately proportional to income in the income classes that include most of the population. (See, for example, Tax Foundation, Inc., 1960, and Joseph A. Pechman, 1969, Table 5). If so, adjusting wages for one or two progressive taxes, such as federal and state income taxes, may cause more serious errors than would not correcting for taxes at all.

[1]Also by excluding people at one end of the range of the dependent variable, it is more difficult to discern the shape of the curve at that end. In addition a smaller sample size produces less efficient estimators.

group. A serious "selectivity bias" may be introduced into the analysis if wages are imputed to nonworking women based on a sample of working women (Gronau, 1972). Exploring the extent of the problems associated with imputing wages is beyond the scope of this study, but it is important to keep such problems in mind when the sample is extended to nonworkers.

Several biases that arise in estimating wage effects on labor supply with observed wages may be lessened if an imputed wage is used for all persons in the sample, even for the ones who have an observed wage. We will explore this possibility by comparing wage estimates based on observed wages with those based on imputed wages. It is important to recognize, however, that the imputed wage methodology is far from perfected. In practice, the empirically estimated wage equations that we and others use to impute wages account for a small to moderate proportion of the variation in observed wages (our R^2s range from .11 to .30). There is a substantial risk that in using an imputed wage we are throwing away valuable information contained in the unexplained residual of the wage equations.

It is possible nevertheless that use of imputed wages corrects for several sources of bias. The observed wage variable, for example, is usually constructed by dividing a measure of total earnings over some period by the amount of time (usually the number of hours) worked during the period. If, as is often the case, one or more of the data elements used in the denominator of the wage rate variable is also used to construct the labor supply measure, errors in reporting labor supply will negatively bias the estimated relation between labor supply and the wage rate. In Section III we examine the importance of this bias by (1) comparing results using observed and imputed wages and (2) estimating the relationships between various measures of market work and market wage, some of which are subject to this bias and some of which are not. In addition, the estimated wage coefficient is subject to the standard errors-in-variables bias. If there are errors in the wage variable that are independent of the labor supply measure (and of the disturbance term in the labor supply equation), the wage coefficient will be biased toward zero. Imputed wages will help correct for this bias also.

Even if measures of observed wages did not contain data elements also used to construct labor supply variables and were measured without error, they could still yield biased estimates of labor supply responses. In many instances wage rates may be determined jointly with other behavioral decisions. For example, certain unobserved factors, such as tastes for work, may affect both wage rates and labor supply. Moreover, as a longer time horizon is considered, wages become increasingly more endogenous because they can be affected by work experience, on-the-job training, and other investments in human capital. Imputed wages may help to correct for this simultaneity bias.

Yet another source of bias in the estimated wage coefficient may arise because current observed wage rates may be systematically related to the wages people expect to receive in the future. Since current behavior may be partly determined by future expectations (see Smith, 1972, 1973), ignoring expectations regarding future wage profiles over the life cycle introduces an omitted variable bias into the regression analysis that can seriously distort the resulting coefficient estimates. This bias cannot be removed through use of the traditional type of imputed wage and is too complex to explore fully within the context of this report. Nevertheless, in Section III, we "experience-adjust" the wage rate in an attempt to bring everyone back to the same point in their life cycles. This adjusted wage can be viewed as an estimate of the lifetime average wage, which, as indicated above (pp. 12-13), is the theoretically correct wage measure.

A related problem is caused by wages that are transitorily high or low. Transitory wage changes should yield uncompensated wage effects that are algebraically larger than those for a permanent (lifetime average) wage change of the same magnitude because the person not only substitutes work now for leisure now but also work now for leisure later. Also the income effect is less from a transitory wage change than from a permanent change. In other words, the substitution effect is larger and the income effect smaller from a one-period wage change than from a permanent change (Smith, 1973).

Imputed wages will remove the transitory elements from the wage variable but not from the labor supply measure. Only if changes in labor supply due to transitory wage changes are independent of all the control variables does the imputed wage technique completely solve this problem.

Although our empirical work concentrates on married males, the labor they supply may depend on the earnings capacities of their wives as well as on their own. This is especially important in welfare reform analysis because an income maintenance program will affect the wage rates of *all* family members. In Section III, we examine the sensitivity of the estimated labor supply functions of married males to three ways of treating the earnings capacities of their wives: wife's observed earnings, wife's observed wage rate, and wife's imputed wage rate. Each of these approaches presents some problems: earnings incorporate wife's labor supply, which is endogenous, and implicitly assume that nonworking wives have a potential wage of zero; not all women have observed wages; imputed wages for wives have the same limitations as imputed wages for males, but selectivity bias is a more serious problem for women. Although none of these approaches are completely satisfactory, they are sufficiently different that the sensitivity of male wages and income coefficients to different specifications should be discernible.

Income Effects: Nonemployment Income and Net Worth

The assumption that leisure is a superior good leads one to expect a negative relationship between income and the amount of labor supplied, other things the same. Because welfare programs will change families' incomes, an accurate assessment of the income effect is important in its own right. Past interest in measuring this effect, however, has often stemmed from the large, negative, *uncompensated wage* effect that many labor supply studies have produced. To achieve the positive *compensated wage* effect that economic theory predicts, given a negative uncompensated effect, the estimated income effect must be of the expected sign (negative) and of sufficient (absolute) magnitude to more than offset the negative uncompensated wage effect.

In some instances, estimated income effects, far from being sufficiently small (algebraically) to offset uncompensated wage effects, are of the wrong sign.[1]

The variables most commonly used to estimate income effects are: (1) nonwage (nonwork-related) income (for example, Garfinkel, 1971; Greenberg and Kosters, 1970); (2) "other family income"--the household's total money income less the earnings of the individual whose labor supply is being estimated (for example, Cohen, Rea, and Lerman, 1970); (3) total family income, including the earnings of the individual whose labor supply is being estimated (for example, Ashenfelter and Heckman, 1971, 1973); and (4) family full income--nonwage income plus the money income the family could earn if its members devoted all their time to market work (for example, Hall, 1971). When the first two income variables are used the coefficient of husband's wage is interpreted as the *uncompensated* wage effect, implying that the compensated wage effect varies with hours worked. For the third measure (total family income) the husband's wage coefficient is a direct estimate of the *compensated* wage effect. With the last income measure (full income), neither the compensated nor the uncompensated wage effect is estimated directly.[2]

For each of the four income measures nonwage income provides the variation in income that is independent of the wage rate. Therefore, to examine exogenous variations in income that are independent of changes in the wage rate, a variable must be constructed from nonemployment income flows. One problem in constructing the nonwage income variable correctly arises in distinguishing transitory sources of these flows from more permanent sources. Another problem is that much of the nonwage income families receive in monetary form are transfer payments--for example, unemployment compensation, social security, welfare, and disability insurance--that to some extent are contingent

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[1]For example, see Kosters (1969).

[2]See Appendix B for documentation of these statements.

on not engaging in market work. An additional problem is that nonmonetized returns from such assets as one's home and automobile cannot be directly observed and are not easy to measure accurately. Thus, developing the nonwage income variable usually involves imputing flows from assets by establishing interest rates for different categories of assets. However, even if an appropriate interest rate is used, total family nonwage income will be understated since the values of many assets, such as TV sets and home appliances, are not reported in most data sets. Furthermore, one might also argue that one's own children represent an asset--certainly the number of children a family decides to have may be traded off against more traditional physical assets. But here, of course, measuring the nonmonetized flow is extremely difficult. Estimates of income effects may be quite sensitive to which asset components are included in the nonwork-related income measure.

One problem with using assets, or the flow from those assets, to estimate income effects is that, at any point in time, the level of assets may be endogenous to most family decisionmaking. Because assets are generated from past labor market earnings, life cycle patterns of assets and labor supply are simultaneously determined by similar economic factors (Smith, 1973). A substantial proportion of the cross-sectional variation in labor supply and asset levels may merely be a reflection of variations in life cycle positions, time preferences, tastes for assets, or lifetime patterns of market wages, and may have nothing to do with the causal relationship between income and labor supply.

In using assets to estimate income effects, an effort should be made to purge them of their endogenous, time-related component. As with wages and labor supply, the theoretically correct measure of assets is their lifetime average level. In Section III, we construct an age-adjusted, imputed assets variable. Imputing should help correct for simultaneity bias and rid the variable of transitory components. Age adjusting should help control for life cycle variation in assets.

Since nonwage income is a component of each of the four variables used to measure the income effect, these measures are all subject to the asset-related problems just discussed. Moreover, the other family income and total family income measures present additional estimation problems because they depend on the amount of time family members spend working. The "other family income" measure includes the wife's labor supply, which is jointly determined with the husband's work decision. This income variable should be treated as endogenous in estimation. The "total family income" variable includes not only spouse's labor supply but also the dependent variable. Errors in measuring husband's labor supply will positively bias the estimated income effect based on total family income. Instrumental variables estimates of the income variable can be used to treat both these problems.

Our emphasis throughout Section III is on nonwage income because of the four income measures it is the one most commonly used in previous labor supply studies (see Table 3). In our empirical work we first determine the differential effects on labor supply of different components of nonwage income and then undertake a preliminary exploration of the direction and magnitude of the other biases discussed above by using imputed and age-adjusted income measures.

Labor Supply

In constructing their labor supply variables, researchers have debated whether to measure labor supply as hours offered or actual hours worked. Some economists have argued that because of disequilibrium in labor markets, actual hours of employment for some workers may differ substantially from their "offered hours"--the number of hours they wish to work at their received market wage rate (Greenberg and Kosters, 1970, and Garfinkel, 1971). This may have important implications for welfare reform analysis. For example, if a worker is employed fewer hours than he offers, he may not reduce his hours worked in response to an income increase until the gap between hours worked and hours offered is closed (Greenberg, 1972). In practice, however, one cannot directly observe the number of hours or weeks offered. Most often, a measure of "offered" labor supply is computed simply by adding reported weeks of unemployment to actual weeks worked (see Table 3). An unknown fraction of the time that individuals report as unemployed, however, is probably not really available to employers. Moreover, a measure of offered hours that includes unemployment plus weeks worked only permits correction of the measure of labor supply for those individuals who are working *less* than they would like. One must also consider the possibility that at any point in time, because of institutional rigidities and the like, some individuals may be working more hours or weeks than their "equilibrium level." It seems inappropriate to correct one type of disequilibrium without correcting the other.

The choice of the dimension of labor supply to analyze--participation vs. nonparticipation, weeks of work per year, hours of work per week, or hours of work per year--is often constrained by the available data. In general, an analysis of the binary decision to work or not to work seems less useful for purposes of examining the labor supply effects of income maintenance programs than research that focuses on the quantity of labor supplied as a continuous variable and assigns zero to nonparticipants.[1]

Various continuous measures of labor supply, such as weeks per year and hours per week, may reflect different aspects of the labor

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[1]Boskin (1970) and Kalachek and Raines (1970) assume that decisions on how many hours to work occur only if a decision has already been made to participate in the labor force. They have attempted first to estimate the probability that a given individual will be in the labor force and then to estimate his expected hours of work conditional on his participation. It is possible, of course, that some explanatory variables have different effects on the participation decision and on the numbers of hours supplied. For example, an increase in the fixed cost of working, such as in the cost of getting to work, may decrease the probability that an individual will participate in the labor force (a substitution effect) but increase the number of hours he will work if he does participate (he will work more hours to recoup the higher cost--an income effect). Nevertheless, unless one believes that labor supply decisions really do follow such a two-stage process, it seems more natural to integrate the participation and hours dimensions by measuring labor supply as a continuous variable, assigning a zero value to nonparticipants.

supply decision. Thus, throughout this study, we report estimates for three different measures of labor supply: hours per week; weeks per year; and the product of these two, hours per year. Hours per week focuses on the decision between part-time and full-time work, whereas weeks per year reflects the more fundamental decision of whether to work at all during a given period. If one wishes to analyze these two aspects of the labor supply decision, it is desirable to treat weeks per year and hours per week as separate dependent variables, rather than to multiply them into a single dependent variable. It may also be advantageous to treat these two dimensions of labor supply separately b[illegible]se one of the two types of information may be unavailable or unreliable for some observations; by using weeks per year and hours per week as separate variables, these observations can be incorporated into at least part of the analysis.[1]

Another reason for treating weeks and hours separately is that it may be the only way to break the spurious correlation between the observed hourly wage and hours worked that occurs when the wage is calculated by dividing weekly earnings by hours per week (see Table 3). By treating hours per week and weeks per year separately, one at least eliminates this bias in the estimated relation between wages and weeks.

CHOICE OF THE SAMPLE

Table 4 presents summary information on the sample populations that have been used in seven recent labor supply studies. Variations in the sample, such as those that are evident in the table, may account for much of the inconsistency in results among cross-sectional studies of labor supply.

In estimating behavioral parameters, researchers have given various reasons to justify their particular choice of sample

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[1] In the Survey of Economic Opportunity, for example, weeks per year are reported for persons who are self-employed, but hours per week are not.

Table 4

SAMPLE SELECTION CRITERIA IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

	Major Characteristics of Sample Examined in Table[a]			
Author	Sex	Marital Status	Age	Income
Greenberg-Kosters (1970)	Males	Married, wives present	14-61	Less than $15,000
Garfinkel (1971)	Males	Married	25-61	No limits
Boskin (1970)	Males	Married	Over 20[d]	Family "whole" income per adult member less than $5,500[e]
Cohen-Rea-Lerman (1970)	Males	Married and Single[c]	22-54	No limits
Tella-Tella-Green (1971)	Males & Females	Married and Single[b]	18-64	No limits
Ashenfelter-Heckman (1971)	Males & Females	Married	25-54	No limits

Table 4 continued

SAMPLE SELECTION CRITERIA IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

Groups Excluded from the Sample[r]
("X" Indicates Group with Characteristic was Excluded)

	Persons Whose Opportunity to Work was Restricted During Part or all of the Period Covered by the Study				
Author	Unemployed	Worked part-time Due to Slack Work	Believed They Could Not Find Work	Ill or Disabled	Institutionalized
Greenberg-Kosters (1970)					
Garfinkel (1971)				X	
Boskin (1970)				X	
Hall (1971)				X	
Cohen-Rea-Lerman (1970)				X[f]	
Tella-Tella-Green (1971)	X[g]	X[g]	X[g]	X[g]	X[g]
Ashenfelter-Heckman (1971)					X

Table 4 continued

SAMPLE SELECTION CRITERIA IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

Groups Excluded from the Sample[r]
("X" Indicates Group with Characteristic was Excluded)

Author	Recipients of Work-Conditioned Transfer Payments					
	Unemployment Compensation	Veteran's Disability Payments or Compensation	Pensions	Social Security	Welfare	Workmen's Compensation
Greenberg-Kosters (1970)	X^g	X^g			X^g	
Garfinkel (1971)	X^g		X^g		X^g	X^g
Boskin (1970)					X^g	
Hall (1971)					X^g	
Cohen-Rea-Lerman (1970)						
Tella-Tella-Green (1971)	X^g	X^g		X^g	X^g	X^g
Ashenfelter-Heckman (1971)						

Table 4 continued

SAMPLE SELECTION CRITERIA IN SELECTED STUDIES OF THE LABOR SUPPLY OF PRIME AGE MARRIED MALES

Groups Excluded from the Sample[r]
("X" Indicates Group with Characteristic was Excluded)

Author	Other Excluded Groups				
	Students	Member of the Armed Forces	Self-Employed	Did not Work During Week Before Survey	Nonparticipant in Labor Force in Year Preceeding Survey
Greenberg-Kosters (1970)		X[h]	X[k]	X	X
Garfinkel (1971)	X	X	X[k]	X	X
Boskin (1970)	X				X[p]
Hall (1971)	X		X[m]		
Cohen-Rea-Lerman (1970)	X[f]				X[f]
Tella-Tella-Green (1971)	X	X[g]	X[n]	X[g,q]	
Ashenfelter-Heckman (1971)	X[f]				

Author	Additional Remarks on the Sample
Greenberg-Kosters (1970)	Persons were also excluded from the sample if their employer at the time of the interview differed from the employer for whom they worked the longest during the year preceding the interview. While results were reported for the sample described above, additional analysis was also conducted with 36 subsamples of this sample. Results for five of these subsamples received greatest emphasis: (1) Self-weighting sample based on CPS sample frame (2) Households in which only the husband and his spouse were 18 years old or older (3) Households in which the husband was the sole wage earner (4) Households with children present (5) Households with non-institutionalized male heads
Garfinkel (1971)	Additional analysis was conducted with seven different modifications of the sample described above: The basic sample - plus (1) persons who did not work during the year preceding the survey[s] (2) pensioners[t] (3) pensioners and recipients of unemployment compensation and veteran payments[t] minus (4) persons in families whose income exceeded $10,000 (5) persons whose wage rate exceeded $3.75 per hour (6) persons in the construction, agriculture, forestry, and fishery industries (7) persons with higher than average wage rates and lower than average weeks worked
Boskin (1970)	Nonwhites other than blacks were excluded from the sample. Throughout the analysis black and white labor supply functions were separately estimated.
Cohen-Rea-Lerman (1970)	The part of the study that used $LFP(1)_y$ or $LFP(1)_w$ as dependent variables was limited to residents of the nation's 96 largest SMSAs.

Author	Additional Remarks on the Sample
Tella-Tella-Green (1971)	Additional groups excluded from the sample include: (1) Households living on farms (2) Households that added or lost a member in 1966 (3) Households with a member who worked more than 70 hours during the week prior to the survey (4) Households containing members who usually worked full-time during the year preceding the survey, but only part-time during the week preceding the survey, or vice versa The analysis was conducted with non-participants in the labor force alternatively excluded from and included in the sample. Separate analyses were conducted with two overlapping subsamples of the sample described above:[b] (1) Male heads of households with no other earners (2) Married men and their wives
Ashenfelter-Heckman (1971)	Study limited to the 100 largest SMSAs.

Notes to Table 4

[a]In several of these studies, the labor supply behavior of several different population groups were separately examined (see Table 1). In such a case, in effect, several semi-independent studies were made of several samples. The samples examined in Table 4 are those containing prime age married males, but not necessarily limited to that group.

[b]Income and substitution effects were calculated for a sample that included both husbands and wives. The estimation procedure used allows complete interaction between sex and the wage and income variables. In addition, Tella-Tella-Green (but not Ashenfelter-Heckman) separately examined a sample that included both single men and married men whose spouses did not work.

[c]The effect of marital status on labor supply functions is examined through sets of marital status-wage and marital status-income interaction terms.

[d]Persons 60 years and older are distinguished by age-wage and age-income interaction terms.

[e]For the definition of "whole income" see Table 3, especially footnote k.

[f]These persons were *not* excluded from regressions that used $LFP(1)_y$ or $LFP(1)_w$ as the dependent variable, but were excluded from regressions that used $WKLF_y \cdot HRLF_{w+y}$ as the dependent variable.

[g]Persons were excluded if *any* adult in the household--not just persons whose labor supply is being estimated--had the characteristic.

[h]Only persons who were in Armed Forces at time of survey interview were excluded from main sample. Members of Armed Forces during year preceding the interview were *not* excluded.

[k]More specifically, those excluded were persons whose primary occupation the week prior to the survey was self-employment in their own business, professional practice, or farm.

[m]All individuals in households where total self-employment income exceeded $1,000 during the year preceding the survey were excluded.

[n]All individuals in households with *any* income from business or farms during the year preceding the survey were excluded.

[p]Boskin *included* non-labor force participants in regressions which used $LFP(2)_y$ as the dependent variable, but *excluded* these persons when $E_y \div \hat{WG}$ was used as the dependent variable.

[q]Tella-Tella-Green are not entirely clear on this point, but it appears that they excluded households containing persons who did not

work during the week prior to the survey but did work during the year preceding the survey. Households containing persons who worked during the week preceding the survey, but not during the year preceding the survey may also have been excluded. Households containing persons who did not work during the week preceding the survey *or* during the year preceding the survey apparently have been included.

[r]In addition to the categories listed here, it is obvious that whenever the information necessary to construct a variable for an individual was missing, that person had to be excluded from the sample. However, in studies using imputed wage rates (see Tables 1 and 3), persons with missing wage data are included in the sample. Moreover, in the case of the Survey of Economic Opportunity, the Census Bureau imputed an amount to all income items that were originally in the "don't know" or "no answer" categories. (Amounts were not similarly imputed in the case of unanswered asset questions, however.) To our knowledge, all researchers who have used the SEO to estimate labor supply functions have included in their sample persons with these imputed income amounts.

[s]In order to incorporate non-participants into the analysis Garfinkel assigned each of them an imputed wage rate. Observed wage rates were used for the remainder of the sample.

[t]When including these persons in the sample, Garfinkel sometimes also added the amount of their transfer payments to his measure of nonemployment income.

population.[1] Some researchers, for example, have attempted to restrict their sample to those who are most likely to become participants in income maintenance programs. These studies sometimes use an income cutoff to define their sample. In addition, labor supply studies often exclude persons with constraints on the number of hours they work, such as the unemployed or disabled, or those who are using their time outside the labor market to engage in certain productive activities, such as schooling. Persons who receive work-conditioned transfer payments are sometimes dropped from the sample because researchers are not sure how to treat these transfer payments, since they may be endogenous.[2] Samples must also, of course, be limited to those for whom there is sufficient information to construct the variables.

Although most of the reasons for excluding observations from the sample may be valid under certain circumstances, it seems likely that by removing various groups that provide particular estimation problems one may not get accurate estimates of the response to the introduction of an income maintenance program. This may occur for two reasons: (1) the excluded group may behave differently from the included. In this case it is incorrect to infer that the parameters estimated for the included group are applicable to the excluded group. (2) The two groups may behave the same, but if the variables vary more for the excluded group or the combined groups than for the included group alone, it may be more difficult to identify the coefficients with the smaller, more homogeneous sample. For example, the excluded groups are likely to be disproportionately composed of individuals who work either very many or very few hours (see Table 4). This reduces variation in the dependent variable (labor supplied), variation that is not large for married males even when these groups are included.

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[1] For a detailed discussion of the reasons for excluding the groups listed in Table 4 from the sample and some of the problems their exclusion may cause, see Greenberg (1972), especially Section III.

[2] Another difficulty is that the (potential) wage rates of many recipients of work-related income are subject to various marginal tax rates that are very difficult to estimate from available information (Greenberg, 1972).

Moreover, by eliminating families who receive unemployment compensation or welfare, persons who did not work during the week before the survey, or other such groups, one may systematically exclude from the sample those individuals who are most likely to participate in an income maintenance program and to exhibit strong behavioral responses to it. Furthermore, previous empirical results imply that the labor supply curve may be backward bending; if so, the likelihood of accurately discerning the shape of the supply curve within the low and high hours ranges is reduced as observations are eliminated from near the tails of the hours distribution. This is particularly bothersome at the low hours range, because of the importance of this part of the supply function in examining the effects of welfare reform.

For these reasons, it is important that the sample should be as unrestricted as possible. Some of the obstacles to including commonly excluded observations can, in principle, be overcome through the use of imputed wage and income measures. In practice, however, a reasonable case can be made that including the categories of observations listed in Table 4 is just as likely, if not more likely, to produce inaccurate estimates as excluding them and that the imputation processes developed so far are unreliable. As a consequence, decisions on precisely which observations should be included or excluded from the sample are at this time highly judgmental. It is important, therefore, that researchers should obtain some idea of the effects on their estimates of excluding or including specific categories of observations in their samples. In the last subsection of Section III, we explore the sensitivity of parameter estimates to different sample compositions.

III. SENSITIVITY TESTS

In Section II, we described some of the methodological alternatives that researchers face when attempting to estimate labor supply functions from cross-sectional data. In this section we examine the sensitivity of parameter estimates to some of these methodological choices. Our emphasis is on the wage, asset, and labor supply variables, and on selection of the sample. To keep the analysis manageable--and the number of regressions reasonable--we proceed step by step, allowing only one change in variable definition or sample composition to take place at a time.[1]

Our overall research strategy is illustrated in Figure 1. Briefly, we begin with *observed* measures of wages and assets (our nonwage income variable), and a highly restricted, relatively homogeneous sample of white married male heads of households. All those who seem to present special estimation problems have been excluded from this initial sample. After exploring several forms of the wage, asset, and labor supply measures within this context, we examine the sensitivity of the results to using imputed, rather than observed, wage and asset measures. We then test the sensitivity of the estimates to changes in sample composition.

THE DATA

The econometric analysis is based on a sample drawn from the 1967 Survey of Economic Opportunity (SEO), a file that includes over 27,000 household units. We examined the possibilities of using several other files, including the Income Dynamics Panel and the National Longitudinal Survey ("Parnes" data), but chose the SEO for two reasons: (1) Most previous labor supply studies with disaggregated cross-sectional data have used either the SEO or data files assembled by the Bureau of the Census that are in many ways similar to the SEO (see Table 1). (2)

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[1]Regrettably this strategy rules out the possibility of fully exploring interactions among the different changes.

Figure 1

OUTLINE OF EMPIRICAL WORK

Observed Variables

Alternative Labor Supply Measures (Table 6)

Alternative Functional Form for Wage Variations (Table 7)

Alternative Wage Construction (Table 8)

Components of Net Worth (Table 9)

Imputed Variables

Imputed Wage (Table 10)

Cost-of-Living Adjustment (Table 10)

Experience Adjustment (Table 10)

Imputed Wage and Net Worth (Table 11)

Age/Experience Adjustments (Table 12)

Other Adjustments -- home ownership, children (Table 12)

Treatment of Wife's Earnings or Wage

Working Wives Only (Table 14)

Working and Nonworking Wives (Table 15)

Regression Sample Changes

For Combined Groups (Table 16)

For Marginal Groups (Table 18)

The SEO contains more of the data we wanted for conducting the sensitivity tests than other available data sets. For example, the SEO, unlike many data sets, contains answers to its full set of questions for all family members and is therefore appropriate for studies that are set in a family context. Moreover, the asset and nonemployment income data in the SEO are probably superior to those in any other existing data set. Nevertheless, it is important to recognize that, unlike certain other surveys, the SEO does not provide longitudinal data. In addition, certain other data files offer better measured hours of work and wage variables than the SEO and contain information on past work experience.[1]

THE RESTRICTED SAMPLE

Throughout this report we restrict our analysis to white married male heads of households, 25 through 54 years old. There are 6225 observations on this group in the 1967 SEO. The initial sample we drew from this group is a relatively homogeneous one that excludes as many persons and households as possible that present special estimation problems. In selecting this sample we incorporated most of the restrictions used by the authors listed in Table 4. The resulting "restricted sample" included 2012 households; specific characteristics of this sample are indicated in Table 5.

INITIAL SPECIFICATION AND MEASUREMENT OF VARIABLES

Three alternative measures of labor supply are used as dependent variables: weeks per year in 1966, hours per week in 1967, and annual hours. The last is a composite measure that is calculated as the product of the first two.

The wage variable used in the regressions is computed by dividing earnings during the week before the SEO survey interview by hours worked during the same week. Later we examine the sensitivity of the

[1]For a comparison of the suitability of five nonexperimental data sets for empirical studies of welfare reform effects, see Greenberg (1972), Section VIII, and DaVanzo and Greenberg (1973), Section V.

Table 5

CHARACTERISTICS OF THE RESTRICTED SAMPLE

A. Demographic Characteristics of the Sample
   1. Sex: male
   2. Marital Status: married, wife present, head of household
   3. Age: 25 through 54
   4. Race: white

B. Groups Excluded from the Sample
   1. Households where opportunity to work may have been restricted
      a. Husband or wife was unemployed for at least one week in 1966
      b. During most of the weeks they did not work in 1966, husband or wife was
         i. sick or disabled
         ii. institutionalized
         iii. in military service
      c. Husband or wife worked part-time in 1966 because
         i. their work was slack
         ii. they could not find full-time work
         iii. they were sick or disabled
   2. Households that received transfer payments in 1966 that may be work related
      a. Unemployment compensation
      b. Veterans' disability payments or compensation
      c. Pensions
      d. Social Security
      e. Welfare
      f. Workmen's compensation
   3. Nonworkers[a]
      a. Husband did not work during the week before the survey interview
      b. Husband did not work at all in 1966

4. Wage or hours data particularly likely to contain transitory elements or to be unreliable
   a. Husband usually worked full-time in 1966, but worked less than 35 hour during week preceding the survey; or vice versa
   b. Husband worked for a different employer or at a different kind of work during week preceding the survey than he did during m of 1966
   c. Husband ked 77 hours or more during the week preceding the survey
5. Other households that present special estimation problems
   a. Husband or wife was student at time of survey or in 1966
   b. Husband was in military service at time of survey or in 1966
   c. Husband or wife was self-employed at time of survey or received income from own business or farm in 1966
   d. Member of household, other than husband or wife, is over 18, and hence is a potential full-time worker
6. Households for which data necessary to construct one or more of the variables were missing

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[a]Although data from the 1967 SEO on annual earnings and nonemployment income and on weeks pertain to the year 1966, data on weekly earnings and hours worked per week refer to the week before the survey interview in February 1967. Since we use both weekly and annual earnings data to construct alternative observed wage measures, men who did not work during either the week or the year before the survey, and thus had zero earnings during one or the other period, must be omitted from the restricted sample.

estimated relation between wages and hours to several alternative methods of constructing an observed wage variable.

Non-wage income and wealth components reported in the SEO fall into three groups: (1) those reported only as income flows (transfer payments such as welfare and unemployment compensation); (2) those reported only as stocks (homes and automobiles); and (3) those reported both in flow and stock terms (rents, dividends and interest or, alternatively, real estate holdings, stocks, and bonds). To combine category (3) with *both* categories (1) and (2), an interest rate must be used either to capitalize the transfer payment flows in (1) or to impute flows from the capital stock in (2). However, since the work-related transfer payments that make up most of type (1) income would bias the income effect, type (1) income is never incorporated into our non-wage income measure making the capitalization or imputation process unnecessary. The income variable entered into the first set of regressions, therefore, is simply the reported net worth of the household.[1] Later, we examine the sensitivity of estimates of the income effect to the way in which various household wealth components are combined to construct the income variable.[2]

The labor supply of the head of a household may depend not only on his own wage rate and on non-wage income, but on the wage rates obtained by other family members. The wages of other working family members will have an income effect on the head's labor supply, usually assumed by economists to be negative. In addition, the wage rates of other family members may affect the head's labor supply through cross-substitution effects. These pure substitution effects will be positive or negative depending on whether the time of family members in non-market activities are net complements or substitutes (see De Tray,

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[1]Since net worth is a stock rather than flow concept, its regression coefficient is not a direct estimate of the income effect. However, if the flow of income from net worth is viewed as the product of net worth and an interest rate, the estimated income *elasticity* will be independent of whether the income variable is constructed in terms of stock or flows.

[2]For the moment, we will ignore the endogenous nature of net worth; this matter is taken up later (p. 68).

1973b; and Kosters, 1966). In the initial regressions the earnings of the spouse and of other family members are used as independent variables. However, since these measures incorporate the labor supply, as well as the wage rates, of these persons, and in effect treat the potential wage rates of nonworkers as zero, this approach is far from satisfactory. Later, we examine the sensitivity of the results to several alternative treatments of the actual or potential wage rates of other members of the household.

Age (and age squared) is included in the regression to allow for age effects on labor supply independent of those that work though the wage and net worth.

Education also appears in the regressions. The possibility that education may affect time allocation and therefore labor supply even when wages are held constant has been recognized in several recent studies of family behavior (see for example, Grossman, 1972; Michael, 1972; and De Tray, 1973a). These studies suggest that just as education may increase an individual's market productivity, it may also increase productivity in the home.[1] In addition, education may affect labor supply if more educated persons obtain jobs with higher nonpecuniary returns than less educated persons, or a taste for education is positively correlated with a taste for work.

Previous labor supply studies have assumed that as the number of persons in the household increases, so will the household's demand for commodities, hence their derived demands for the time and goods inputs

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[1]One variant of this argument is that an increase in education raises a person's home productivity by increasing the effective amount of his time in the home, which, in turn, reduces the per unit price of that time. This price reduction causes income and substitution effects, both of which act to increase household demand for effective home time. Whether this increase in the demand for effective time results in an increase in actual time allocated to household production is ambiguous *a priori* and dependent on the elasticity of demand for actual time. It can be shown that the effect of education on observed time depends on the size of the uncompensated price elasticity of demand for actual time in the home. The demand for actual time in the home will rise (fall) if that elasticity is greater (less) than one; if the demand for nonmarket time is unit elastic, education will have no effect on the allocation of observed time.

into household production.[1] An increase in the demand for goods will tend to increase the amount of time devoted to market work (labor supply); an increase in the demand for time will have the opposite effect. Although the net effect on labor supply is ambiguous *a priori*, previous studies have found that wives usually work less and husbands more as family size increases: Number of persons in the household other than spouse, and number of children less than six years old, who may be particularly wife's time intensive, are included in the labor supply equation to capture these effects.

The remaining variables in the regression are included to control for geographic differences in labor supply behavior. There are a number of reasons for at least some geographic variation in labor supply besides those due to regional wage and income differences. In addition to the possibility that tastes for work and leisure may vary among regions and between urban and rural settings, such factors as climate and recreational opportunities may influence the value that individuals place on nonmarket time. Similarly, the opportunities for and costs of nonmarket production may differ among regions. The locational variables may also help to correct for certain types of measurement errors in the wage and net worth variables. For example, as mentioned in Section II, the wage variable should be measured net of such factors as taxes, the cost of living, and work-related expenses. Although there are not now adequate data to do this directly, the locational variables may provide some control. All this suggests that a case can be made for entering locational variables in a labor supply function, and indeed such variables have been used in several previous studies (see, for example, Garfinkel, 1973; and Kalachek and Raines, 1973). However, the case for their inclusion in labor supply regressions is weakened by the possibility that these variables are also likely to reflect geographic variation in *demand*

[1]This is equivalent to assuming that family size is an exogenous variable. For the sake of comparison we will continue with that assumption even though several recent studies have established that fertility is endogenous to family decisionmaking (see, for example, De Tray 1973a).

conditions. To estimate labor supply functions, demand conditions must, of course, be allowed to vary.[1]

REGRESSION RESULTS: OBSERVED VARIABLES

Two important results emerge from Table 6, where our initial regressions are presented. First, the estimated relations between the wage measure and the two dimensions of annual hours--weeks per year and hours per week--are of opposite sign; the wage coefficient is positive in the weeks regression but negative in the hours regression. Second, the estimated relation between net worth and labor supply is positive, rather than negative as expected. The absolute magnitude of the net worth coefficients always exceed their standard errors.

There are two possible explanations for why the uncompensated wage effects on weeks per year and hours per week are of opposite sign: (1) decisions on the number of hours to work per week and the number of weeks to work per year are affected differently by wage changes; or (2) true wage effects on both measures of labor supply are similar in direction and magnitude, but one (or both) of the estimated parameters is biased. In particular, it seems likely that the estimated negative relation between wages and hours per year may be attributable in part to measurement error bias resulting from using hours per week in the denominator of the wage measure. Additional results, presented below, provide some support for this contention.

There are also two possible explanations for the positive signs of the net worth coefficients: (1) net worth may be a poor proxy for the conceptual variable of interest--nonwage-related income; (2) an omitted variable--previous labor supply, life-cycle factors (Smith, 1972), preferences for assets (Greenberg and Kosters, 1970)--may be affecting both assets and labor supply and thus the estimated relationship may not be a causal one; assets and labor supply may be

[1] For this reason, we tested the sensitivity of our results to the exclusion of the locational variables and found the results to be essentially unchanged, even when imputed values of wages and net worth were entered in the regression.

Table 6

INITIAL LABOR SUPPLY REGRESSIONS

	Labor Supply Measures					
	Weeks Per Year $WKWRK_y$		Hours Per Week $HRWRK_w$		Annual Hours $WKWRK_y \cdot HRWRK_w$	
Independent Variable	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Wage and Income Variables						
Hourly wage rate ($E_w \div HRWRW_w$)	0.0579	2.23	-1.65	-13.83	-82.67	-13.15
Net worth[a]	0.00212	1.04	0.0164	1.74	0.946	1.90
Spouse's annual earnings[a]	0.0252	1.20	-0.344	-3.55	-16.35	-3.20
Annual earnings of other family members[a]	0.0400	0.227	1.05	1.29	55.58	1.30
Control Variables						
Age	-0.00325	-0.0624	0.139	0.578	7.03	0.556
Age squared	0.0000358	0.0540	-0.00148	-0.484	-0.0745	-0.463
Years of school completed	-0.00461	-0.352	0.446	7.39	23.02	7.24
Number of persons in household, other than spouse	0.0720	2.38	0.406	2.92	24.78	3.38
Number of children under 6 in household	0.0526	0.972	-0.0126	-0.051	1.81	0.138
Town	0.0690	0.434	1.10	1.64	60.34	1.71
Rural	-0.0117	-0.0822	1.11	1.70	57.62	1.67
Suburb	0.0968	1.03	0.606	1.40	36.01	1.58
Poor city	0.247	1.28	-1.19	-1.34	-49.37	-1.06
Poor rural South	-0.0689	-0.425	0.624	0.837	29.23	0.744
Rest of South	-0.0448	-0.388	0.917	1.73	43.75	1.56
North central	-0.161	-1.58	1.61	3.43	74.62	3.02
West	-0.212	-1.83	0.632	1.18	23.88	0.850
Small SMSA	0.196	1.51	0.309	0.518	26.42	0.840
Medium SMSA	0.0972	0.898	0.639	1.28	36.40	1.39
Itercept term	51.50		40.81		2096.26	
R	0.015		0.121		0.114	
Man of dependent variable	51.84		45.43		2354.	
Sandard deviation of dependent variable	1.63		7.94		417.	
Nmber of observations	2012		2012		2012	

riable measured in $1,000s.

Definitions of Variables in Table 6

Self-explanatory variables, for example, age, are omitted from this list of definitions.

The interview for the 1967 SEO took place in February 1967.

Subscripts

- w data pertain to week before survey interview week
- y data pertain to year before survey interview year.

Labor Supply

- $WKWRK_y$ weeks of work per year
- $HRWRK_w$ hours of work per week
- $WKWRK_y \cdot HRWRK_w$ hours of work per year.

Earnings and Assets

E_w weekly earnings of head

Annual earnings of other family members: annual earnings of household less the annual earnings of the husband and wife

Net Worth: Household assets less debts

Assets include:
- Market value of own home
- Market value of vehicles
- Value of real estate holdings
- Money in bank accounts
- Stocks and bonds
- Personal loans to others
- Value of other assets: boats, trailers, oil royalties, patents, etc., but excluding personal belongings and furniture

Debts include:
- Debt owed on own home
- Debt owed on vehicles
- Debt owed on real estate holdings
- Debt owed stores, banks, lending institutions
- Noninterest debt: debt owed doctors, hospitals, utility companies, etc.

Geographic Characteristics

Poor city	A dummy that equals 1 if the person lives within a "poverty area" (defined in 1967 SEO Codebook, p. 23-24) that is in an SMSA with population 250,000 or more; 0 otherwise.
Suburb	A dummy that equals 1 if the person lives in the urban fringe of an SMSA; 0 otherwise.
Town	A dummy that equals 1 if the person lives in an urban area outside an SMSA; 0 otherwise.
Rural	A dummy that equals 1 if the person lives in a rural area outside an SMSA; 0 otherwise.
Poor rural South	A dummy that equals 1 if the person lives in a poor Southern county, outside an SMSA (defined in 1967 SEO Codebook, p. 33); 0 otherwise.
Rest of South	A dummy that equals 1 if the person lives in the Southern Census region but not in areas that are "poor rural South" (see above); 0 otherwise.
North Central	A dummy that equals 1 if the person lives in the North Central Census region; 0 otherwise.
West	A dummy that equals 1 if the person lives in the West Census region; 0 otherwise.
Small SMSA	A dummy that equals 1 if the person lives in an SMSA whose population is less than 250,000; 0 otherwise.
Medium SMSA	A dummy that equals 1 if the person lives in an SMSA whose population is between 250,000 and 750,000; 0 otherwise.

determined simultaneously. Further evidence on this point will be presented below.

These results are based on a relatively homogeneous, highly restricted sample. Although the restrictions we used to define this sample (see Table 5) were not designed intentionally to exclude persons with relatively low labor supply, this is a consequence of their application. Hours per week do not fall below 35 for any person in the sample, and the weeks-per-year variable is very nearly a constant; only 41 men in the sample of 2012 reported that they worked fewer than 50 weeks.[1] This small variation in the dependent variables makes it difficult to find systematic relationships between labor supply and its determinants. However, this lack of variation does not necessarily mean that our sample is inherently unrepresentative. For example, if low hour individuals excluded from the sample were off their long run labor supply curves, the small variation in labor supplied by the sample population would presumably mean that labor supply schedules for white prime-age males are very inelastic.

Because of the absence of variation in the weeks-per-year variable, variation within the composite annual hours measure (which is calculated as the product of weeks per year and hours per week) is almost wholly attributable to variation in hours per week.[2] The results in the annual hours regression are, therefore, very similar to those in the hours-per-week regression, with the regression

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[1] Weeks worked in 1966 are reported in the SEO for the following intervals: 1 to 13 weeks, 14 to 26 weeks, 27 to 39 weeks, 40 to 47 weeks, 48 to 49 weeks, and 50 to 52 weeks. To calculate the weeks-per-year variable, the midpoints of each of these intervals, but the last, were used. Since Tella, Tella, and Green (1973) report that a sample check they made of actual SEO questionnaires "showed that workers who fell into the 50-52 weeks category invariably reported to have worked 52 weeks," these persons were assigned a value of 52. As a consequence of this and the fact that only 41 persons were assigned a value of less than 52 weeks, the mean of the weeks-per-year variable is 51.84.

[2] The simple correlation coefficient between annual hours and hours per week is 0.9788; the correlation between annual hours and weeks per year is 0.1658.

coefficients differing roughly by a scale factor of 52.[1]

Most of the other coefficients in Table 6 conform to *a priori* expectations. Wife's earnings has a negative and significant effect on male hours. Other family earnings, however, have a positive coefficient, but one that is never significant at the 5 percent level.

The coefficients of age and age squared are never significantly different from zero at conventional levels. Education has a positive and significant effect on hours, but its relation to weeks is insignificant. Labor supply is positively related to family size; it appears that additional family members increase the derived demand for goods more than the derived demand for husband's time in household production.

The coefficients of the geographic variables show the additional number of weeks or hours people in these areas work, other things the same, relative to people for whom all the geographic dummies equal zero (non-poverty areas in central cities in SMSAs with population greater than 750,000 in the North East census region).

Curvilinearity in the Wage Relation

To allow for the possibility of a backward bending labor supply curve, labor supply researchers usually use functional forms for the wage rate variable that allow for curvilinearity. The wage coefficients in Table 7 are from regressions that are identical to those presented in Table 6 except for the functional form of the wage variables. The linear wage coefficients that appear on the first line of the table are the same as those reported in Table 6. The natural logarithm of the wage measure was used to estimate the coefficients on the second line of the table. Both wage and wage squared were entered into the regressions to estimate the third set of coefficients. The final set of regression coefficients is based on a step function formulation that allows both the intercept term and the slope of the

[1]For the sake of consistency, we will continue to report our results for all three labor supply measures, even though the weeks-per-year results are of limited interest when they are based on the restricted sample. The weeks-per-year results become more meaningful for the less restricted samples used later in this section.

Table 7

ESTIMATED WAGE (RATE) COEFFICIENTS: ALTERNATIVE FUNCTIONAL FORMS OF THE WAGE RATE VARIABLE[a]

	Labor Supply Measures											
	Weeks Per Year $WKWRK_y$				Hours Per Week $HRWRK_w$				Annual Hours $WKWRK_y \cdot HRWRK_w$			
Wage Rate Measures	Coefficient	t-Value	Elasticity[b]		Coefficient	t-Value	Elasticity[b]		Coefficient	t-Value	Elasticity[b]	
$E_w \div HRWRK_w$	0.0579	2.23	0.00398		-1.65	-13.83	-0.129		- 82.67	-13.67	-0.125	
$\ln(E_w \div HRWRK_w)$	0.339	3.22	0.00654		-8.45	-17.[illegible]6	-0.186		-420.95	-16.91	-0.179	
$E_w \div HRWRK_w$	0.165	2.79	0.00706		-3.97	-14.94	-0.206		-197.40	-14.05	-0.198	
$(E_w \div HRWRK_w)^2$	-0.00873	-2.02			0.189	9.71			9.34	9.09		
	Slope Coefficient	t-Value	Dummy Coefficient	t-Value	Slope Coefficient	t-Value	Dummy Coefficient	t-Value	Slope Coefficient	t-Value	Dummy Coefficient	t-Value
$(E_w \div HRWRK_w) < \$1.75$	0.469	0.969	-1.09	-1.56								
$(E_w \div HRWRK_w) \geq \$1.75$	0.0368	1.38										
$(E_w \div HRWRK_w) < \$1.25$					-5.78	- 0.962	23.30	3.80	-432.39	- 1.36	1280.6	3.95
$(E_w \div HRWRK_w) = \$1.25$ to 4.74					-3.07	-12.80	12.81	8.35	-153.65	-12.08	641.95	7.90
$(E_w \div HRWRK_w \geq \$4.75$					-0.338	- 1.69			- 17.07	- 1.61		

[a] The other explanatory variables in the regressions are the same as those in Table 6.

[b] Elasticities are evaluated at the mean values for the three labor supply measures and at the mean value for $(E_w \div HRWRK_w)$, i.e., \$3.56.

wage relation to vary within separate wage intervals. We began with 11 intervals.[1] However, by collapsing adjacent wage cells in which slope coefficients were not statistically different or dummy coefficients were not significantly different, we eventually reduced the number of intervals to two (<\$1.74, ≥\$1.75) in the weeks-per-year regression and to three (<\$1.25, \$1.25-\$4.74, ≥ \$4.75) in the hours-per week and annual-hours regressions.[2]

The results in Table 7 for the relationship between wages and weeks per year indicate that the wage effect becomes increasingly less positive at higher wages. In the quadratic formulation the curve bends backward at a wage of approximately \$9.50 per hour. However, these estimates are based on a mere 41 persons out of 2012 who worked less than year round. The estimated curvilinear relation between wages and hours per week or annual hours runs counter to *a priori* expectations; the estimated labor supply curve is forward-bending, rather than backward bending. (Hill, 1970, and others have found this also.) This result is, however, difficult to interpret given the potential negative bias in the wage coefficient.

The sensitivity of the estimated effect of wages on labor supply to alternative functional forms for the wage variable is perhaps best indicated diagramatically. The shapes of the relations between wages and work effort that are indicated by the coefficients in Table 7 are presented in Fig. 2. In general, these figures indicate that within the middle wage range, the shape of the estimated supply function is not very sensitive to the functional form of the wage variable. However, within the lowest wage range--the portion of the supply curve that is most important for evaluating the effects of income maintenance programs--and within the highest wage range, the estimates are rather sensitive to functional form.

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[1]Under \$.75, \$.75-\$1.24, \$1.25-\$1.74, ..., \$4.25-\$4.74, \$4.75-\$5.24, and \$5.25 and over.

[2]These difference tests were made separately for the slope and dummy coefficients; it is interesting that these two independent tests both implied the same final set of intervals.

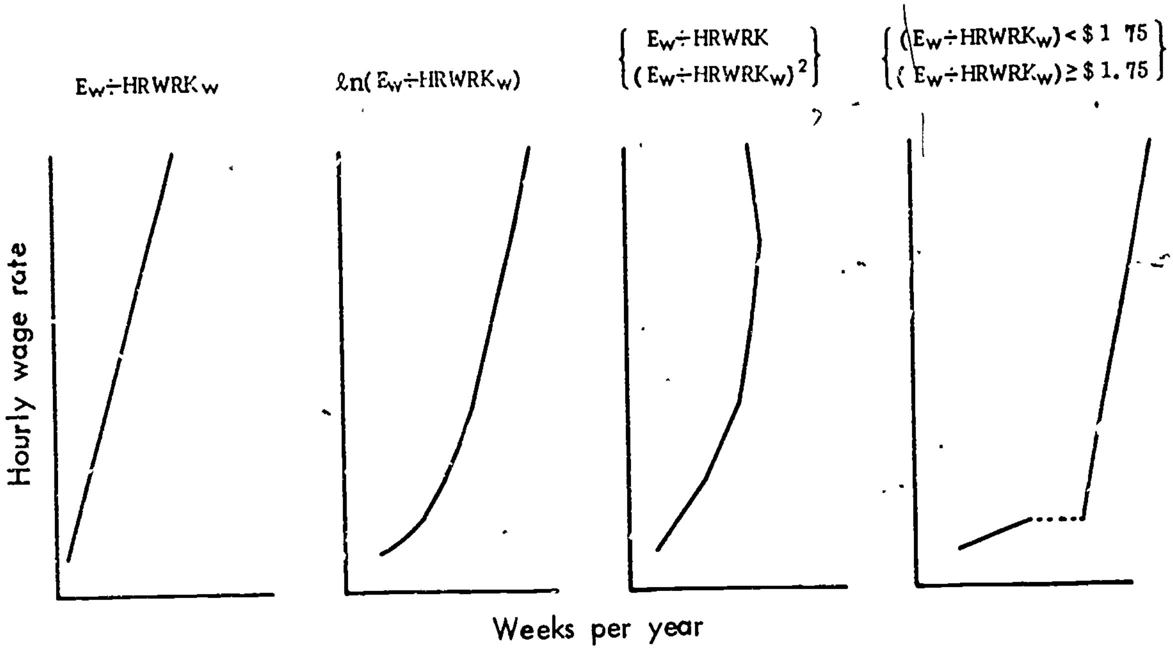

Fig. 2a—Estimated relation between weeks per year and alternative functional forms of the wage variable

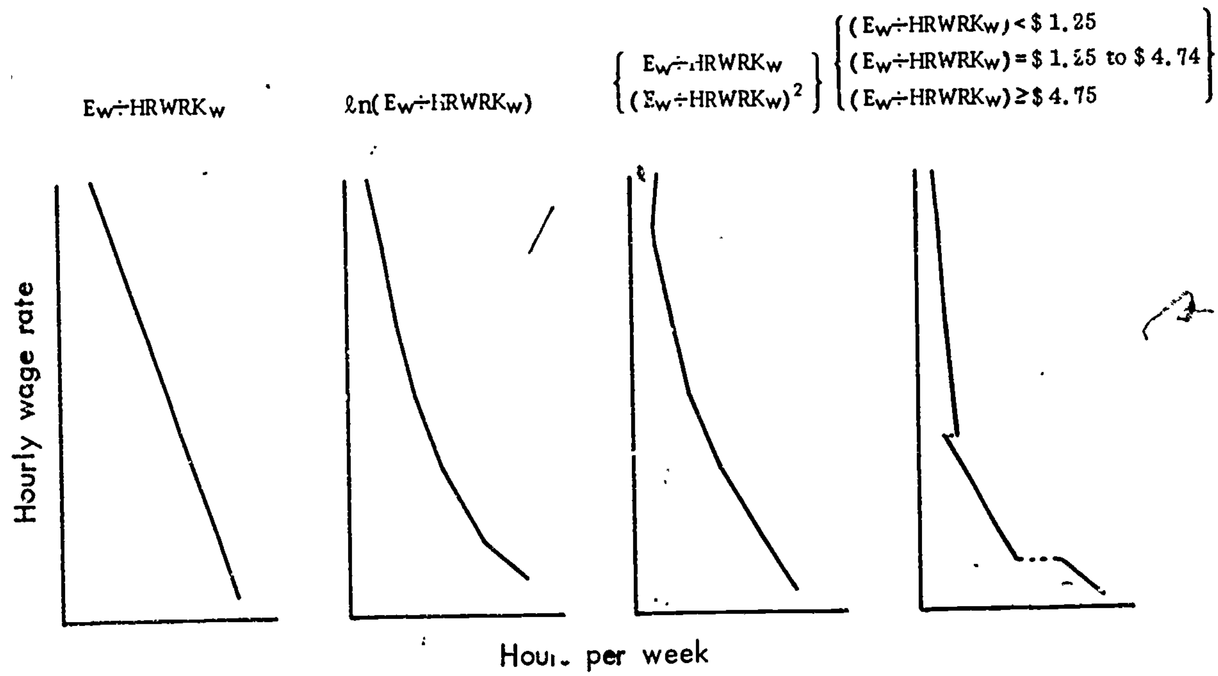

Fig. 2b—Estimated relation between hours per week and alternative functional forms of the wage variable

Alternatively Constructed Wage Rate Measures

Thus far, the only wage measure we have used is an hourly wage rate constructed by dividing weekly earnings by weekly hours ($E_w \div HRWRK_w$). Three additional wage measures can be constructed from data in the SEO: (1) annual earnings divided by the product of hours per week and weeks per year ($E_y \div [HRWRK_w \cdot WKWRK_y]$), (2) weekly earnings ($E_w$), and (3) annual earnings divided by weeks per year, ($E_y \div WKWRK_y$). The last two variables, of course, are measures of weekly, rather than hourly, wage rates. All four measures have at one time or another been used in labor supply studies (see Table 3).

Regression coefficients based on the four wage measures appear in Table 8. Once again, these coefficients are all from regressions that, except for the wage measure, are identical to those presented in Table 6. Because of scale differences among the wage measures, the wage elasticities are more readily compared than the wage coefficients themselves.

For three of the four wage rate measures, a source of potential bias results from their being constructed by dividing information on time at work ($HRWRK_w$, $WKWRK_y$, or the product of the two) into an earnings variable (E_w or E_y).[1] Evidence of the seriousness of this measurement error bias can be obtained from Table 8. For example, in the weeks-per-year regressions, the coefficients for the two wage measures that contain $WKWRK_y$ in their denominators tend to be less significant and their elasticities smaller than the two measures that are not divided by $WKWRK_y$.[2]

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[1]The magnitude of this bias may have been increased because participants in the SEO who raised any questions when asked about their employment experience during the week before the survey were told to give their *normal* weekly earnings but their *actual hours* worked.

[2]Errors in the $WKWRK_y$ variable in the SEO may be of two types: (1) the respondent may have incorrectly reported the number of weeks he worked last year; (2) weeks worked last year were coded in intervals (see footnote 1, p. 54); all persons falling in a given interval were assigned the same value of $WKWRK_y$, even though the actual number of weeks worked varied within the interval.

Table 8

ESTIMATED WAGE RATE COEFFICIENTS: ALTERNATIVELY CONSTRUCTED WAGE RATE MEASURES[a]

Wage Rate Measures	Labor Supply Measures: Weeks Per Year $WKWRK_y$			Hours Per Week $HRWRK_w$			Annual Hours $WKWRK_y \cdot HRWRK_w$		
	Coefficient	t-Value	Elasticity[b]	Coefficient	t-Value	Elasticity[b]	Coefficient	t-Value	Elasticity[b]
Hourly Wages									
$E_w \div HRWRK_w$	0.0579	2.23	0.00398	-1.65	-13.83	-0.129	- 82.67	-13.67	-0.125
$E_y \div (HRWRK_w \cdot WKWRK_y)$	0.0455	1.57	0.00308	-2.36	-18.26	-0.183	-120.31	-17.63	-0.179
Weekly Wages									
E_w	0.00119	1.93	0.00363	0.0122	4.14	0.0425	0.690	4.47	0.0465
$E_y \div WKWRK_y$	0.000762	1.08	0.00229	0.00472	1.39	0.0162	0.279	1.56	0.0185

[a]The other explanatory variables in the regression are the same as those in Table 6.

[b]Elasticities are evaluated at the means of the labor supply measures and the wage rate measures.

In the hours-per-week ($HRWRK_w$) equations the two hourly wage measures, which use $HRWRK_w$ in their denominator, have negative coefficients, and the two weekly wage rates have positive coefficients. However, the relationship between the E_w and $HRWRK_w$ is likely to be positively biased because earnings the week preceding the survey (E_w) may be high (low) simply because the person worked an atypically large (small) number of hours that week ($HRWRK_w$).[1] Indeed the coefficient of E_w is larger than that of $E_y \div WKWRK_y$, the other measure of the weekly wage. The wage elasticities for the annual hours equation follow the same pattern as those for the weekly hours equation.

Labor Supply and the Components of Net Worth

As described earlier, the variable that is used in labor supply studies to measure the income effects is usually built up by aggregating income or wealth from disparate sources, often with one or more interest rates being used so that sources reported in the data as stocks can be combined with those reported as flows. It is important to determine whether there are substantial differences among

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[1]One can estimate the extent of the negative bias in using $E_w \div HRWRK_w$ as the wage variable by calculating the hourly elasticity implied by the coefficients of E_w. To do this, one must assume that E_w is measured accurately and independently of labor supply. (This would be the case if all respondents give their usual, rather than actual, weekly earnings--see footnote 1, p. 59.) For simplicity, we use a constant elasticity formulation in the following discussion.

If the true model relates hours per week to the hourly wage rate, then

$$\ln(HRWRK_w) = b\ln(E_w \div HRWRK_w) + \ldots$$

$$\ln(HRWRK_w) = \frac{b}{1+b}\ln E_w + \ldots = c\ln E_w + \ldots$$

Since $b = c/(1-c)$, b can be estimated indirectly from a regression of $HRWRK_w$ on E_w. Because our regressions are in linear form, we will use elasticities evaluated at the means to compare the direct and indirect estimates of b. The indirect estimate of b is $\frac{.0425}{1-.0425} = .044$. The direct estimate is -0.129.

Note that these two numbers are really estimates of the range since the coefficient of E_w that produced c is likely to be biased upward.

these income and wealth sources in their relations to the labor supply measures. For if there are, estimates of the income effect will be sensitive to which sources are incorporated into the income variable and how these income and wealth components are weighted by interest rates.

To test for differential effects on labor supply of the components of net worth, the sum of all components of net worth is entered as a variable; in addition, each of the individual components but one were entered as explanatory variables. In effect all of the components except the omitted one are entered twice, once in the total net worth variable where their coefficients were constrained to be the same, and once as individual components, where the coefficients are allowed to vary. The coefficients of separate components measure the extent to which the effect on labor supply of that component *differs* from the effect of the omitted component. If the coefficient is statistically significant, the labor supply effects of that component and the omitted component are significantly different. The *total effect* of an included component is the net worth coefficient plus the component's coefficient.[1]

The omitted component of net worth consists of real estate holdings, stocks and bonds, and money in bank accounts. Each of the coefficients of separate components of net worth shows how the labor supply effect of that component differs from the labor supply effect of this omitted component.[2] The results are presented in Table 9.

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[1]The advantage of this approach is that it yields t-ratios that can be interpreted directly as tests of differences among components of net worth; had each component been entered separately, we would have had to perform a series of tests on pair-wise comparisons for the same result.

[2]Note that the differences may occur for several reasons--for example, interest rates may differ among components (especially between debt and asset components), or the underlying relationship between different components and hours worked may differ. Ultimately, this distinction may be important, but for our purposes, it is necessary only to establish whether there are differences, regardless of their source.

Table 9

ESTIMATES OF DIFFERENTIAL EFFECTS ON LABOR SUPPLY OF THE COMPONENTS OF NET WORTH[a]

	Labor Supply Measures					
	Weeks Per Year $WKWRK_y$		Hours Per Week $HRWRK_w$		Annual Hours $WKWRK_y \cdot HRWRK_w$	
	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Net Worth[b]	0.000196	0.0752	0.0176	1.48	0.912	1.45
Included Components of Net Worth[c]						
Market value of own home	0.0124	1.81	-0.0336	-1.07	-0.0113	-0.685
Debt on own home[d]	0.00892	0.909	-0.132	-2.95	-0.0640	-2.70
Market value of vehicles	0.000967	0.174	0.0101	0.396	0.583	0.436
Debt on vehicles[d]	-0.0154	-0.336	-0.467	-2.23	-24.73	-2.24
Value of other assets	-0.0234	-0.647	0.259	1.56	11.95	1.37
Debts to stores, banks, lending institutions[d]	-0.0109	-0.306	-0.286	-1.75	-15.47	-1.799
Non-interest debt[d]	-0.0339	-0.340	0.194	0.425	8.53	0.355
Net worth coefficients from Table 6	0.00212	1.04	0.0164	1.74	0.946	1.90

[a]Variables measured in \$1000s; the other explanatory variables in the regression are the same as those in Table 6.

[b]Total net worth is made up of the seven included components plus real estate holdings, stock and bonds, and money in bank accounts.

[c]The coefficients on these components of net worth represent the <u>difference</u> between the effect on labor supply of those components and the effect on labor supply of the "excluded" component (see note b). The total effect of each component is the sum of the own coefficient plus the net worth coefficient in the first line of the table.

[d]Debts are entered with a negative sign.

For comparison purposes, the last line of Table 9 gives the net worth coefficients from Table 6, where only the sum of net worth components entered the regression (that is, all component effects were constrained to be identical).

With the exception of the house value component, the various components of net worth do not have significantly different effects on weeks worked per year. However, the absence of apparent differential effects in this case may be attributable to the lack of variation in the dependent variable. The estimates for the hours equations indicate that three of the debt components of net worth (debt on own home; debt on automobiles; and debts to stores, banks, and lending institutions) may have a more negative effect on hours worked than the return from household investments. Since debts are in effect a negative asset and have been entered into the regressions with a minus sign, persons with large debts are particularly likely to work long hours. This may reflect the attitudes of lenders toward hard workers or it may indicate that decisions to incur large debts are made jointly with decisions to work long hours. In any event, these results indicate that different components of net worth have quite different effects on labor supply and suggest that at least some components of net worth may be subject to endogenous influences.

REGRESSION RESULTS: IMPUTED VARIABLES

The wage and net worth coefficients presented above are based on observed wage and net worth measures. However, as suggested in Section II, observed measures may not provide accurate estimates of how individuals will respond to the income and wage changes caused by the introduction of an income maintenance program. In Section II we discussed the major problems in using empirical wage and income measures and suggested that imputed wage and nonemployment income (or assets) variables can be used to treat many of these problems. In this section we compare the performances of observed and imputed wage and net worth measures in labor supply equations. The equations used to impute wages and assets are presented and discussed in Appendixes C and E.

Observed vs. Imputed Wages in the Labor Supply Equation

Observed wage rates may introduce several biases into the estimated labor supply wage relation. (1) If the labor supply measure is used as a divisor to construct the wage variable, any errors in measuring labor supply will cause a spurious negative relationship between the two variables. (2) Other errors in measuring earnings or wages will cause an errors-in-variables bias toward zero. (3) If wage rates are endogenous (jointly determined with labor supply), a simultaneous equations bias will result; Schultz (forthcoming) hypothesizes that the effect of this bias is to make the wage coefficient more positive. (4) Transitory changes in wage rates will cause substitution effects to be larger and income effects smaller than permanent changes. (5) Lifetime average (permanent) wage, not current wage, is the theoretically correct wage variable. Since some of these biases work in opposing directions, their net effect is unknown *a priori*. Imputed wage rates should help correct the first four of these biases and give us some idea of their *net* magnitude. Later, we experience-adjust imputed wages to attempt to correct for the last bias.

In Table 10 we compare the coefficients of observed and imputed wages for several different model specifications and wage adjustments. In this table and all of those that follow, the wage variable is the natural logarithm of the hourly wage rate.[1]

In all of the equations explaining hours of work (both hours per week and annual hours), switching from observed to imputed wage rates increases the magnitude of the wage coefficient.[2] The observed-wage coefficients are always negative; the imputed-wage coefficients are either less negative (lines 1-4) or positive (lines 5 and 6). Thus it appears that the net bias in using the observed wage rate to explain hours of work is negative.

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[1]The main reason for using the logarithmic form is that in Table 7 the logarithm of the wage was more significant than the wage entered linearly for all three measures of labor supply.

[2]Hereafter when we refer to wage measures or wage coefficients, we mean those pertaining to the *natural logarithm* of the wage.
The compensated substitution effects these wage coefficients imply are discussed in the next subsection.

Table 10

ESTIMATED WAGE RATE COEFFICIENTS: IMPUTED VS. OBSERVED WAGE RATES, COST OF LIVING ADJUSTMENTS, EXPERIENCE ADJUSTMENTS

Equation Specification Wage Rate Measures[b]	Labor Supply Measures: Weeks Per Year WKWRKy — Observed Wage Coefficient	t-Value	Imputed Wage Coefficient	t-Value	Hours Per Week HRWRKw — Observed Wage Coefficient	t-Value	Imputed Wage Coefficient	t-Value	Annual Hours WKWRKy · HRWRKw — Observed Wage Coefficient	t-Value	Imputed Wage Coefficient	t-Value
Other explanatory variables same as in Tables 6-8												
(1) LW	0.339	(3.22)	0.514	(1.17)	-8.45	(-17.96)	-7.48	(-3.55)	-420.95	(-16.91)	-355.26	(-3.21)
(2) LW_L	0.347	(3.33)	0.553	(1.27)	-8.31	(-17.80)	-7.41	(-3.54)	-413.09	(-16.73)	-349.63	(-3.18)
(3) LW_I	0.330	(3.14)	0.509	(1.15)	-8.46	(-17.98)	-8.01	(-3.76)	-421.83	(-16.94)	-382.06	(-3.42)
(4) LW_X			0.687	(1.75)			-5.18	(-2.75)			-225.21	(-2.28)
Same as above except education omitted from equation												
(5) LW	0.303	(3.18)	0.205	(1.06)	-6.57	(-15.08)	0.0971	(0.105)	-324.57	(-14.12)	18.24	(0.374)
(6) LW_X			0.165	(1.19)			0.505	(0.756)			36.76	(1.05)

[a] The equations used to impute LW, LW_L, and LW_I are presented and discussed in Appendix C.

[b] $LW = \ln [E_w \div HRWRK_w]$, i.e., natural logarithm of hourly wage.

$LW_L = \ln [(E_w \div HRWRK_w)/P_L]$, i.e., natural logarithm of hourly wage adjusted by Department of Labor cost-of-living index.

$LW_I = \ln [(E_w \div HRWRK_w)/P_I]$, i.e., natural logarithm of hourly wage adjusted by Watts' Iso-Prop cost-of-living index.

$LW_X = \hat{LW} - (\text{Coeff. of Exp}) \cdot Exp - (\text{Coeff. of } Exp^2 \cdot Exp^2$ = Imputed natural logarithm of hourly wage adjusted for experience (Exp), i.e., evaluated at Exp = 0.

In the equation explaining weeks per year, both observed and imputed wages always have positive coefficients. In lines 1-3 the imputed wage coefficient is always larger than the corresponding observed wage coefficient, though in line 5 the imputed wage coefficient is smaller than the observed.

In lines 2 and 3 the cost-of-living-adjusted wages are used (see Appendixes C and D). Adjusting for cost of living appears to have little effect on the estimated wage coefficients.

In line 4 we use an "experience-adjusted" wage (LW_X). Each person's imputed wage is adjusted back to what it would be if he were just beginning work--that is, his imputed wage is calculated at experience = 0. This adjustment is a crude attempt to purge the wage of life cycle effects and give a better measure of an individual's *permanent* (average lifetime) wage. For all three labor supply measures, the coefficient of $\widehat{LW}_X$ is more positive than the corresponding coefficient of $\widehat{LW}$.

Because education is an important determinant of imputed wage rates,[1] the simple correlation between years of schooling completed and imputed wages is quite high: 0.77.[2] Some authors who use imputed wages (for example, Kalachek and Raines, 1970) have omitted education from the labor supply equation to remove this source of multicollinearity. When education is excluded (lines 5 and 6)[3], the wage coefficients in the hours of work equations become larger,

[1]For men, separate equations were estimated for three education groups; the intercepts vary substantially for these groups. In addition, within each group, years of education exert a significant influence on wage levels.

[2]The simple correlation between education and the experience-adjusted wage ($\widehat{LW}_X$) is 0.88. The correlation between education and the observed wage (LW) is 0.46.

[3]The education (grades of school completed) coefficients and t-statistics in the hours equation using LW (line 1) were as follows:

Type of Wage Measure	Labor Supply Measure: $HRWRK_w$ Coefficient	$HRWRK_w$ t-Value	$WKWRK_y \cdot HRWRK_w$ Coefficient	$WKWRK_y \cdot HRWRK_w$ t-Value
Observed	.558	(9.44)	28.48	(9.11)
Imputed	.524	(4.01)	25.80	(3.76)

especially in line 6 where LW_X is the wage measure. The observed wage coefficients in the hours equations are still negative, but are smaller in absolute magnitude; the imputed wage coefficients become positive when education is omitted.[1] This result may partly explain why the substitution effects Kalachek and Raines (1970) estimated are the largest of the studies listed in Table 2.

The overall impression Table 10 conveys is that estimates of wage effects on labor supply are not strongly sensitive to whether an observed or an imputed wage variable is used, as long as the labor supply equation contains education. It appears that whatever biases are embodied in the observed wage variable are also reflected by the imputed wage variable, although possibly to a lesser extent.

Observed, Imputed, and Age-Adjusted Net Worth

In this subsection we concentrate on net worth (assets) as the variable for measuring income effects. Our intent is not to develop a theory of asset accumulation but rather to determine whether various ways of adjusting measures of net worth affect estimated labor supply responses.

We assume that labor supply and net worth measured at any point in time contain permanent, age-related, and error components (see pp. 12-13). To estimate the relationships between permanent levels of net worth, we must purge either observed labor supply or observed assets, or both, of their age-related components. As with wages, in order to estimate the "exogenous" component of household net worth, we must first determine how net worth varies by age. To do this we have taken a very simple view of the determinants of household net worth.[2] To allow the effect of age on net worth to vary across wealth classes, the initial sample (2012 families) was divided into five

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[1]The education coefficient is negative, but insignificant, in the weeks-per-year equations. When education is omitted, the wage coefficients become smaller (compare line 1 with line 5 and line 4 with line 6), which is consistent with the omitted variables bias.

[2]See Appendix E for details.

"lifetime wage" groups; that is, each family was assigned to one of five wage groups based on the husband's experience-adjusted imputed wage (see p. 67).

Regressions of observed net worth on a number of factors postulated to determine asset levels were then run across families within each of these wage categories. The right-hand variables in these regressions include personal characteristics such as age and education, as well as locational variables. In this way, the age path of net worth within each experience-adjusted wage group could be ascertained.[1] In addition, two other factors were entered in the regression--the total number of children that families had, and a dummy indicating whether or not they were homeowners.[2] The argument for including these variables is that, since families may allocate their nonhuman wealth in many different ways, we must somehow ensure that we do not systematically exclude any important component of net worth. Other things equal, a family with children may have relatively small amounts of observable net worth because they hold a relatively large portion of their wealth in the form of children. Unless we take this substitution between children and net worth into account, we may systematically underestimate the amount of real net worth (and thus real wealth) that large families have.[3]

The argument for the home ownership dummy proceeds along slightly different lines. To the extent that housing equity does represent a large fraction of homeowners' net worth *and* to the extent that housing

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[1]To allow for nonlinearities, age was entered as a series of dummies, with each household assigned to one of seven (five-year) age groups (25-29 to 55-59).

[2]We recognize that these variables are very likely simultaneously determined with net worth and therefore that the net worth estimating equations are probably subject to simultaneous equations bias. For the sake of this exercise, however, we will assume that children and home status are exogenously determined.

[3]The presumption here is that there will in fact be a substitution between observed net worth and children; that is, the coefficient on the children variable in the net worth regression was assumed to be (and, in fact, is) negative. See p. 129.

equity is a visible and relatively well measured variable, we might expect that estimates of actual net worth for homeowners would be more accurate than the estimated net worth of non-homeowners. Put another way, assuming no systematic differences of wealth or tastes among renters and homeowners, we would expect homeowners and renters with a given set of characteristics to hold approximately the same total amount of financial assets. If our data indicate that this is not true, one explanation may be that the net worth of renters is being systematically undermeasured because renters hold some significant portion of their net worth in forms not captured in the SEO data.

There are, of course, other explanations for the estimation relationships between net worth and both home status and numbers of children (see Appendix E). It is useful in the context of this study to think of the hypothesis given above as representing one extreme; for example, *all* differences in net worth between homeowners and renters are attributed to error in measuring renters' net worth. At the other extreme, given the negative (and significant) coefficient on the renter dummy, one might speculate that, other things equal, renters are in fact poorer than homeowners. Under this hypothesis, renters report less net worth than homeowners because they have less overall wealth. Support for this hypothesis might be found in such considerations as the difficulty that poor families have in accumulating down payments, obtaining financing, and the like.

Although none of these arguments stands on well developed conceptual grounds, determining whether such factors do affect the estimated relationship between net worth and the amount of market labor supplied should help direct future research efforts in this area.

To test the sensitivity of the relationship between labor supply and alternative measures of net worth, each family in our sample was assigned six different imputed net worth values. The first of these is an imputed net worth (INW) based on the equation given in Table E-2. The second measure (INW_A) takes INW and adjusts it for differences in age among individuals, that is, each family is assigned a net worth that is assumed to be the net worth they would have held when the husband was age 25-29 (see Appendix F for details). The third variant ($INW_{A,C}$) adjusts INW_A for children, setting the number of children in each household equal to 0 rather than its actual value. Similarly, the fourth measure ($INW_{A,C,R}$) takes $INW_{A,C}$ and adjusts it for differences in home status by setting the housing status variable equal to 0 for every individual whether a renter or not. The two remaining measures are based on an alternative form of the asset imputing equation, one in which neither housing status nor numbers of children is used as an independent variable. The first of these last two measures (INW2) is the imputed value based on Table E-3; the second measure, ($INW2_A$) is INW2 adjusted for age as described above.

Table 11 presents wage and net worth coefficients for alternative combinations of imputed and actual wage rates and imputed and actual net worth. For the broadest definition of market work, annual hours, two facts are evident. First, when the observed wage (LW) is combined with either observed net worth (NW) or imputed net worth (INW) the sign of the net worth coefficient is always positive and always significant; however, when the imputed wage replaces the actual wage, the signs of the coefficients on both the actual and imputed net worth variables are always negative, though never significant. Thus the *sign* of net worth coefficient is sensitive to whether *wages* are imputed, but not to whether net worth is imputed. Second, in all cases, imputing net worth increases the absolute size of the coefficient of net worth.

Table 12 presents the effects of different adjustment procedures for both imputed wage and imputed net worth variables. Concentrating again on the annual hours results, note first that experience-adjusting the imputed wage and age-adjusting imputed net worth (second row) increases the wage coefficient and decreases the net worth coefficient. In a sense, both of these represent moves in the "right" direction, that is, toward a more positive wage effect and a more negative income effect. The most dramatic change is in the imputed net worth coefficient, which decreases from -1.29 (t = -0.59) to -4.87 (t = -2.06).

Table 11

ESTIMATED WAGE AND NET WORTH COEFFICIENTS:

IMPUTED VS. OBSERVED MEASURES[a]

Labor Supply Measure	Wage $LW = Ln(E_w \div HRWRK)$				Net Worth[b]			
	Observed (LW)		Imputed (ILW)		Observed (NW)		Imputed (INW)	
	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Weeks per year ($WKWRK_y$)	0.339	(3.22)			0.00191	(0.936)		
			0.514	(1.17)	0.00292	(1.45)		
	0.320	(3.02)					0.0152	(1.75)
			0.348	(0.778)			0.0190	(2.18)
Hours per week ($HRWRK_w$)	-8.45	(-17.96)			0.0188	(2.06)		
			-7.48	(-3.55)	-0.00700	(-0.724)		
	-8.45	(17.85)					0.066	(1.69)
			-7.13	(-3.33)			-0.0409	(-0.980)
Annual hours ($WKWRK_y \cdot HRWRK_w$)	-420.95	(-16.91)			1.05	(2.19)		
			-355.26	3.21)	-0.229	(-0.45)		
	-421.6	(-16.84)					4.08	(1.98)
			-344.2	(-3.06)			-1.29	(-0.587)

[a]The rest of the explanatory variables in these equations are the same as those in Table 6.

[b]Variable measured in $1000s.

Table 12

ESTIMATED WAGE AND NET WORTH COEFFICIENTS FOR VARIOUS

FORMS OF IMPUTED WAGE AND IMPUTED NET WORTH[a]

Dependent Variable Form of Imputation[b]	Wage Coefficient	Wage t-Value	Net Worth[c] Coefficient	Net Worth[c] t-Value
Weeks Per Year				
ILW/INW	0.[illegible]8	(0.778)	0.0190	(2.18)
ILW_X/INW_A	0.[illegible]62	(1.69)	0.0229	(2.43)
ILW_X/INW_A[d]	0.0644	(0.432)	0.0206	(2.21)
$ILW_X/INW_{A,C}$	0.647	(1.05)	0.0221	(2.35)
$ILW_X/INW_{A,C,R}$	0.690	(1.75)	0.0077	(0.434)
ILW/INW2	0.418	(0.939)	0.0190	(1.50)
$ILW_X/INW2_A$	0.751	(1.90)	0.0271	(1.68)
Hours Per Week				
ILW/INW	-7.13	(-3.33)	-0.0409	(-0.098)
ILW_X/INW_A	-5.08	(-2.70)	-0.114	(-2.53)
ILW_X/INW_A[d]	1.02	(1.42)	-0.091	(-2.03)
$ILW_X/INW_{A,C}$	-5.02	(-2.67)	-0.0957	(-2.12)
$ILW_X/INW_{A,C,R}$	-5.22	(-2.76)	-0.0374	(-0.439)
ILW/INW2	-8.21	(-3.85)	0.119	(1.96)
$ILW_X/INW2_A$	-5.18	(-2.73)	-0.0130	(-0.169)
Annual Hours				
ILW/INW	-344.2	(-3.06)	-1.29	(-0.59)
ILW_X/INW_A	-221.5	(-2.24)	-4.87	(-2.06)
ILW_X/INW_A[d]	58.95	(1.57)	-3.80	(-1.62)
$ILW_X/INW_{A,C}$	-219.1	(-2.22)	-3.93	(-1.66)
$ILW_X/INW_{A,C,R}$	-227.4	(-2.29)	-1.68	(-0.376)
ILW/INW2	-397.1	(-3.55)	6.95	(2.19)
$ILW_X/INW2_A$	-222.3	(-2.23)	0.576	(0.142)

[a] All other variables as in Table 6.

[b] ILW = imputed natural logarithm of wage
ILW_X = imputed log of wage adjusted for experience
INW = imputed net worth
INW_A = imputed net worth adjusted for age
$INW_{A,C}$ = imputed net worth adjusted for age and children
$INW_{A,C,R}$ = imputed net worth adjusted for age, children, and renters
INW2 = imputed net worth based on second form of net worth regression (Table E-3)
$INW2_A$ = imputed net worth based on second form of net worth regression adjusted for age

[c] Net worth in $1000.

[d] Education excluded from right hand side of equations.

The potential for serious multicollinearity among husband's imputed wage, imputed net worth, and education is well illustrated by the fact that the correlation between education and husband's imputed wage is .77 and between imputed wage and imputed net worth is .60. As a further indication, the third row in Table 12 gives the results of regressions on experience-adjusted imputed wage and age-adjusted net worth but with education left out of the right-hand variables. The sensitivity of the imputed wage coefficient to this respecification is dramatic (a change from -221.5 to +58.95); as discussed above, a specification bias introduced into the labor supply regression by not including education may account for part or all of this increase in the imputed wage coefficient.

The next two rows in the table demonstrate the effect on the imputed net worth coefficient of adjusting INW_A for numbers of children and then for housing status. Both of these adjustments make the net worth coefficient less negative and reduce its signficance. The last two lines indicate the sensitivity of the net worth coefficient to respecification of the net worth imputing equations. Note that net worth imputation based on a regression that does not contain either children or a rent-homeowner dummy always yields positive net worth coefficients, although age-adjusting does significantly reduce the size of that coefficient.

A final comparison of these results can be obtained by calculating the compensated wage effects implied by the various imputed wage and net worth coefficients (Table 13). Since it seems pointless to "compensate" the wage coefficient in the wrong direction, compensated wage effects were calculated only for annual hours regressions in which the imputed net worth coefficient was of the "right" (negative) sign. Although not all the compensated wage effects have the predicted positive sign, it should be noted that the negative cases are based on regressions in which the imputed net worth coefficients were statistically insignificant.[1]

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[1]Some explanation may be necessary for the choice of interest rates given in Table 13 (10 percent and 5 percent). Some interest rate must be used to convert the stock net worth into a flow of

Table 13

COMPENSATED WAGE EFFECTS -- ANNUAL HOURS[a]

Specification of Regression Equation	Interest Rate (r) 10 percent	5 percent
ILW/INW	-66.3	-36.0
ILW_X/INW_A	52.4	167.1
ILW_X/INW_A[c]	106.0	195.5
$ILW_X/INW_{A,C}$	31.0	123.5
$ILW_X/INW_{A,C,R}$	-24.3	15.2

[a]Calculated from coefficients in Table 12. Compensated elasticities can be calculated by multiplying these effects by a factor of .0015 (= $\bar{W}/\bar{H}$).

Formula: $\frac{\alpha_1}{\bar{W}} - \bar{H} \cdot \frac{1}{r} \cdot \alpha_2$,

where

α_1 = coefficient of ln (wage)

$\bar{W}$ = mean wage

$\bar{H}$ = mean annual hours

r = return on net worth

α_2 = coefficient of net worth.

[b]See Table 12 for definitions.

[c]Based on regressions with education excluded from right hand side.

Three conclusions can be drawn from the results in Tables 11 and 12. First, imputing measures of husband's wage and net worth does affect the estimated relationship between these two variables and labor supplied, but not as substantially as one might have expected *ex ante*. Second, a methodology such as the one we have used to rid regressions of age influences is extremely sensitive to differences in specification of the imputing regressions. Third, even though the results are less than totally convincing, age adjusting in the manner postulated above *in general* has the desired effect of making the imputed wage coefficient less negative and the imputed asset coefficient more negative. Additional adjustments for either children or renters-non-renters are of dubious empirical value.

WIFE'S "WAGE" AND HUSBAND'S LABOR SUPPLY

Recent economic models of the family have emphasized that a husband's labor supply may be affected not only by his own wage but by his wife's wage (or potential wage) as well. If the wife works, her earnings will have an income effect on her husband's labor supply, which economic theory predicts to be negative; in addition, her wage will have a cross-substitution effect on her husband's labor supply The direction of this effect is uncertain, *a priori*. Assuming household activities have three inputs--husband's time, wife's time, and purchased market goods and services--the cross-substitution effect between husband's labor supply and wife's wage will be positive if husband's and wife's time at home are net complements, but negative if the two inputs are net substitutes.[1] When the time inputs are

nonwage-related income since only the latter can be used to complete compensated wage effects. Since individuals are being compared at a point in time, we want a real (as opposed to nominal) rate of return on net worth held by the household. The 5 percent interest rate probably more closely reflects actual conditions than the 10 percent interest rate; in any case, the two rates do indicate that the calculations are highly sensitive to this choice.

[1]A formal derivation of these results in the context of the household production model is given in De Tray (1973b).

complements, the income and substitution effects work in opposite directions and the net effect of a change in a working wife's wage is ambiguous. In previous work both negative (Smith, 1972) and positive (Ashenfelter and Heckman, 1971) effects of wife's wage on husband's labor supply have been found.

To this point in our empirical work we have attempted to capture this effect by including wife's earnings as a right-hand variable, a procedure used in several previous studies (for example, Greenberg and Kosters, 1971; and Garfinkel, 1971). As indicated above, this approach has several serious drawbacks: (1) the hours component of a wife's earnings is probably jointly determined with her husband's labor supply; (2) high wife's earnings do not necessarily imply high wife's wage; and, finally, (3) zero wife's earnings clearly do not mean the wife's potential wage is zero. In this subsection we explore the sensitivity of parameter estimates to the use of alternative measures of wife's earning capacity.

To make direct comparisons between the effect of wife's earnings and the effect of wife's *observed* wage, we first further restrict our sample to those families in which both husbands and wives work. With this subsample, which consists of 632 households, we are able to examine the differential effect on the male wage and net worth coefficients of using observed female earnings, observed female wage, or imputed female wage. We then return to our original (2012) sample for comparisons between wife's observed earnings and wife's imputed wage.

Although a number of authors have imputed wages to working and nonworking wives using regressions estimated from a sample of working wives (for example, Boskin, 1971; Hall, 1971; Kalachek and Raines, 1970; and Schultz, forthcoming), this approach is subject to several potentially serious problems. For example, the relatively smaller portion of women who participate in market work at any point in time increases the possibility of selectivity bias (see pp. 23-24); and, as we point out in Appendix C, the specification of the wage imputing equation may be especially poor for women (see p. 122). In order to make the following comparisons, we will: (1) ignore problems associated with selectivity bias; (2) assume that our wage estimating equation yields a wage that, when experience-adjusted, does measure average *lifetime* market wage; and (3) assume that since most women work at some point in their life cycle (90 percent in 1960), their predicted average lifetime wage does measure their value of time at the margin (see Heckman, 1972, and Gronau, 1971, for other approaches to this problem).

Turning first to the subsample that contains only working wives (Table 14), we note that the relationship between wife's earnings and weeks worked by the husband is positive (t = 1.8), but that the relationship between wife's earnings and hours worked per week is negative (t = -1.45). Although the female earnings coefficient is not especially interesting as such, the sign reversal between the weeks-per-year and hours-per-week components of labor supply is emerging as a surprisingly persistent and disturbing pattern.

The relationship between wife's observed wage and husband's labor supply is consistently negative for this sample, but it is never statistically significant. When wife's observed wage is replaced by its imputed counterpart, the coefficients are again never significantly different from zero, but this time they are consistently positive. Since none of these coefficients is significant at conventional levels, comparison is not very meaningful; it appears, however, that using an imputed rather than an observed wage changes the sign of the estimated relationship between wife's wage and husband's labor supply from negative to positive.

One important finding in Table 14 is the insensitivity of the male wage coefficient to the form in which wife's "wage" is entered in the regression (see lines 1 and 2 for each labor supply measure). It appears that misspecifying the wife's wage variable (by, say, using female earnings as a proxy) has little effect on the estimated male wage coefficient.[1]

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[1]As a final check, we excluded any measure of wife's wage or earnings from the regression; again, the resulting male wage and net worth coefficients were virtually unchanged from those estimated from other specifications. This may, however, be a function of the very restricted nature of the sample of 632 households.

Table 14

WIFE'S EARNINGS/WAGE COMPARISONS: WORKING WIVES ONLY (n = 632)

	Form of Principal Variables[a]			Female Earnings/Wage		Male Wage		Net Worth[d]	
Dependent Variable	Female Earnings (E) or Wage (W)	Male Wage	Net Worth	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Weeks per year	E - Obs.	Imp.	Imp.	0.079	(1.81)	-0.387	(-0.36)	0.0527	(2.77)
	W - Obs	Imp.	Imp.	-7.81	(-0.04)	-0.406	(-0.38)	0.0589	(3.08)
	W - Imp.	Imp.	Imp.	0.296	(0.54)	-0.456	(-0.43)	0.0548	(2.72)
	W - Imp. Adj.[b]	Imp. Adj.[b]	Imp. Adj.[c]	0.247	(0.46)	-0.316	(-0.33)	0.0580	(2.68)
Hours per week	E - Obs.	Imp.	Imp.	-0.232	(-1.45)	-7.17	(-1.86)	-0.051	(-0.74)
	W - Obs.	Imp.	Imp.	-0.626	(-0.95)	-7.03	(-1.81)	-0.056	(-0.81)
	W - Imp.	Imp.	Imp.	0.935	(0.47)	-7.27	(-1.87)	-0.0811	(-1.10)
	W - Imp. Adj.[b]	Imp. Adj[b]	Imp. Adj.[c]	1.67	(0.85)	-6.98	(-2.03)	-0.178	(-2.27)
Annual hours	E - Obs.	Imp.	Imp.	-7.88	(-0.93)	-389.2	(-1.50)	-0.261	(-0.07)
	W - Obs.	Imp.	Imp.	-33.95	(-0.98)	-382.7	(-1.86)	-0.194	(-0.05)
	W - Imp.	Imp.	Imp.	67.80	(0.64)	-398.6	(-1.93)	-1.76	(-0.45)
	W - Imp. Adj.[b]	Imp. Adj.[b]	Imp. Adj.[c]	133.97	(1.00)	-377.9	(-2.06)	-6.69	(-1.60)

[a]Obs = observed
Imp. = imputed
Imp. Adj. = Imputed adjusted.

[b]Adjusted for experience.

[c]Adjusted for age.

[d]Net worth in $1000s.

The net worth coefficient is also relatively stable across the different wife's wage/earnings variables. An exception occurs in the annual hours results when the wife's imputed wage is substituted for her actual wage, but it should be noted that this comparison is between two insignificant coefficients.

The results for the full restricted sample (2012) (Table 15) are similar to those for the 632 sample: the coefficients for husband's wage and net worth are not affected by the change from wife's earnings to wife's imputed wage; although significance levels are still low (t-ratios in the hours equation ranging from 1.04 to 1.54), the wife's positive imputed wage coefficient implies that an increase in a wife's wage induces her husband to work more hours in the marketplace.

ALTERNATIVE SAMPLES

So far, our empirical estimates have been based on a very restricted sample that excluded as many households presenting special estimation problems as possible (see Table 5). Although this procedure avoids some problems, it may be the source of others. Moreover, as indicated by Table 4, the samples vary considerably among labor supply studies. Many of the observations we have excluded so far have been included in the sample populations used in some previous labor supply research, but excluded from others. These differences in sample composition may be an important source of the variation in the results of these studies.

In this subsection, we report the effect on parameter estimates of adding to our "restricted" sample (of 2,012 households) 3,282 of the 4,213 SEO households headed by white married males, 25 to 54 years of age, that were initially excluded.[1] A comparison of estimated wage and net worth coefficients for the restricted and the "unrestricted"

[1]We continue to exclude the 931 households where the head was in the military or an institution, where there was another person over 18 years old besides the head and his spouse, or where there were missing values for key variables such as labor supply, age, or education.

Table 15

WIFE'S EARNINGS/WAGE COMPARISONS: RESTRICTED SAMPLE (n = 2012)

Dependent Variable	Form of Principal Variables[a]: Female Earnings (E) or Wage (W)	Male Wage	Net Worth	Female Earnings/Wage Coefficient	Female Earnings/Wage t-Value	Male Wage Coefficient	Male Wage t-Value	Net Worth[b] Coefficient	Net Worth[b] t-Value
Weeks per year	E - Obs.	Imp.	Imp.	0.0164	(0.78)	0.348	(0.78)	0.0190	(2.13)
	W - Imp.	Imp.	Imp.	0.0466	(0.17)	0.345	(0.77)	0.0191	(2.08)
	W - Imp. Adj.	Imp. Adj.	Imp. Adj.	0.00239	(0.01)	0.666	(1.70)	0.0235	(2.33)
Hours per week	E - Obs.	Imp.	Imp.	-0.189	(-1.87)	-7.13	(-3.33)	-0.0409	(-0.98)
	W - Imp.	Imp.	Imp.	1.35	(1.04)	-7.12	(-3.32)	-0.0636	(-1.44)
	W - Imp. Adj.	Imp. Adj.	Imp. Adj.	1.93	(1.52)	-5.17	(-2.75)	-0.148	(-3.07)
Annual Hours	E - Obs.	Imp.	Imp.	-8.71	(-1.65)	-344.16	(-3.06)	-1.29	(-0.59)
	W - Imp.	Imp.	Imp.	74.47	(1.10)	-344.04	(-3.06)	-2.47	(-1.07)
	W - Imp. Adj.	Imp. Adj.	Imp. Adj.	102.52	(1.54)	-225.81	(-2.29)	-6.63	(-2.61)

[a] Obs. = observed
Imp. = imputed
Imp. Adj. = Imputed adjusted

[b] Net worth in $1000s.

samples is presented in Table 16.[1] Since only imputed measures of net worth and the husband's wage rate are available for 1,969 members of the unrestricted sample, Table 16 also reports results for an intermediate sample composed of that subset of households in the unrestricted sample for whom *observed* measures of these variables can be constructed.

Ignoring for the moment the regression specifications that include the dummy variables, we note that both the net worth and male wage coefficients almost always become more positive as the sample becomes less restrictive. The coefficients of the female earnings and wage variable, however, almost always become more negative as the sample becomes less restrictive. These results are true for all three labor supply measures and for both the observed and imputed forms of the wage and net worth variables. In many cases these differences are substantial. This is especially clear in a comparison of the unrestricted sample with either of the other two samples, a comparison that captures many of the major sampling differences frequently found between studies that use observed measures of the wage rate and those that use imputed measures. Male wage and net worth coefficients that were positive in regressions computed on the restricted or intermediate samples become larger and generally more significant in the regressions using the unrestricted sample; coefficients that were initially negative invariably change signs. Taken at face value, these results suggest that substitution effects are weaker and income effects stronger for those who are included in the restricted sample than for those who are left out. Before we make such a conclusion, however, it is important to probe more deeply.

[1]The number of observations included in the unrestricted sample depends on which measure of labor supply is being used. There are some persons (the self-employed, for example) for whom the weeks-per-year variable is available, but the hours-per-week and annual hours measures are not. And there are others (those who did not work during the week before the survey interview) for whom both the hours-per-week and weeks-per-year measures can be constructed, but the annual hours variable cannot.

Table 16a

ESTIMATED LABOR SUPPLY PARAMETERS FOR THREE ALTERNATIVE SAMPLES:

WEEKS PER YEAR

Sample / Specification of Equation / Form of Wage and Net Worth Variables	Male Wage Coefficient	Male Wage t-Value	Net Worth[b] Coefficient	Net Worth[b] t-Value	Female Earnings or Wage[a] Coefficient	Female Earnings or Wage[a] t-Value	Husband Disabled[c] Coefficient	Husband Disabled[c] t-Value	Student Husband[c] Coefficient	Student Husband[c] t-Value	Husband Business Income Recipient[c] Coefficient	Husband Business Income Recipient[c] t-Value	Wife's Labor Supply Constrained[c] Coefficient	Wife's Labor Supply Constrained[c] t-Value
Restricted Sample (2012)														
Observed	0.339	(3.22)	0.00190	(0.94)	0 0262	(1.25)								
Imputed	0.344	(0.77)	0.0190	(2.08)	0.0466	(0.17)								
Adjusted imputed	0.666	(1.70)	[illegible]	(2 33)	0.00239	(0.01)								
All Men With Observed Wage and Net Worth (3320)														
Without Dummies														
Observed	0.586	(2.79)	0.0116	(2.37)	0.0250	(0.55)								
Imputed	0 382	(0.52)	0.0993	(5.12)	-0.996	(-1.78)								
Adjusted imputed	1.98	(2.98)	0.113	(5.31)	-1.12	(-2.05)								
With Dummies														
Observed	0.420	(2.18)	0.00812	(1.80)	0.0467	(1.12)	-8.87	(-23.59)	-3.60	(-8.07)	-0.00678	(-0.01)	0.929	(3.16)
Imputed	-0.361	(-0.79)	(0.0798)	(4.48)	-0.558	(-1.09)	-8.87	(-23.61)	-3.55	(-7.96)	-0.0889	(-0.16)	0.903	(3.08)
Adjusted imputed	1.38	(2.16)	0.0973	(4.97)	-0.769	(-1.53)	-8.83	(-23.55)	-3.58	(-8.05)	0.322	(0.580)	0 914	(3.12)
Unrestricted Sample (5289)														
Without Dummies														
Imputed	3.86	(4.11)	0.143	(5.22)	-1.65	(-2.09)								
Adjusted imputed	6.18	(7 27)	0.199	(6.49)	-1.91	(-2.48)								
With Dummies														
Imputed	1.70	(2.04)	0.126	(5.23)	-1.60	(-2.33)	-16.78	(-39.64)	-6.62	(-11.51)	1.63	([illegible]94)	-0.873	(-3.06)
Adjusted imputed	3.67	(4.91)	0.164	(6.14)	-1.76	(-2.62)	-16.70	(-39.62)	-6.67	(-11.66)	1.75	(5.34)	-0.837	(-2.94)

[a]The observed variable is measured in $1,000's of female earnings; the imputed variable is an imputed wage rate for the wife.

[b]Measured in $1,000's.

[c]Each of the dummy variables has a value of one if the household has the indicated characteristic and zero otherwise. The characteristics are defined as follows:

<u>Husband Disabled</u>. Husband reported that illness or disability affected the number of hours he could work in 1966.
<u>Student Husband</u>. Husband was a student at the time of the SEO interview or in 1966.
<u>Husband Business Income Recipient</u>. Husband received income from a farm or business in 1966.
<u>Wife's Labor Supply Constrained</u>. Wife reported that demand conditions or her health affected the number of hours she would work in 1966.

Table 16b

ESTIMATED LABOR SUPPLY PARAMETERS FOR THREE ALTERNATIVE SAMPLES:

HOURS PER WEEK

Sample / Specification of Equation / Form of Wage and Net Worth Variables	Male Wage Coefficient	Male Wage t-Value	Net Worth[b] Coefficient	Net Worth[b] t-Value	Female Earnings or Wage[a] Coefficient	Female Earnings or Wage[a] t-Value	Husband Disabled[c] Coefficient	Husband Disabled[c] t-Value	Student Husband[c] Coefficient	Student Husband[c] t-Value	Husband Business Income Recipient[c] Coefficient	Husband Business Income Recipient[c] t-Value	Wife's Labor Supply Constrained[c] Coefficient	Wife's Labor Supply Constrained[c] t-Value
Restricted Sample (2012)														
Observed	-8.45	(-17.96)	0.0188	(2.06)	-0.350	(-3.73)								
Imputed	-7.12	(-3.32)	-0.0636	(-1.44)	1.35	(1.04)								
Adjusted imputed	-5.17	(-2.75)	-0.148	(-3.07)	1.93	(1.52)								
All Men With Observed Wage and Net Worth (3320)														
Without Dummies														
Observed	-8.92	(-21.26)	0.0335	(3.43)	-0.366	(-4.02)								
Imputed	-1.46	(-0.93)	-0.0922	(-2.23)	1.41	(1.18)								
Adjusted imputed	-1.16	(-0.82)	-0.141	(-3.08)	1.82	(1.55)								
With Dummies														
Observed	-9.01	(-21.48)	0.0343	(3.49)	-0.341	(-3.75)	0.0258	(0.031)	-3.88	(-3.99)	-1.40	(-1.19)	0.957	(1.49)
Imputed	-2.02	(-1.22)	-0.0971	(-2.34)	1.71	(1.42)	0.318	(0.364)	-3.24	(-3.12)	-1.05	(-0.80)	1.22	(1.79)
Adjusted imputed	-1.55	(-1.05)	-0.142	(-3.10)	2.04	(1.74)	0.340	(0.39)	-3.15	(-3.04)	-0.777	(-0.60)	1.21	(1.77)
Unrestricted Sample (4398)														
Without Dummies														
Imputed	6.29	(2.99)	0.0853	(1.53)	-0.827	(-0.52)								
Adjusted imputed	7.36	(3.85)	0.0916	(1.48)	-0.505	(-0.32)								
With Dummies														
Imputed	4.95	(2.28)	0.0331	(0.60)	0.237	(0.15)	-11.13	(-11.22)	-7.83	(-5.70)	5.46	(2.78)	4.37	(4.24)
Adjusted imputed	5.98	(3.08)	0.0478	(0.79)	0.364	(0.24)	-11.07	(-11.19)	-7.85	(-5.74)	5.59	(2.89)	4.36	(4.23)

[a] The observed variable is measured in $1,000's of female earnings; the imputed variable is an imputed wage rate for the wife.

[b] Measured in $1,000's.

[c] Each of the dummy variables has a value of one if the household has the indicated characteristic and zero otherwise. The characteristics are defined as follows:

<u>Husband Disabled</u>. Husband reported that illness or disability affected the number of hours he could work in 1966.
<u>Student Husband</u>. Husband was a student at the time of the SEO interview or in 1966.
<u>Husband Business Income Recipient</u>. Husband received income from a farm or business in 1966.
<u>Wife's Labor Supply Constrained</u>. Wife reported that demand conditions or her health affected the number of hours she could work in 1966.

Table 16c

ESTIMATED LABOR SUPPLY PARAMETERS FOR THREE ALTERNATIVE SAMPLES:

ANNUAL HOURS

Specification of Equation Form of Wage and Net Worth Variables	Male Wage		Net Worth[b]		Female Earnings or Wage[a]		Husband Disabled[c]		Student Husband[c]		Husband Business Income Recipient[c]		Wife's Labor Supply Constrained[c]	
	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Restricted Sample (2012)														
Observed	-420.95	(-16.91)	1.06	(2.19)	-16.59	(-3.34)								
Imputed	-344.04	(-3.06)	-2.47	(-1.07)	74.47	(1.10)								
Adjusted imputed	-225.81	(-2.29)	-6.63	(-2.61)	102.52	(1.54)								
All Men With Observed Wage and Net Worth (3320)														
Without Dummies														
Observed	-413.10	(-17.52)	2.23	(4.07)	-17.15	(-3.35)								
Imputed	-55.12	(-0.69)	0.957	(0.42)	32.96	(0.50)								
Adjusted imputed	55.08	(0.70)	-0.715	(-0.28)	48.16	(0.74)								
With Dummies														
Observed	-426.50	(-18.54)	2.07	(3.85)	-14.86	(-2.98)	-496.85	(-11.06)	-381.49	(-7.15)	-64.92	(-1.01)	89.20	(2.54)
Imputed	-133.95	(-1.50)	-0.340	(-0.16)	71.26	(1.10)	-482.18	(-10.21)	-347.96	(-6.20)	-53.64	(-0.76)	101.11	(2.74)
Adjusted imputed	4.22	(0.053)	-1.67	(-0.68)	78.34	(1.24)	-478.72	(-10.14)	-344.98	(-6.16)	-15.58	(-0.22)	101.09	(2.74)
Unrestricted Sample (4106)														
Without Dummies														
Imputed	182.46	(2.14)	3.46	(1.53)	-2.55	(-0.04)								
Adjusted imputed	279.96	(3.62)	3.13	(1.24)	11.52	(0.18)								
With Dummies														
Imputed	15.29	(0.18)	1.19	(0.56)	52.27	(0.86)	-838.15	(-20.61)	-442.99	(-8.07)	39.43	(0.53)	107.92	(2.77)
Adjusted imputed	144.49	(1.90)	0.995	(0.42)	56.74	(0.95)	-831.94	(-20.50)	-441.91	(-8.07)	70.47	(0.96)	108.01	(2.78)

[a] The observed variable is measured in $1,000's of female earnings, the imputed variable is an imputed wage rate for the wife.

[b] Measured in $1,000's.

[c] Each of the dummy variables has a value of one if the household has the indicated characteristic and zero otherwise. The characteristics are defined as follows:

<u>Husband Disabled</u>. Husband reported that illness or disability affected the number of hours he could work in 1966.
<u>Student Husband</u>. Husband was a student at the time of the SEO interview or in 1966.
<u>Husband Business Income Recipient</u>. Husband received income from a farm or business in 1966.
<u>Wife's Labor Supply Constrained</u>. Wife reported that demand conditions or her health affected the number of hours she could work in 1966.

Some insight into the factors causing these rather striking changes can be obtained through the use of dummy variables that represent the reasons certain of the households in the unrestricted sample were excluded from the restricted sample. Results from regression runs that include such dummies are reported in Table 16. These dummies are an attempt to control for specification error that may otherwise occur when households having the characteristics indicated by the dummies are admitted to the sample population.[1] For example, the wage that a disabled person can obtain and the number of hours he is physically able to work are both likely to be relatively low. However, it is also possible that the disabled person freely chooses to work low hours *because* his market wage is low. The intent of the disability dummy is to separate these two possibilities. Labor supply and wage rates are also both likely to be influenced by the fact that an individual is currently a student; both measures may contain substantial negative transitory components.[2] The student dummy should help to take account of this possibility. The purpose of the business income dummy is to correct for the possibilities that recipients of business and farm income, many of whom are self-employed, have systematically different tastes for work than the rest of the population and that the wage and net worth variables for these persons are especially subject to systematic measurement error. The wife's labor supply constraint dummy is included to account for the possibility that if a wife cannot work as many hours as she would like,

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[1]Including the dummy variables in the regressions allows for differences in intercept terms between households having the indicated characteristics and the rest of the sample population, but does not take account of the possibility that the labor supply responses of these households to wage and income changes may also differ from the remaining sample. Investigation of this important topic, which requires sets of interaction terms or computation of separate regressions for each subgroup, will be conducted in our future research.

[2]One purpose of using an imputed wage variable is, of course, to free the wage measure of transitory components. However, one of the major predictors of most imputed wage rates is education. Since the completed years of education of a person who is currently a student is understated, his imputed wage will be lower than his permanent wage.

her husband's labor supply will be higher than it otherwise would be.

In almost all cases where the coefficients on the dummy variables are statistically significant or nearly significant by conventional standards, the coefficients have the expected sign. Other things being the same, men who are students or disabled appear to work fewer hours than other men, while recipients of business income apparently tend to work greater hours. It was also expected that men whose wives' labor supplies were constrained would work more hours than other men, and in most cases this hypothesis is borne out. However, the weeks-per-year regressions for the unrestricted sample indicate that men whose wives are unemployed or disabled work almost a week less per year than other men in the unrestricted sample.

More important for our purposes are the effects on the male wage and net worth coefficients of adding the dummy variables. With but one exception, these coefficients become more negative when the dummies are added to the regressions. (However, the addition of the dummies always causes the female earnings and female wage coefficients to become more positive.) In other words, the dummy variables cause the labor supply parameter estimates for the less restrictive samples to move toward those for the restricted sample. Generally, however, the pattern that we found before the dummy variables were added to the regressions continues to hold: The male wage and net worth coefficie s usually become more positive as the sample becomes less res ctive.

An explanation for the effect of the dummy variables on the wage and net worth coefficients is found by examining what happens to the dependent variables as the sample becomes less restrictive. As we pointed out earlier, excluding persons from the sample who were unemployed, disabled, self-employed, and the like greatly reduces the overall variation in labor supply, especially weeks worked per year. Adding back such persons should increase variation in the labor supply measures. That this is indeed the case is indicated by Table 17, which presents summary statistics for the three samples used in the regressions reported in Table 16. The presence of the dummies in the regressions for the less restricted samples tends to

Table 17

SELECTED SUMMARY STATISTICS FOR THREE ALTERNATIVE SAMPLES

Sample / Variables	Restricted Sample		All Men with Observed Wage and Net Worth		Unrestricted Sample	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Weeks per year	51.84	1.63	50.63	4.56	49.27	8.51
Hours per week	45.43	7.94	44.18	9.63	40.63[b]	15.11[b]
Annual hours	2,354	416.8	2,234	532.9	2,203[c]	592.7[c]
Observed wage[d]	3.25	1.52	3.16	1.56	a	a
Imputed wage[d]	3.25	1.28	3.14	1.31	3.09	1.34
Observed net worth	10,790	19,000	10,080	17,110	a	a
Imputed net worth	9,477	6,597	8,941	6,598	9,097	6,642
Unemployed[e]	0.0	0.0	6.5	24.6	8.0	27.2
Disabled[e]	0.0	0.0	3.8	19.2	5.8	23.4
Student[e]	0.0	0.0	2.8	16.5	3.3	17.8
Number of observations	2,012		3,320		5,294	

[a]Not available for sample.

[b]Computed for the sub-group of 4,398 observations used in the hours-per-week regressions.

[c]Computed for the sub-group of 4,106 observations used in the annual hours regressions.

[d]Means and standard deviations are reported as the anti-logs of the means and standard deviations of the natural log of the wage rate.

[e]Percentage of the group having the characteristic.

make the wage and net worth coefficients in these regressions revert back toward their values in the regressions for the restricted sample by absorbing much of the additional intergroup variation in the dependent variable that occurs when the sample is expanded.

One difficulty with interpreting the results in Table 16 is that although the overall effects of adding previously excluded observations to the sample is apparent, it is unclear which particular subset of the incremental observations is most responsible for the observed changes. To observe the effect on parameter estimates of excluding individuals for only one particular reason, many of the observations that were excluded from the restricted sample but admitted to the unrestricted sample were categorized into nine mutually exclusive groups. For each of these groups, we computed a regression for the set of observations composed of the restricted sample plus the group. This procedure allows us to observe the marginal effect of each group on the parameter estimates. The regressions are reported in Table 18. Summary statistics for the nine groups are reported in Table 19. The characteristics of the observations in the nine marginal groups are:

1. Husband Unemployed. Husband reported that demand conditions affected the number of hours he could work in 1966 (the year preceding the SEO survey).

2. Husband Disabled. Husband reported that his health affected the number of hours he could work in 1966.

3. Student Husband. Husband was a student at the time of the SEO interview (February 1967) or in 1966.

4. Husband Business Income Recipient. Husband received income from a business or farm in 1966.

5. Husband Self-employed. Husband was self-employed at the time of the SEO interview.

6. Inconsistencies. Husband's labor force status in 1966 was "inconsistent" with his status during week before the SEO interview (for example, part-time work last week but full-time work last year or a change in employers between the two time periods).

7. Husband Worked 0 Hours, Positive Weeks. Husband did not work during the week before the survey interview, but did work in 1966.

Table 18a

MARGINAL SAMPLE CHANGES:

WEEKS PER YEAR

Sample Form of Equation	Male Wage Coefficient	Male Wage t-Value	Net Worth[a] Coefficient	Net Worth[a] t-Value	Dummy Coefficient	Dummy t-Value	Dummy Coefficient	Dummy t-Value
Restricted Sample (2012)								
Observed measures	0.339	(3.22)	0.00190	(0.94)				
Imputed measures	0.344	(0.77)	0.0191	(2.08)				
Restricted Sample + Unemployed (2101)								
Observed measures	-0.0248	(-0.14)	0.00499	(1.40)				
Imputed measures	0.489	(0.65)	0.0502	(3.20)				
Restricted Sample + Disabled (2089)					Husband Disabled			
Observed measures	0.375	(2.33)	0.00466	(1.48)				
Observed measures and dummy	0.218	(1.71)	0.00230	(0.92)	-8.36	(-35.39)		
Imputed measures and dummy	0.557	(1.07)	0.0201	(1.82)	-8.35	(-35.23)		
Restricted Sample + Students (2077)					Student Husband			
Observed measures	0.457	(3.64)	0.00214	(1.27)				
Observed measures and dummy	0.392	(3.18)	0.00261	(1.08)	-2.23	(-8.82)		
Imputed measures and dummy	0.545	(1.04)	0.0248	(2.32)	-2.25	(-8.89)		
Restricted Sample + Business Income Recipients (2060)					Husband Business Income Recipient			
Observed measures	0.351	(3.37)	0.00219	(1.09)				
Observed measures and dummy	0.350	(3.35)	0.00227	(1.12)	-0.080	(-0.32)		
Imputed measures and dummy	0.361	(0.90)	0.0168	(1.84)	-0.009	(-0.03)		
Restricted Sample + Self-Employed (2380)					Husband Business Income Recipient			
Imputed measures	-0.235	(-0.697)	0.0240	(2.7?)				
Imputed measures and dummy	-0.332	(-0.967)	0.0242	(2.74)	-0.162	(-1.63)		
Restricted Sample + Inconsistencies (2407)								
Observed measures	0.304	(2.88)	0.00326	(1.43)				
Imputed measures	-0.0823	(-0.18)	0.0309	(3.14)				
Restricted Sample + Men Who Worked 0 Hours, Positive Weeks (2304)					Husband Disabled		Student Husband	
Imputed measures	2.34	(2.08)	0.137	(5.62)				
Imputed measures and dummies	-0.585	(-0.56)	0.103	(4.60)	-12.80	(-18.61)	-9.64	(08.79)
Restricted Sample + Men Who Worked 0 Hours, 0 Weeks (2073)					Husband Disabled		Student Husband	
Imputed measures	22.73	(10.64)	0.109	(2.34)				
Imputed measures and dummies	0.564	(0.51)	0.0701	(3.02)	-49.67	(-75.10)	-38.80	(-18.42)
Restricted Sample + Men Whose Wives' Labor Supplies Were Constrained (2225)					Wives' Labor Supplies Constrained			
Observed measures	0.336	(3.33)	0.00200	(1.01)				
Observed measures and dummy	0.336	(3.33)	0.00200	(1.00)	-0.00670	(-0.06)		
Imputed measures and dummy	0.521	(1.26)	0.0203	(2.33)	-0.0123	(-0.10)		

[a]Measured in $1,000's.

Table 18b

MARGINAL SAMPLE CHANGES:

HOURS PER WEEK

Sample Form of Equation	Male Wage		Net Worth[a]		Dummy		Dummy	
	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Restricted Sample (2012)								
Observed measures	-8.45	(-17.96)	0.0183	(2.06)				
Imputed measures	-7.12	(-3.32)	-0.0636	(-1.44)				
Restricted Sample + Unemployed (2101)								
Observed measures	-8.17	(-17.54)	0.0177	(1.93)				
Imputed measures	-6.36	(-3.08)	-0.0608	(-1.40)				
Restricted Sample + Disabled (2089)					Husband Disabled			
Observed measures	-8.39	(-18.09)	0.0207	(2.29)				
Observed measures and dummy	-8.39	(-18.08)	0.0207	(2.28)	-0.175	(-0.20)		
Imputed measures and dummy	-7.45	(-3.50)	-0.0648	(-1.50)	0.0617	(0.07)		
Restricted Sample + Students (2077)					Student Husband			
Observed measures	-8.[illegible]7	(-17.63)	0.0195	(2.09)				
Observed measures and dummy	-8.46	(-17.82)	0.0188	(2.01)	-3.06	(-3.14)		
Imputed measures and dummy	-6.15	(-2.84)	-0.0638	(-1.44)	-2.31	(-2.21)		
Restricted Sample + Business Income Recipients (2060)					Husband Business Income Recipient			
Observed measures	-8.54	(-18.27)	0.0173	(1.92)				
Observed measures and dummy	-8.56	(-18.30)	0.0182	(2.01)	-0.965	(-0.867)		
Imputed measures and dummy	-5.77	(-2.97)	-0.0659	(-1.50)	-1.61	(-1.23)		
Restricted Sample + Inconsistencies (2407)								
Observed measures	-9.63	(-20.64)	0.0323	(3.20)				
Imputed measures	-4.92	(-2.26)	-0.0827	(-1.75)				
Restricted Sample + Men Who Worked 0 Hours, Positive Weeks (2304)					Husband Disabled		Student Husband	
Imputed measures	11.51	(2.87)	0.187	(2.16)				
Imputed measures and dummies	2.61	(0.69)	0.068	(0.84)	-37.42	(-15.00)	-41.24	(-10.41)
Restricted Sample + Men Who Worked 0 Hours, 0 Weeks (2073)								
Imputed measures	13.20	(4.87)	0.0207	(0.35)				
Imputed measures and dummies	-6.53	(-2.92)	-0.0135	(-0.29)	-44.20	(-33.04)	-34.44	(-8.08)
Restricted Sample + Men Whose Wives' Labor Supplies Were Constrained (2225)					Wives' Labor Supplies Constrained			
Observed measures	-8.61	(-19.15)	0.0193	(2.18)				
Observed measures and dummies	-8.62	(-19.15)	0.0192	(2.16)	-0.273	(-0.52)		
Imputed measures and dummies	-5.87	(-2.95)	-0.0678	(-1.62)	-0.0332	(-0.06)		

[a]Measured in $1,000's.

Table 18c

MARGINAL SAMPLE CHANGES:

ANNUAL HOURS

Sample / Form of Equation	Imputed Male Wage		Imputed Net Worth[a]		Dummy		Dummy	
	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value	Coefficient	t-Value
Restricted Sample (2012)								
Observed measures	-420.95	(-16.91	1.06	(2.19)				
Imputed measures	-344.04	(-3.06)	-2.47	(-1.07)				
Restricted Sample + Unemployed (2101)								
Observed measures	-420.20	(-16.46)	1.13	(2.25)				
Imputed measures	-303.21	(-2.70)	-0.938	(-0.40)				
Restricted Sample + Disabled (2089)					Husband Disabled			
Observed measures	-399.75	(-15.73)	1.21	(2.45)				
Observed measures and dummy	-407.95	(-16.38)	1.10	(2.25)	-435.61	(-9.42)		
Imputed measures and dummy	-299.51	(-2.76)	-2.57	(-1.12)	-422.36	(-8.58)		
Restricted Sample + Students (2077)					Student Husband			
Observed measures	-412.06	(-16.29)	1.15	(2.31)				
Observed measures and dummy	-419.33	(-16.63)	1.09	(2.20)	-249.35	(-4.82)		
Imputed measures and dummy	-290.82	(-2.55)	-2.39	(-1.03)	-211.86	(-3.85)		
Restricted Sample + Business Income Recipients (2060)					Husband Business Income Recipient			
Observed measures	-424.72	(-17.17)	0.988	(2.07)				
Observed measures and dummy	-425.88	(-17.20)	1.04	(2.17)	-53.21	(-0.903)		
Imputed measures and dummy	-272.74	(-2.67)	-2.71	(-1.17)	-82.98	(-1.11)		
Restricted Sample + Inconsistencies (2407)								
Observed measures	-482.23	(-19.54)	1.82	(3.41)				
Imputed measures	-257.72	(-2.26)	-2.84	(-1.15)				
Restricted Sample + Men Who Worked 0 Hours, 0 Weeks (2073)					Husband Disabled		Student Husband	
Imputed measures	703.31	(4.98)	1.87	(0.61)				
Imputed measures and dummies	-315.07	(-2.69)	0.105	(0.04)	-2281.8	(-32.57)	-1783.0	(-7.99)
Restricted Sample + Men Whose Wives' Labor Supplies Were Constrained (2225)					Wives' Labor Supplies Constrained			
Observed measures	-429.16	(-18.03)	1.09	(2.32)				
Observed measures and dummy	-429.43	(-18.03)	1.09	(2.31)	-13.60	(-0.48)		
Imputed measures and dummy	-273.37	(-2.61)	-2.63	(-1.20)	-1.54	(-0.05)		

[a] Measured in $1,000's.

Table 19

SUMMARY STATISTICS FOR THE RESTRICTED SAMPLE AND SUBGROUPS OF OBSERVATIONS EXCLUDED FROM THE RESTRICTED SAMPLE

Sample / Variables	Restricted Sample		Husband Unemployed		Husband Disabled		Student Husband		Husband Business Income	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Weeks	51.84	1.63	42.07	6.65	43.39	6.24	49.6	6.18	51.63	1.80
Hours per week	45.43	7.94	42.40	8.76	45.16	8.07	43.28	12.18	46.19	9.78
Annual hours	2354	416.8	1789.8	1437.1	1910.8	522.22	2153.2	660.8	2384.5	514.16
Observed wage[b]	3.25	1.52	3.06	1.48	2.68	1.41	3.12	1.41	2.46	1.51
Imputed wage[b]	3.25	1.28	2.79	1.28	2.77	1.32	3.54	1.23	2.00	1.55
Observed net worth	10,790	19,000	5,764.3	8,971.7	6,527.8	11,533.0	5,374.9	6,134.7	23,064	19,517
Imputed net worth	9,477	6,597	5,821.8	6,089.1	7,815.8	6,292	7,226.1	5,745.8	8,880.3	4,285.4
Unemployed[c]	0.0	0.0	100.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Disabled[c]	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0	0.0	0.0
Student[c]	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.0	0.0	0.0
Number of observations	2012		89		77		65		48	

[a]Not available.

[b]Means and standard deviations are reported as the anti-logarithms of the means and standard deviations of the natural log of the wage rate.

[c]Percentage of the group having the characteristic.

Table 19 continued

SUMMARY STATISTICS FOR THE RESTRICTED SAMPLE AND SUBGROUPS OF OBSERVATIONS EXCLUDED FROM THE RESTRICTED SAMPLE

Sample / Variables	Husband Self-Employed		Recipient Inconsistencies		Husband Worked 0 Hours, Positive Weeks		Husband Worked 0 Hours, 0 Weeks		Wife's Labor Supply Constrained	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Weeks	51.67	1.94	51.49	2.96	44.39	10.5	0.0	0.0	51.78	1.57
Hours per week	a	a	39.05	12.82	0.0	0.0	0.0	0.0	43.36	7.60
Annual hours	a	a	2009.6	669.13	a	a	0.0	0.0	2348.6	401.23
Observed wage[b]	a	a	3.24	1.74	a	a	a	a	3.02	1.46
Imputed wage[b]	3.01	1.43	3.07	1.32	2.86	1.32	2.27	1.41	3.07	1.30
Observed net worth	a	a	7,788.6	13,160	a	a	a	a	8,767.3	12,458
Imputed net worth	10,238	6,625.0	7,706.4	6,750.2	7,436.9	6,443.3	6,596.1	6,748.9	9,059.3	6,519.3
Unemployed[c]	0.0	0.0	0.0	0.0	29.8	45.7	4.9	21.6	0.0	0.0
Disabled[c]	0.0	0.0	0.0	0.0	14.0	34.7	77.0	42.1	0.0	0.0
Student[c]	0.0	0.0	0.0	0.0	5.5	22.8	6.6	24.8	0.0	0.0
Number of observations	368		393		292		61		213	

[a]Not available.

[b]Means and standard deviations are reported as the anti-logarithms of the means and standard deviations of the natural log of the wage rate.

[c]Percentage of the group having the characteristics.

8. Husband Worked 0 Hours, 0 Weeks. Husband did not work at all in 1966 and did not work during the week before the survey interview.[1]

9. Wife's Labor Supply Constrained. Wife reported that demand conditions or her health affected the number of hours she could work in 1966.

In defining these groups we have attempted to keep them mutually exclusive and relatively pure in order to isolate those changes in sample composition that have the greatest effect on the estimated parameters. For example, a man who was both unemployed and a student in 1966 was not assigned to any of the nine subgroups. We also wanted to separate those for whom observed wage and net worth variables are available from those for whom only imputed measures can be obtained. Since observed wages were not available for persons who did not work during the weeks preceding the SEO survey, we required that persons in all but groups 5, 7, and 8 had to have worked during that week. Thus, persons who were unemployed, disabled, or students and did not work the week preceding the survey are not included in the first three groups but do appear in groups 7 and 8. A person who both received business income in 1966 and was self-employed at the time of the SEO interview was placed in the self-employed group.

Table 18 indicates that of the nine marginal additions to the restricted sample the most dramatic changes in the male wage and net worth coefficients occur with the addition of men who did not work the week preceding the survey, especially those men who also did not work at all in 1966. Admission of either nonworkers (that is, zero hour, zero weeks persons) or zero hour, positive weeks persons to the sample population generally results in substantially more positive male wage and net worth coefficients. As expected, these results are particularly strong in regressions that do not include the dummy variables. Similar patterns, although not as strong or consistent, are found for unemployed husbands and disabled husbands: the addition of these two marginal groups to the sample, when it has much effect at

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[1] We found no men who did not work at all in 1966, but did work during the week before the survey interview.

all, is generally to make the wage and net worth coefficients more positive.[1]

Explanation for these results is found in the fact that persons who did not work in 1966 or during the week before the survey, or who reported that they were unemployed or disabled during part of 1966, have much lower hours than persons in the restricted sample, and they are also likely to have lower (potential) wages and smaller net worth (see Table 19). This suggests that persons with low wages and low net worth, who also work relatively few hours, tend to be systematically excluded from the sample in labor supply studies, while low wage, low net worth individuals who work relatively high hours are usually included. The consequence of this is illustrated in Figure 3, where we have plotted the mean imputed wage and mean weeks-per-year combinations for the restricted sample and for each of the nine marginal groups. The labor supply curve implied by the estimated relation between imputed wages and weeks per year for the restricted sample is also drawn. The points for unemployed husbands; disabled husbands; husbands with zero hours, positive weeks; and especially for nonworkers are all well to the left of the estimated labor supply curve and substantially below the mean-weeks-mean-wage point for the restricted sample, indicating as we have found that the addition of these groups to the sample usually results in a more positive estimated relation between imputed wages and weeks per year. Analogous diagrams that use observed wages, or observed or imputed net worth

[1]The major exception to this occurs in the weeks-per-year regressions where including the unemployed subsamples changes the observed wage coefficient from positive and significant (coefficient = .339, t = 3.22) to negative and insignificant (coefficient = -.02, t = -.14). It appears from these results that although the mean observed wage for the unemployed group is slightly lower than the corresponding mean for the restricted sample, the <u>relative</u> difference in observed wages is much less than the relative difference in weeks worked. We are, therefore, effectively adding back people whose wages are distributed much the same as the wages of husbands in our restricted sample, but whose weeks worked are much lower on the average than weeks worked for the 2012 sample; the net effect apparently is a zero statistical relationship between observed wages and weeks worked.

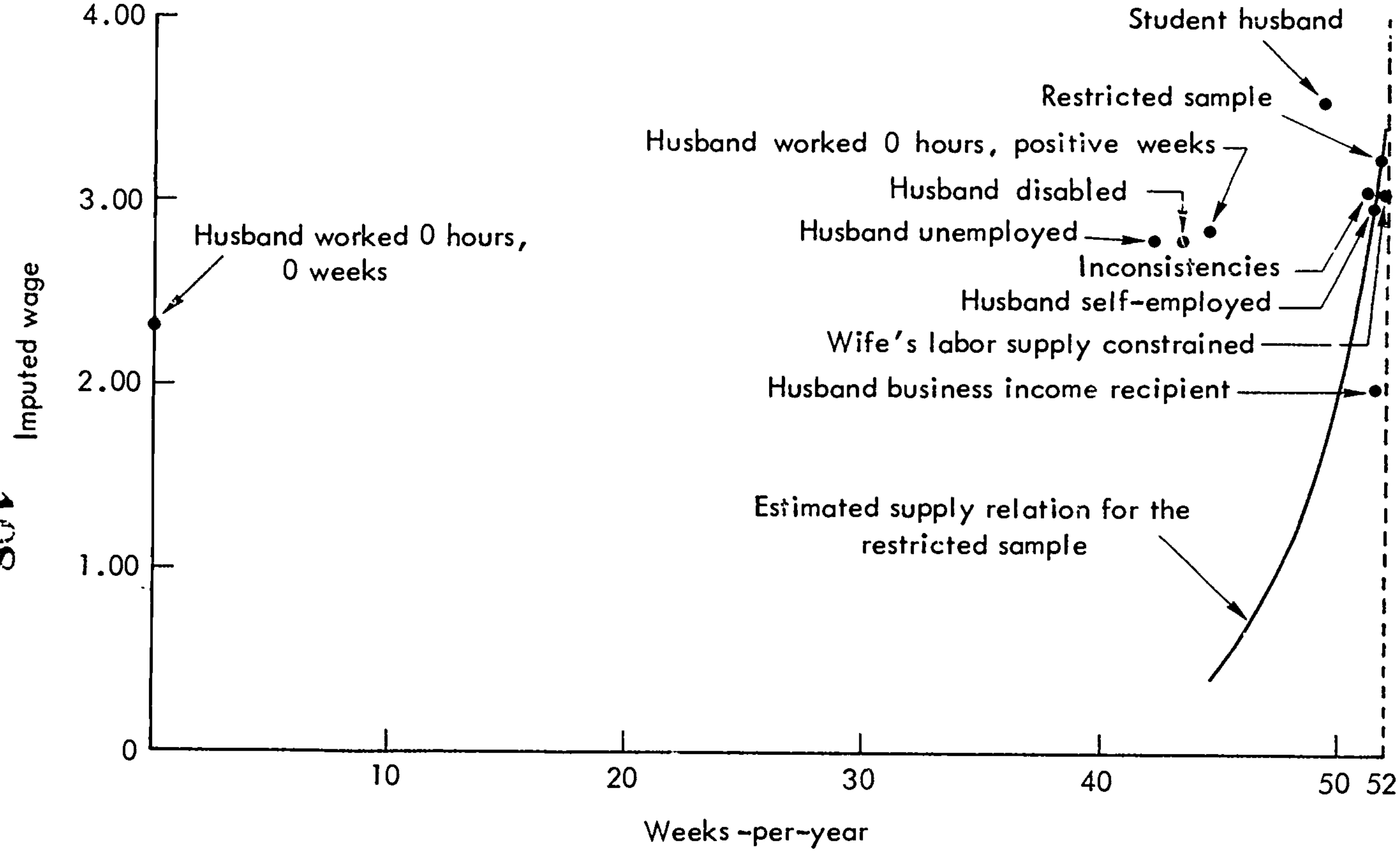

Fig. 3 — Estimated relation between weeks-per-year and imputed wages for the restricted sample, with mean imputed wage/mean weeks-per-year combinations for selected subgroups of observations

Source: Tables 18 and 19

on the vertical axis, or hours-per-week or annual hours on the horizontal axis have similar implications.

The results presented in this subsection have several important implications for labor supply studies. For example, a rather persistent difference between previous labor supply studies that have used observed wage measures and those that have used imputed wage measures is that the wage effects found in the latter tend to be more positive than those found in the former. In fact, not infrequently a negative relation is found between labor supply and observed wages, and a positive relation is found between labor supply and imputed wages. Our findings indicate that when observed wage coefficients and imputed wage coefficients are computed <u>for the same sample</u>, the coefficient of the imputed is usually relatively more positive (see Tables 16 and 18).[1] This difference, however, is apparently accentuated by the differences in sample composition between studies using observed wages and those using imputed wages. In particular, persons who did not work during the period over which wages are observed must usually be left out of sample in the former type of study but can be included in the latter type of study. Since the addition of such persons to the sample tends to have a positive effect on the male wage coefficient, much of the difference between the two types of studies can probably be traced to differences in sample composition.

The findings reported above also emphasize the importance in estimating labor supply functions of appropriately treating unemployed and disabled persons, for these persons make up most of the subgroups with zero hours and zero weeks, and zero hours and positive weeks.[2]

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[1]The difference between the observed and imputed wage coefficients is usually larger when the marginal groups are included in the sample than when they are excluded. The difference is especially great in the hours regressions for the sample that includes the inconsistencies group, indicating the presence of substantial measurement error in the observed wage variable for this group.

[2]For example, 77 percent of the zero hours, zero weeks subgroup reported that they were disabled in 1966, and some 30 percent of the zero hours, positive weeks subgroup reported that they were unemployed

It is unclear, however, whether the estimated regression coefficients more closely reflect reality when the unemployed and disabled are included in the sample or when they are excluded.

The appropriateness of leaving unemployed persons out of the sample depends on the extent to which reported unemployment is truly involuntary and represents a disequilibrium position. That considerable voluntarism may be involved is suggested by Cain and Watts (1973, pp. 348-349):

> For adult males in particular, the employment decisions are to some extent restricted to working full-time--that is, roughly 40 hours a week the year round--or to not working at all. However, over the course of a year it is likely that some flexibility is achieved by means of time between jobs or in absenteeism or time on layoffs or other forms of unemployment. Perhaps one manifestation of a positive substitution effect between 'labor and supply' and the potential wage rate is an inverse relation between these modes of not working and the wage rate. All such modes are likely to be reported as 'unemployment' by adult males in answer to survey questions.

The larger the voluntary component in reported unemployment, the greater will be the biases introduced by excluding the unemployed from the sample population. The researcher's predicament is, of course, that among those who report unemployment there is no way to distinguish between voluntary and involuntary unemployment. One approach to this problem is to attempt to place boundaries around the true labor supply parameters under the alternative assumptions that all reported unemployment is involuntary and that all of it is voluntary. The estimates could be computed for alternative samples that exclude and include those who report unemployment; or using a slightly different approach, labor supply could be alternatively measured as time

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part of 1966. Although we did not compute the number, most of the individuals in the zero hour, positive weeks subgroup probably gave unemployment as the reason they were not working in February 1967 when the SEO interview took place.

actually worked and as time worked plus time unemployed.[1] Our findings imply, however, that the resulting boundaries may be too broad to be very useful for drawing conclusions for policy purposes.

The disabled could remain within the sample population without problem if the effect of disability on hours of work operated entirely through the wage rate. However, the disabled may have different labor supply response parameters as well. Moreover, because of minimum wage laws and institutional rigidities a disabled person may not be able to work as many hours as he would like at his market wage. Although these problems can to some extent be controlled through independent variables that indicate the existence of a disability, it is tempting to play safe by excluding these persons from the sample population. As with the unemployed, the difficulty in this is that if to an important extent the low hours of the disabled represent a voluntary reaction to low wages, the omission of the disabled from the sample may result in an understatement of hours responsiveness to wage changes. Thus, it is unclear whether accurately estimating the lower part of the labor supply function is best accomplished by including or excluding the disabled from the sample.

The relative sensitivity of the results to adding individuals with zero hours per week or zero weeks per year to the sample may also indicate the importance of using an appropriate regression technique. Ordinary least squares, the regression technique we use, is inappropriate for bounded dependent variables. This problem is likely to be especially severe for the weeks worked per year regressions, since weeks worked has effective lower and upper bounds (0 weeks and 52 weeks). Although it was beyond the scope of our study to do so, future research should test the sensitivity of results to alternative regression techniques.[2]

[1] We computed several (unreported) regressions where the dependent variable was constructed by combining weeks worked with measured weeks of unemployment. The results imply that this procedure is very nearly equivalent to leaving the unemployed out of the sample population.

[2] Probit seems like a particularly appropriate technique, since it was designed to treat dependent variables that are bounded on both sides.

IV. SUMMARY AND CONCLUSIONS

In this report we have explored the sensitivity of labor supply parameter estimates to alternative ways of estimating wage and income effects and to changes in sample composition. This sensitivity analysis has enabled us to isolate some of the factors that strongly affect estimated labor supply paramters. These findings are summarized below.

Our analysis using only observed measures of wages and net worth mainly confirms what past labor supply studies have indicated:

- A negative bias is introduced into the wage coefficient when the dependent variable (or some part thereof) is used as a divisor to calculate the wage measure;
- There is some weak evidence of curvilinearity in the relationship between labor supply and wages, but only at very high and very low wage rates;
- Observed net worth and the amount of time individuals work are positively, not negatively, related; the size and significance of this relationship varies substantially among the different components of net worth.

It is sometimes argued that imputed measures of wages and assets overcome many of the shortcomings and biases that may be present in observed measures; our experiment with imputed wages and an imputed measure of net worth for the restricted sample led us to conclude that:

- The coefficient of the imputed male wage is always more positive than that of the observed wage. In the equations explaining hours worked, this result is consistent with the negative bias expected for observed wages. However, as long as the hours equations include male education as an explanatory variable, the sign of the imputed wage coefficient is always negative and only slightly smaller in absolute magnitude than the coefficient of the observed wage.

- The size and sign of the imputed wage coefficients in equations explaining hours of work are extremely sensitive to whether male education is included as an explanatory variable in the labor supply equation.
- Adjusting wage rates by cost-of-living indexes has almost no effect on estimated wage parameters.
- In equations explaining hours of work, the sign of the net worth coefficient (for both imputed and actual measures of net worth) depends on whether the male wage variable is observed or imputed but not on whether net worth is observed or imputed. When observed male wage is used in the regression, the sign of the net worth coefficient is usually positive; but when imputed male wage is entered in the regression, the net worth coefficient is usually negative.
- Adjusting imputed wages and imputed net worth for experience or age in general changes the coefficients in the "right" direction--that is, in a positive direction for the imputed wage coefficient and in a negative direction for the imputed net worth coefficients.
- Changing the specification of the net worth imputing regression can affect the sign of the net worth coefficient in the labor supply regression; when children and home status are left out of the net worth imputing regression, the relationship between imputed net worth and labor supplied changes from negative to positive.
- The relationships between labor supply and imputed wages and net worth are often of opposite signs for the two components of labor supply--weeks worked per year and hours worked per week.

In our exploration of the effect of wives' earning capacity on husbands' labor supply, we found that:

- Neither the husband's wage nor the net worth coefficients are sensitive to whether wife's earnings, wife's observed wage, or wife's imputed wage is entered in the labor supply regression or even to whether a "wife's" variable is entirely omitted from the regression.

In our tests concerning the effect of sample composition on estimated male wage and net worth parameters, we found that:

- Both the net worth and male wage coefficients become more positive as the sample becomes less restricted.
- The most dramatic positive changes in the male wage and net worth coefficients occur when men who did not work the week before the survey (especially those who did not work at all in 1966) are added to the sample.
- When dummies representing the reason for initial exclusion (for example, disability and student dummies) are included in the regression equation, there is less difference between parameter estimates for the restricted and those for the unrestricted samples, but the wage and net worth coefficients still become more positive as the sample becomes less restricted.
- The differences between studies that use observed wages and those that use imputed wages are due to differences in sample composition as well as to using observed or imputed wages per se. Our results indicate that when observed and imputed wage coefficients are computed for the same sample, the imputed are usually more positive than the observed. This difference is accentuated because studies that use observed wages must restrict their samples to those who report wages, whereas studies that impute wages tend to be less restrictive in defining their samples, often including nonworkers.

Our analysis thus indicates that major differences in estimated wage and income effects can often be attributed to one of three factors: (1) the measure of labor supply used, (2) the broadness of the sample used, especially the inclusion of individuals with zero values for the dependent variable, and (3) for those studies that used imputed wages, the inclusion or exclusion of education in the labor supply regression. The resolution of the differences we have reported is beyond the scope of this study, but our findings should be useful guides for future theoretical and empirical research in these areas. We recommend that this type of sensitivity analysis be extended to analyze the effects on labor supply parameters of using different estimation techniques--especially those appropriate for bounded dependent variables--and different data sets, especially longitudinal files, since these files should permit better control for life cycle effects.

Appendix A

PROOF OF EQUIVALENCY OF ALTERNATIVE METHODS FOR REMOVING AGE-RELATED EFFECTS FROM LABOR SUPPLY REGRESSIONS

Let the true labor supply model be

$$H_p = \alpha w_p + u \qquad (A.1)$$

where H_p is observed permanent hours worked and w_p is permanent market wage, and u is the random error term.[1]

The true wage coefficient, α, can be estimated as

$$\hat{\alpha} = \frac{\sigma_{w_p H_p}}{\sigma_{w_p w_p}} .$$

where σ_{ij} is the covariance between i and j.

Current hours and wages (H_t and w_t) each consist of a permanent component, an age-related component, and a random error term. That is,

$$H_t = H_p + \beta t + \varepsilon_H$$

$$w_t = w_p + \gamma t + \varepsilon_w$$

where t is age, ε_H and ε_w are error terms, and the age-related component is assumed to be a linear function of age.

By assumption, permanent levels of hours and wages are uncorrelated with age (t). Consider the following four alternative estimating equations:

$$H_t = b_1 w_p + c_1 t + u_1 \qquad (A.2)$$

[1]For simplicity, hours worked are assumed to be a function only of market wages; income effects are ignored, but similar proof would hold if income were included in eq. (A.1).

$$H_t = b_2 w_p + u_2 \tag{A.3}$$

$$H_t = b_3 w_t + c_3 t + u_3 \tag{A.4}$$

$$H_t = b_4 w_t + u_4 \tag{A.5}$$

where u_1, u_2, u_3, u_4 are random error terms.

The wage coefficient from eq. (A.2) can be derived as follows:

$$\begin{bmatrix} \hat{b}_1 \\ \hat{c}_1 \end{bmatrix} = \begin{bmatrix} \sigma_{w_p w_p} & \sigma_{w_p t} \\ \sigma_{w_p t} & \sigma_{tt} \end{bmatrix}^{-1} \begin{bmatrix} \sigma_{w_p H_t} \\ \sigma_{t H_t} \end{bmatrix}$$

but $\sigma_{w_p t} = 0$ by definition, so

$$\hat{b}_1 = \frac{\sigma_{tt}\, \sigma_{w_p H_t}}{\sigma_{tt}\, \sigma_{w_p w_p}} = \frac{\sigma_{w_p H_t}}{\sigma_{w_p w_p}} .$$

The covariance between permanent wages and hours at age t can be written as

$$\sigma_{w_p H_t} = w_p' \,(H_p + \beta t + \varepsilon_H)$$

$$= \sigma_{w_p H_p} + \beta \sigma_{w_p t} + \sigma_{w_p \varepsilon_H} .$$

Since $$\sigma_{w_p t} = \sigma_{w_p \varepsilon_H} = 0 ,$$

$$\hat{b}_1 = \frac{\sigma_{w_p H_p}}{\sigma_{w_p w_p}} = \hat{\alpha} .$$

Similarly, for eq. (A.3)

$$\hat{b}_2 = \frac{\sigma_{w_p H_t}}{\sigma_{w_p w_p}} = \frac{\sigma_{w_p H_p}}{\sigma_{w_p w_p}} = \hat{\alpha} \ .$$

For eq. (A.4),

$$\begin{bmatrix} \hat{b}_3 \\ \hat{c}_3 \end{bmatrix} = \begin{bmatrix} \sigma_{w_t w_t} & \sigma_{w_t t} \\ \sigma_{t w_t} & \sigma_{tt} \end{bmatrix}^{-1} \begin{bmatrix} \sigma_{w_t H_t} \\ \sigma_{t H_t} \end{bmatrix}$$

so

$$\hat{b}_3 = \frac{\sigma_{tt}\,\sigma_{w_t H_t} - \sigma_{w_t t}\,\sigma_{t H_t}}{\sigma_{w_t w_t}\,\sigma_{tt} - \sigma_{t w_t}\,\sigma_{t w_t}} \ .$$

But

$$\sigma_{w_t H_t} = (w_p + \gamma t + \epsilon_w)'(H_p + \beta t + \epsilon_H)$$

$$= \sigma_{w} + \beta\gamma\sigma_{tt} \ .$$

Similarly,

$$\sigma_{t H_t} = \beta\sigma_{tt}$$

and

$$\sigma_{w_t w_t} = \sigma_{w_p w_p} + \gamma^2\sigma_{tt} \ ,$$

so,

$$\hat{b}_3 = \frac{\sigma_{tt}\,(\sigma_{w_pH_p} + \beta\gamma\sigma_{tt}) - \beta\gamma\sigma_{tt}\sigma_{tt}}{\sigma_{tt}\,(\sigma_{w_pw_p} + \gamma^2\sigma_{tt}) - \gamma^2\sigma_{tt}\sigma_{tt}}$$

$$= \frac{\sigma_{w_pH_p}}{\sigma_{w_pw_p}} = \hat{\alpha} .$$

However, for eq. (A.5),

$$\hat{b}_4 = \frac{\sigma_{w_tH_t}}{\sigma_{w_tw_t}}$$

$$= \frac{\sigma_{w_pH_p} + \beta\gamma\sigma_{tt}}{\sigma_{w_pw_p} + \gamma^2\sigma_{tt}} \neq \hat{\alpha} .$$

Therefore, for the estimated wage coefficient to be an unbiased estimate of the true wage coefficient, either wages must be measured at their permanent level or age must be included in the regression.

Appendix B

UNCOMPENSATED AND COMPENSATED WAGE EFFECTS

In Table B-1 we present simple linear labor supply models using each of the four income variables discussed in the text (p. 27). The last two columns in the table give the uncompensated and compensated wage effects for each model.

With the first and second income measures, the uncompensated own wage effect is estimated directly and is constant if a linear model specification is used. In these models the compensated wage effect varies with the number of hours worked. If the labor supply curve is backward bending, the compensated wage effect becomes smaller as the wage rate increases; if the curve is upward sloping, the compensated effect becomes greater as the wage rate increases. The difference between models 1 and 2 is in the way one estimates the effect of a change in the wife's wage rate on her husband's labor supply. In model 1 both husband's and wife's uncompensated wage effects are estimated directly (α_1 and α_2). In model 2 it is the compensated wife's wage effect that is estimated directly (α_2'). A more commonly used form of model 2 is one in which cross-substitution effects between wife's wage and husband's labor supply are ruled out by assumption (Cohen, Rea, and Lerman, 1970, for example). When that is the case, wife's wage is not entered as a separate independent variable, and changes in wife's wage are assumed to affect husband's labor supply only through income effects. In model 3 both husband's and wife's compensated wage effects are estimated directly. In the full income approach (model 4) neither the compensated nor the uncompensated wage effect is estimated directly.[1]

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[1] Hall (1973), who used a full income variable, incorrectly interpreted α_1''' as the compensated wage effect. In effect, he adjusted the uncompensated effect by subtracting $\alpha_3'''T$, rather than $\alpha_3''' L_h$, which results in a compensated effect that is too small if $L_h < T$, too large if T is defined such that $L_h > T$.

Table B-1

UNCOMPENSATED AND COMPENSATED WAGE EFFECTS CORRESPONDING TO DIFFERENT INCOME VARIABLES

Income Variable (Y)	Model	Own (Husband's) Uncompensated Wage Effect $\frac{\partial L_h}{\partial W_h}$	Own (Husband's) Compensated Wage Effect $\frac{\partial L_h^c}{\partial W_h} = \frac{\partial L_h}{\partial W_h} - \frac{\partial L_h}{\partial Y} \cdot L_h$
1 Nonwage income	$L_h = \alpha_0 + \alpha_1 W_h + \alpha_2 W_w + \alpha_3 Y_n$	α_1	$\alpha_1 - \alpha_3 L_h$
2 Other family income	$L_h = \alpha_0' + \alpha_1' W_h + \alpha_2' W_w + \alpha_3'(W_w L_w + Y_n)$	α_1	$\alpha_1' - \alpha_3' L_h$
3 Total family income	$L_h = \alpha_0'' + \alpha_1'' W_h + \alpha_2'' W_w + \alpha_3''(W_h L_h + W_w L_w + Y_n)$	$\alpha_1'' + \alpha_3'' L_h$	α_1''
4 Family full income	$L_h = \alpha_0''' + \alpha_1''' W_h + \alpha_2''' W_w + \alpha_3'''(W_h T + W_w T + Y_n)$	$\alpha_1''' + \alpha_3''' T$	$\alpha_1''' + \alpha_3''' N_h$

Subscripts
- h = husband's
- w = wife's

Variables
- L = labor supply
- W = wage rate
- Y_n = nonwage income
- T = total time available
  = $L_i + N_i$, where N = time spent in nonmarket activities.

Appendix C

WAGE EQUATIONS[1]

Following the human capital tradition,[2] the dependent variable in our wage equations is the natural logarithm of the hourly wage rate $[\ln(E_w \div HRWRK_w)]$.[3] Three different wage measures serve as dependent variables for each sex-schooling group: (1) $LW = \ln[E_w \div HRWRK_w]$, the natural logarithm of the hourly wage; (2) $LW_L = \ln[(E_w \div HRWRK_w) \div P_L]$, the natural logarithm of the hourly wage adjusted for cost of living, using a Department of Labor cost-of-living index (P_L); and (3) $LW_I = \ln[(E_w \div HRWRK_w) \div P_I]$, the natural logarithm of the hourly wage adjusted by Watts' Iso-Prop cost-of-living index (P_I).[4]

The explanatory variables in the wage equations have been divided into two categories: (1) personal characteristics of the individual that (are hypothesized to) affect his market productivity and hence his wage, and (2) geographic attributes of his residence that characterize the labor market in which he works and, in the case of the

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[1]In specifying the wage equation we drew from yet unpublished work by T. Paul Schultz.

[2]For example, Mincer (1972).

[3]The explanatory power (R^2s) of equations explaining absolute changes in wages (i.e., equations explaining $[E_w \div HRWRK_w]$, $[(E_w \div HRWRK_w) \div P_L]$, $[(E_w \div HRWRK_w) \div P_I]$) were considerably lower than the R^2s of equations explaining the logarithms of wages.

[4]There is no ideal cost-of-living index. The theoretical and practical problems in constructing price indexes are well documented. Because of these difficulties we have tried two different indexes based on two different approaches to constructing price indexes. Both of these indexes are supposed to correct for urban/rural price level differences and differences among the four Census regions. The constructions of these two indexes are discussed in Appendix D.

By adjusting for cost of living before estimation, one can net out the cost-of-living effects of the explanatory variable and assess their effect, if any, on real wage levels. By comparing the coefficients of the explanatory variables in the equations predicting LW to those in the equations predicting LW_L and LW_I, one can decompose the effects of these variables into real and cost-of-living components.

equation explaining LW, geographic differences in the cost of living. The explanatory variables are defined in Table C-5.

Tables C-1 through C-3 present wage equations estimated for the restricted sample of men discussed on p. 44.[1] We have divided this sample into three schooling groups--those with 0-8, 9-12, and 13+ years of schooling--and have estimated separate equations for each group. F-tests of coefficient homogeneity rejected the null hypothesis that the vectors of coefficients are the same for each schooling group.[2] The wage equations for women (reported in Table C-4) are estimated for a sample composed of the working wives (those reporting wages) of the men in the restricted sample. We have included dummies for the 9-12 and 13+ groups to allow intercepts to vary for the schooling subgroups and have included separate years of schooling variables for each of the three groups to allow for slope differences by schooling group. The other coefficients did not differ significantly among education groups for women.

COEFFICIENTS OF PERSONAL CHARACTERISTICS

The signs and magnitudes of the coefficients of the personal characteristics variables are usually in accord with our *a priori* expectations. For each education group of men and for the sample of women, additional years of education increase wage rates. The years-of-schooling coefficients are significant at the 5-percent level for men but are not significant for women. In addition, for

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[1]Actually the sample used to estimate the wage equations consisted of 1933 men; 79 men were omitted by mistake--those residing in households where either the husband or wife worked part time because they were going to school or keeping house. We reestimated the wage equations including these 79 men and found that the coefficients barely differed from those reported here.

[2]Even when we pooled the schooling groups and included dummies for the 9-12 and 13+ groups to allow for intercept differences by schooling groups and included separate years of schooling variables for each group to allow for slope differences by schooling group, at the 5-percent significance level the F-test rejected the null hypothesis that the other (non-schooling) coefficients were the same for each schooling group.

Table C-1

WAGE EQUATIONS FOR MEN WITH 0-8 YEARS OF SCHOOLING

(sample size=304)

Dependent Variable / Explanatory Variables[a]	$LW=Ln[E_w \div HRWRK_w]$ Coefficient	t-Statistic	$LW_L=Ln[(E_w \div HRWRK_w) \div P_L]$ Coefficient	t-Statistic	$LW_I=Ln[(E_w \div HRWRK_w) \div P_I]$ Coefficient	t-Statistic
Personal characteristics						
Years of schooling (0-8)	.0390	(3.29)	.0388	(3.24)	.0435	(3.62)
Experience	.0416	(2.72)	.0427	(2.76)	.0321	(2.07)
$Experience^2$	-.000735	(-2.50)	-.000756	(-2.55)	-.000476	(-1.59)
Disability	-.221	(-2.82)	-.221	(-2.80)	-.209	(-2.63)
Geographic characteristics						
Small SMSA	-.137	(-1.82)	-.144	(-1.90)	-.155	(-2.03)
Medium SMSA	-.0383	(-0.56)	-.0464	(-0.68)	-.0466	(-0.67)
Poor city	-.260	(-3.27)	-.259	(-3.23)	-.253	(-3.14)
Suburb	.00402	(0.07)	.00431	(0.07)	.00490	(0.08)
Town	-.256	(-3.13)	-.202	(-2.45)	-.255	(-3.08)
Farm	-.673	(-4.55)	-.685	(-4.58)	-.745	(-4.96)
Rural	-.294	(-3.80)	-.242	(-3.10)	-.108	(-1.38)
Poor rural South	.0532	(0.63)	.173	(2.05)	.381	(4.49)
Rest of South	-.0121	(-0.18)	.0892	(1.32)	.320	(4.73)
North Central	.135	(2.08)	.158	(2.41)	.323	(4.91)
West	.111	(1.55)	.103	(1.43)	.204	(2.03)
Constant	.221		.177		.0223	
R^2	.262		.224		.295	

[a] See Table C-5 for definitions of variables.

Table C-2

WAGE EQUATIONS FOR MEN WITH 9-12 YEARS OF SCHOOLING

(sample size=1079)

Dependent Variable / Explanatory Variables[a]	$LW=Ln[E_W \div HRWRK_W]$ Coefficient	t-Statistic	$LW_L=Ln[(E_W \div HRWRK_W) \div P_L]$ Coefficient	t-Statistic	$LW_I=LN[(E_W \div HRWRK_W) \div P_I]$ Coefficient	t-Statistic
Personal characteristics						
Years of high school (1-4)	.0353	(1.66)	.0358	(1.67)	.0404	(1.87)
High school graduation	.0208	(0.43)	.0215	(0.44)	.0164	(0.33)
Experience	.0285	(4.65)	.0291	(4.70)	.0164	(2.65)
$Experience^2$	-.000614	(-3.89)	-.000625	(-3.92)	-.000244	(-1.52)
Disability	-.153	(-3.08)	-.153	(-3.06)	-.158	(-3.13)
Geographic characteristics						
Small SMSA	-.138	(-3.91)	-.139	(-3.89)	-.143	(-3.99)
Medium SMSA	-.0884	(-2.95)	-.0888	(-2.93)	-.0778	(-2.56)
Poor city	-.0959	(-1.79)	-.0928	(-1.71)	-.0782	(-1.44)
Suburb	.0507	(1.99)	.0488	(1.90)	.0464	(1.80)
Town	-.189	(-4.65)	-.128	(-3.11)	-.192	(-4.65)
Farm	-.360	(-3.54)	-.385	(-3.74)	-.389	(-3.76)
Rural	-.0912	(-2.38)	-.0237	(-0.61)	.0900	(2.32)
Poor rural South	-.126	(-2.84)	.000336	(0.01)	.212	(4.73)
Rest of South	-.0157	(-0.50)	.0991	(3.11)	.326	(10.18)
North Central	.0363	(1.33)	.0732	(2.65)	.248	(8.93)
West	.0630	(1.98)	.0752	(2.33)	.180	(5.56)
Constant	.783		.727		.604	
R^2	.160		.113		.194	

[a] See Table C-5 for definitions of variables.

Table C-3

WAGE EQUATIONS FOR MEN WITH 13 OR MORE YEARS OF SCHOOLING

(sample size=550)

Dependent Variable / Explanatory Variables[a]	$LW=Ln[E_W \div HRWRK_W]$		$LW_L=Ln[(E_W \div HRWRK_W) \div P_L]$		$LW_I=Ln[(E_W \div HRWRK_W) \div P_I]$	
	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
Personal characteristics						
Years of college	.0300	(2.01)	.0306	(2.03)	.0309	(2.03)
College graduation	.172	(2.98)	.172	(2.95)	.153	(2.62)
Experience	.0320	(4.32)	.0321	(4.29)	.0239	(3.18)
$Experience^2$	-.000758	(-2.82)	-.000753	(-2.77)	-.000442	(-1.62)
Disability	.0905	(0.91)	.0899	(0.90)	.112	(1.11)
Geographic characteristics						
Small SMSA	-.140	(-2.36)	-.137	(-2.30)	-.140	(-2.33)
Medium SMSA	-.0773	(-1.69)	-.0770	(-1.67)	-.0770	(-1.66)
Poor city	.102	(0.76)	.0992	(0.74)	.140	(1.04)
Suburb	.0297	(0.72)	.0292	(0.71)	.0184	(0.44)
Town	-.244	(-3.88)	-.173	(-2.73)	-.259	(-4.07)
Farm	-.429	(-1.89)	-.420	(-1.84)	-.438	(-1.91)
Rural	-.303	(-4.31)	-.232	(-3.27)	-.123	(-1.72)
Poor rural South	-.0657	(-0.80)	.0794	(0.96)	.278	(3.35)
Rest of South	.0286	(0.55)	.159	(3.00)	.354	(6.66)
North Central	-.00538	(-0.12)	.0502	(1.07)	.196	(4.15)
West	.0340	(0.65)	.0624	(1.19)	.132	(2.49)
Constant	1.07		1.00		.877	
R^2	.236		.203		.222	

[a]See Table C-5 for definitions of variables.

Table C-4

WAGE EQUATIONS FOR WOMEN
(Sample size-632)

Dependent Variable	$LW = Ln[E_w \div HRWRK_w]$		$LW_L = L_n[(E_w \div HRWRK_w) \div P_L]$		$LW_I = Ln[E_w \div HRWRK_w) \div P_1]$	
Explanatory Variables[a]	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
Personal characteristics						
Education						
Years 0-8	.00992	(0.35)	.00763	(0.26)	.0175	(0.60)
Dummy for 9-12 group	.0739	(0.32)	.0594	(0.25)	.126	(0.53)
Years of high school (1-4)	.0687	(1.28)	.0690	(1.29)	.0758	(1.39)
High school graduation	-.0257	(-0.21)	-.0268	(-0.22)	-.0238	(-0.19)
Dummy for 13+ group	.396	(1.72)	.385	(1.67)	.464	(1.97)
Years of college (1-8)	.0553	(1.17)	.0548	(1.15)	.0606	(1.24)
College graduation	.223	(1.51)	.223	(1.50)	.224	(1.48)
Experience	.00162	(0.25)	.00200	(0.31)	-.00699	(-1.06)
$Experience^2$	.0000225	(0.13)	.0000154	(0.09)	.000321	(1.78)
Disability	-.0578	(-0.71)	-.0601	(-0.73)	-.0589	(-0.71)
Geographic Characteristics						
Small SMSA	-.166	(-2.62)	-.165	(-2.60)	-.171	(-2.64)
Medium SMSA	-.133	(-2.57)	-.133	(-2.56)	-.140	(-2.64)
Poor city	-.162	(-1.76)	-.159	(-1.73)	-.159	(-1.70)
Suburb	-.0326	(-0.69)	-.0336	(-0.71)	-.0387	(-0.80)
Town	-.240	(-3.46)	-.165	(-2.38)	-.250	(-3.53)
Rural	-.216	(-3.11)	-.140	(-2.00)	-.0348	(-0.49)
Poor rural South	-.0743	(-0.97)	.0590	(0.77)	.265	(3.38)
Rest of South	-.0686	(-1.22)	.0572	(1.01)	.271	(4.72)
North Central	-.0717	(-1.37)	-.0208	(-0.40)	.144	(2.69)
West	.0122	(0.21)	.0341	(0.58)	.107	(1.78)
Constant	.466		.419		.215	
R^2	.226		.197		.225	

[a]See Table C-5 for definitions of variables.

Table C-5

DEFINITIONS OF VARIABLES IN TABLES C-1 - C-4

Dependent Variables

$LW = \ell n[E_w \div HRWRK_w]$	Natural logarithm of the hourly wage (defined as earnings the week preceding the SEO survey divided by the number of hours in that week).
$LW_L = \ell n[(E_w \div HRWRK_w) \div P_L]$	The natural logarithm of a cost-of-living-adjusted wage -- the hourly wage deflated by P_L, a Department of Labor Cost of Living index. (The construction of P_L is discussed in Appendix D.)
$LW_I = \ell n[(E_w \div HRWRK_w) \div P_I]$	The natural logarithm of another cost-of-living-adjusted wage. Now the hourly wage is deflated by P_I, Watts' Iso-Prop Index. (The construction of P_I is discussed in Appendix D.)

Explanatory Variables

Personal characteristics

Education

Years of schooling 0-8: For those with 0-8 years of schooling, the number of years of schooling completed. In the equation estimated for women, where the schooling groups are pooled, this variable equals 0 if the woman has completed more than 8 years of schooling.

Dummy for 9-12 group (only in female wage equation): A dummy variable that equals 1 if the woman has completed 9-12 years of schooling (and no more), 0 otherwise. The inclusion of this variable allows the intercept to be different for the 9-12 group than for the 0-8 group.

Years of high school: For those with no more than 9-12 years of schooling, the number of years of (four-year) high school they have completed. The variable = 1 if the person has completed 9 years of schooling, 2 if he has completed 10 years, 3 if he has completed 11 years, 4 if he has completed 12 years, and 0 if he has completed no more than 0-8 years or if he has completed 13 or more years of schooling.

High school graduation: For persons who have completed 9-12 years of schooling, a dummy that equals 1 if they completed the 12th year, 0 otherwise. In the equation estimated for females, where education groups are pooled, this variable = 0 for those who have completed 13 or more years of schooling.

Dummy for 13+ group (only in female wage equation): A dummy that equals 1 if the woman has completed 13 or more years of schooling, 0 otherwise. The inclusion of this variable allows the intercept to be different for the 13+ group than for the other two schooling groups.

Years of college: The number of years of college (both undergraduate and graduate studies) the person has completed (8 maximum).

College graduation: A dummy that equals 1 if the person has completed 4 years of college, 0 otherwise (since the SEO does not specify whether or not the person graduated from college, we may be assigning a 1 to some persons who finished 4 or more years but did not graduate, or vice versa).

Experience: Current age less the age at which the person is assumed to have started working, which is defined as follows:

Years of schooling completed	Age at which person is assumed to have started working[a]
0-4	10
5-7	14
8	16
9-11	18
12	20
13-15	23
16	26
17+	28

Experience^2: The square of experience. This variable is included to allow the relationship between the wage and experience to be nonlinear.

Disability: A dummy that equals 1 if the persons said that their health affects the amount or kind of work they can do, 0 otherwise.

Geographic characteristics

Small SMSA
Medium SMSA
Poor city
Suburb
Town

} See Table 6.

Farm: A dummy that equals 1 if the person resides in a farm household (defined in 1967 SEO Codebook, p. 20), 0 otherwise.

Rural
Poor rural South
Rest of South
North Central
West
} See Table 6.

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[a]These ages of school leaving were estimated by Hanoch (1969); Mincer (1972) and Schultz (forthcoming) also use Hanoch's school-leaving ages to compute their experience variables.

both men and women, wages increase substantially as one moves from the lowest education group to the highest; that is, for men the intercepts become larger as we consider more educated groups, and for women the group dummies are positive and increase with education. For both men and women the coefficient of the high school graduation dummy is never statistically significant, but the coefficient of the college graduation dummy is always significantly greater than zero.

Experience is postulated to exert a positive but diminishing effect on percentage change in wage rate (Mincer, 1972). Therefore, in addition to experience, we include experience squared to allow for this nonlinearity. In the equations estimated for men these two variables always have the expected positive and negative signs, respectively, and both are generally quite significant. Because formal schooling and on-the-job trainin are thought to be complementary, we expected experience to have a greater effect on wage increase for more educated groups than for less educated. However, Fig. C-1, which illustrates how the natural logarithm of the wage varies with experience for each of the three schooling groups of men, shows that this is not the case here. The curve is steepest for the least educated group (0-8 years of schooling). For this group the wage of persons with peak earnings is nearly 60 percent higher than that of persons just beginning to work; the comparable figures for the 9-12 and 13+ schooling groups are 33 percent and 34 percent. This unexpected pattern may be because the experience variable is picking up cohort, as well as life cycle, effects. Ideally we would like to know the relationship between wages and experience (or age) over a person's life cycle. However, because our data do not follow a single group of individuals over time, we must infer this relationship by observing people of different ages at a point in time.[1]

[1]For example, we infer what a man currently 20 years old will earn when he is 45 on the basis of what men with similar characteristics who are currently 45 earn now. But men currently 20 years old who have not gone beyond eighth grade may be less intelligent and capable on average than men who are currently 45 years old and have had no more than eight years of schooling. Eight years of schooling is much less now than eight years of schooling was 20-30 years ago.

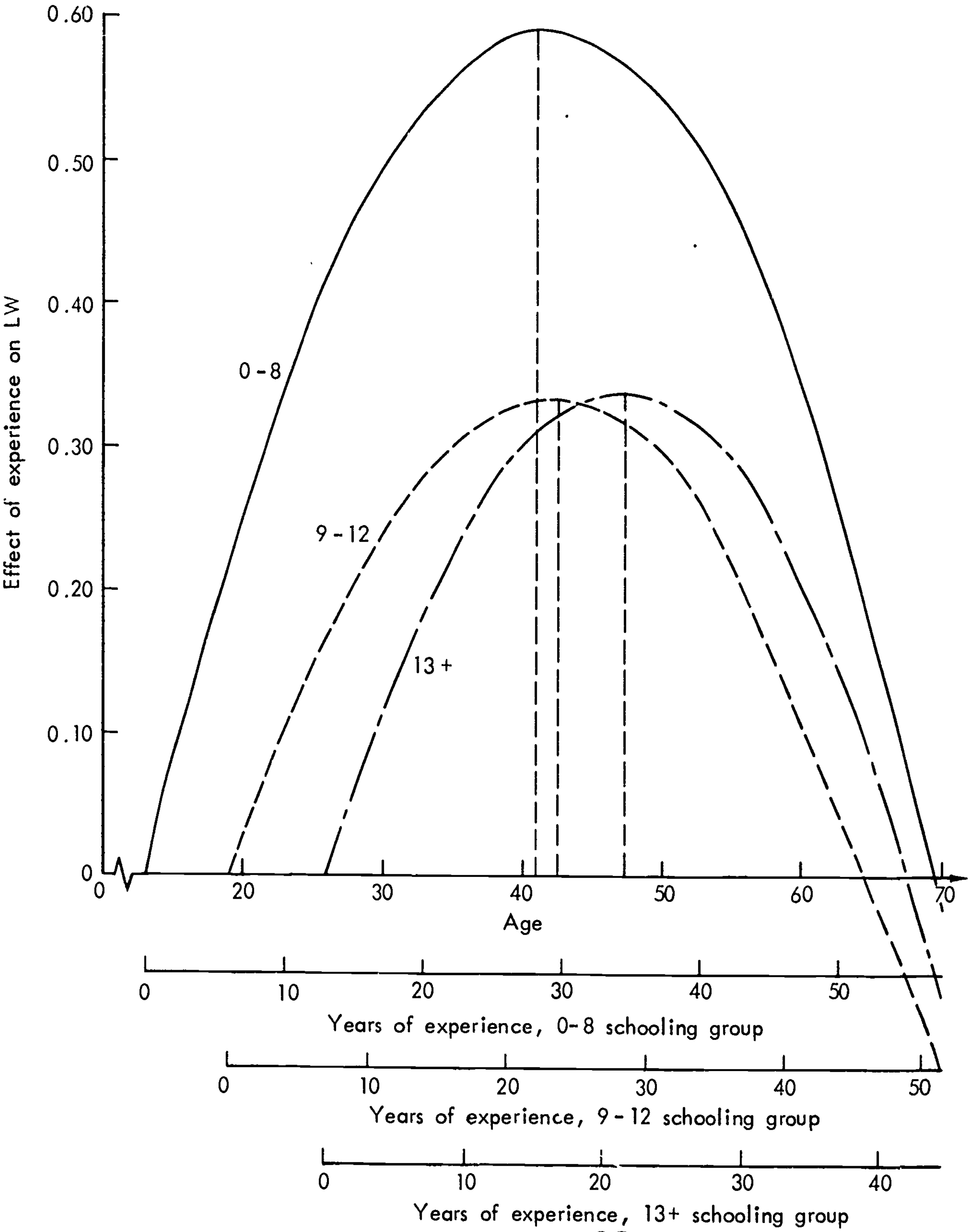

Fig. C-1 — Effect of experience on LW (= Ln [$E_W \div HRWRK_W$]) for males, by schooling group

For women, experience and experience squared often do not have the hypothesized signs, but the coefficients are usually not significantly different from zero. However, as indicated in Table C-5, experience is measured here as approximate number of years since finishing school, rather than number of years of actual work experience. For women these two may be quite different. Unfortunately, the latter is not available in the SEO data.[1]

The coefficient of the disability dummy is expected to be negative, because when the range of jobs a person can hold is restricted, the range of wage rates may be restricted also. This expectation is borne out for the 0-8 and 9-12 schooling groups of men and for women. For men, disability becomes less of a negative influence on wage rates as education increases.

COEFFICIENTS OF GEOGRAPHIC VARIABLES[2]

All of the geographic variables are dummies indicating whether or not a person lives in a certain type of area. Since the classifications are not mutually exclusive, an individual may fall into several categories simultaneously. For example, if a person lives in the North Central Region in an SMSA with population 50,000 that is classified as a poverty area, the variables Medium SMSA, Poor city, and North Central will all have a value of 1; all other geographic dummies will equal 0.

Most of the geographic variables perform about as expected in the equations explaining LW. However, the magnitudes and often the significance of the coefficients of the geographic variables are sometimes

[1]Preliminary explorations with the Parnes data, which do give actual years of work experience, showed that women's actual experience performs much better than the computed experience measure used here, that is, the coefficients of actual experience and actual experience squared had the expected signs and were statistically significant.

[2]The farm variable has been omitted from the wage equation estimated for women. There were only two farm women in the sample of 632 women. When the farm variable was included it had a coefficient of +.691. This led us to conclude that the two farm women in the sample are very high-wage women and probably not representative of farm women as a whole. We felt it would be incorrect to impute higher wages to all farm women on the basis of this result.

quite sensitive to whether or not the wage has been adjusted for cost of living. This is especially true for the coefficients of variables indicating ruralness or region of residence--farm, rural, poor rural South, rest of South, North Central, and West. This is not unexpected, for the cost-of-living indexes we use are supposed to correct for cost-of-living differences between rural and urban areas and among Census regions. For example, adjusting for cost-of-living decreases the negative effect on wages of living in a rural area. However, in some cases adjusting for cost of living appears to "overcorrect." For instance, for the 0-8 and 9-12 groups the coefficient of the "rest of South" variable in the equation explaining LW is around -.01, indicating that, other things the same, nominal wages for persons living in the part of the South that is not "poor rural" tend to be 1 percent lower than the wages of the group for which all of the geographic dummies equal 0;[1] but when adjusted by cost-of-living index P_L, wages in the "rest of South" are about 9 percent higher than in the area for which all geographic dummies equal 0 for the 0-12 schooling group (15 percent higher for the 13+ group), and when adjusted by P_I, real wages are 32 percent higher (35 percent for the 13+ group) in the rest of the South than in the areas for which all dummies equal 0. This last difference seems exceptionally large and may simply indicate the unreliability of this index.

[1]Non-poverty areas in central cities in SMSAs with population greater than 750,000 in the North East Census region.

Appendix D

CONSTRUCTION OF THE COST OF LIVING INDEXES USED TO DEFLATE WAGE RATES

DEPARTMENT OF LABOR COST-OF-LIVING INDEX (P_L)

The Department of Labor (1969) has published 1967 cost-of-living indexes for 24 regional and income groups: all combinations of the four Census regions, metropolitan and non-metropolitan areas, and three family income groups (annual family income less than $7000, between $7000 and $11,000, and over $11,000). Census region of residence is denoted in the SEO data. We have defined "metropolitan" areas to be SMSAs and "non-metropolitan" areas to be non-SMSAs. Because the published indexes are based on family income for a family of four persons, we have adjusted family income for family size by deflating by Watts' family size index (described below) to determine which of the three family income groups the family belongs in.[1] For households that did not report their head's wage, family income was set equal to the household's total earnings plus other income. For the remaining households, the head's reported annual earnings were replaced by annual earnings calculated as the product of his hourly wage ($E_w \div HRWRK_w$) and his annual hours ($HRWRK_w \cdot WKWRK_y$). This was done so that the family income used to determine the appropriate cost-of-living index would be consistent with the wage rate the index is used to deflate ($E_w \div HRWRK_w$).

WATTS' ISO-PROP COST-OF-LIVING INDEX (P_I)

Harold Watts (1967) has proposed a cost-of-living index derived from Engel curves. The basic premise underlying his approach is that families that spend, on average, an equal fraction of their income on "necessities" (food, housing, clothing, and transportation) are

[1]We have normalized the family size index to equal 1.00 for a family of four.

equally poor. The index is derived from the coefficients of a double-log linear regression of expenditures for necessities on the following variables: income before taxes, family size, dummies for the Census regions, an urban-rural dummy, age of household head, and a homeowner dummy.

Grouped data from the 1960 Survey of Consumer Expenditures were used. The regression coefficients (Watts, 1967, Table 1, line 1, p. 11) yield separate indexes for regions, for urban and rural areas, and for family size. (Age and the homeowner dummy were included in the regression only as controls for these factors; their coefficients are not used in constructing the index.) For each household the appropriate indexes for region, urban and rural, and family size are multiplied together to create the Iso-Prop index for that household. The indexes are then normalized so that their geometric average is one.

Limitations of this approach, at present, are: (1) the regression form used implies constant elasticities of expenditure with respect to income, family size, and age,[1] and (2) the effects on cost-of-living of region, rural and urban, and family size are assumed to be independent of one another. For example, rural cost of living is 83 percent of urban regardless of the region of residence and regardless of family size.

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[1]Watts is aware of this problem. He tests the accuracy of this assumption regarding constant income elasticity and rejects the hypothesis that the income elasticity is constant across all income groups.

Appendix E
NET WORTH EQUATIONS

The net worth imputing equations and age, children, and home status adjusting factors were derived in the following manner: First, each household in the base sample (2012) was assigned to one of six sub-groups based on the experience-adjusted imputed wage of the husband. These groups were: <$2.00, 2.00-2.24, 2.25-2.49, 2.50-2.74, 2.75-2.99, and ≥3.00. Within each of these groups it was assumed that the only factors that systematically affected observed net worth were geographic factors, education, home status, number of children, and the age of the head of the household. In other words, dividing the sample by the "exogenous" experience-adjusted wage was assumed to have the effect of removing the intra-group variation in wealth (and net worth) attributable to all other factors except those used in the imputing regressions.

Table E-1 presents summary statistics for selected variables for each of these six subsamples. Although the ranges are quite narrow, each wage group contains approximately the same number of households, because both imputation and the experience adjusting process considerably narrow the range of the wage variable.[1]

The summary statistics indicate that in general these samples behave as one might expect. All variables related to wealth or productivity (actual wage, husband's and wife's education, and actual net worth) rise monotonically as the experience-adjusted imputed wage rises. The mean age and the mean values of the six age dummies indicate that although there is some indication of cohort effects (the less than $2.00 group is slightly older on the average than the other groups), in general differences in age distributions between the groups are not large.

[1]The extent of this "narrowing" is well illustrated in Table E-1 and by the fact that although only 411 individuals had an experience-adjusted imputed wage greater than $3.00, 1308 individuals had an actual reported wage greater than $3.25.

Table E-1

SUMMARY STATISTICS FOR SELECTED VARIABLES USED IN NET WORTH EQUATIONS

Variable	Experience-Adjusted Imputed Wage					
	<2.00	2.00-2.24	2.25-2.49	2.50-2.74	2.75-2.99	>3.00
Sample Size	387	261	343	333	175	411
Age dummy means						
25-29	0.140	0.261	0.201	0.210	0.206	0.190
30-34	0.180	0.253	0.222	0.246	0.240	0.263
35-39	0.212	0.161	0.201	0.207	0.234	0.219
4[illegible]	0.191	0.161	0.149	0.165	0.148	0.153
45-49	0.129	0.111	0.157	0.114	0.108	0.102
50-54	0.147	0.054	0.070	0.057	0.063	0.073
Mean Age	39.38	35.79	37.26	36.27	36.41	36.55
Age-adjusted imputed wage						
Mean	1.56	2.13	2.38	2.6[illegible]	2.86	3.66
Standard deviation	0.282	0.066	0.066	0.076	0.055	0.463
Maximum	1.99	2.24	2.49	2.74	2.99	4.79
Minimum	0.742	2.01	2.26	2.50	2.75	3.01
Actual wage						
Mean	2.64	2.97	3.34	3.56	4.03	4.87
Standard deviation	0.99	1.07	1.12	1.02	[illegible]	2.03
Maximum	6.87	8.33	13.00	10.37	[illegible]	17.50
Minimum	0.40	0.96	0.72	1.2[illegible]	1.50	1.23
Male education						
Mean	7.76	11.02	11.54	11.85	13.13	15.97
Standard deviation	2.31	1.32	1.21	1.05	1.61	1.83
Maximum	16.0	15.0	15.0	16.0	19.0	20.0
Minimum	0	9.0	9.0	9.0	12.0	13.0
Female education						
Mean	9.40	11.15	11.34	11.69	12.01	13.45
Standard deviation	2.70	1.88	1.82	1.60	1.66	2.20
Maximum	17.0	18.0	16.0	18.0	17.0	19.0
Minimum	0	2.0	3.0	3.0	5.0	4.0
Net worth (actual)						
Mean	5966.0	7636.2	9014.7	9695.1	10031.0	16716.0
Standard deviation	7735.0	7564.6	8841.6	9229.5	9273.8	19160.0
Maximum	57600.0	32700.0	38950.0	47600.0	42500.0	125550.0
Minimum	-2912.0	-9425.0	-9050.0	-3112.0	-13500.0	-1050.0

The results of this regression analysis of net worth are given in Tables E-2 and E-3. Table E-2 presents results for the full set of "independent" variables--that is, 10 regional variables, education of husband and wife, home status, number of children (entered as the square root of actual number of children to allow for economies of scale), and five of the six age dummies (the omitted group being ages 25-29). In the results presented in Table E-3, home status and children were excluded from the net worth equation.

One problem encountered in estimating these regressions was that although the overall sample size for each group was reasonably large (the smallest being 175) certain of the dummy categories had very few people in them. When the number of people in a given category falls below 15 or 20, a single observation can and often does dominate the size and sign of the estimated coefficient. In essence, including these explanatory variables would sometimes have meant assigning to all those in a given category the characteristics of one person. To avoid this, variables were dropped as the cell size became so small that coefficients were highly sensitive to the values of individual observations.

A similar problem arose with the six age group dummies; however, in this case, small cells could not be excluded. To avoid contaminating the estimated net worth for all individuals in an age group that contained a small number of observations and a significant outlier, those families with actual assets more than three standard deviations from the mean for each wage group were excluded from the sample.[1]

The imputation process itself is straightforward. Each family in the sample is assigned six imputed net worth values, four based on the regressions presented in Table E-2 and two on the regressions

[1]This resulted in the exclusion from the sample of one low (-$129,000) and 36 high net worth families. Means and standard deviations were calculated for each group from the full sample; the means and standard deviations given in the table are, of course, calculated excluding the rejected outliers. The range of actual net worth values is still very large even given this restriction.

Table E-2

NET WORTH REGRESSIONS FIRST SPECIFICATIONS

Independent Variables[b]	Imputed Wage Groups[a]											
	<2.00		2.00-2.24		2.25-2.49		2.50-2.74		2.75-2.99		>3.00	
	Coeff.	(t)	Coeff.	(t)	Coeff.	(t)	Coeff.	(t)	Coeff.	(t)	Coeff	(t)
Town	-2442	(-1.87)	-5447	(-2.54)	434	(0.193)	c		c		-3575	(-1 28)
Rural	-1994	(-1.48)	-5978	(-2.89)	-1116	-0.684)	1409	(0.462)	-2295	(-0.599)	c	
Suburb	904	(0.861)	-1356	(-1.00)	2394	(2.21)	2506	(2.40)	1430	(0.671)	345	(0 213)
Poor City	-1101	(-0.812)	-3588	(-1.90)	c		c		c		c	
Poor Rural South	- 509	(-0.389)	956	(0.56)	c		-7943	(-1.80)	c		c	
Rest of South	- 561	(-0.497)	- 900	(-0.74)	-2261	(-1.88)	- 248	(-0.190)	-1714	(-0.591)	-5436	(-2.49)
North Central	- 371	(-0.336)	- 61.8	(-0.054)	33.4	(0.032)	2058	(1.46)	-2615	(-1.04)	-1586	(-0 821)
West	1401	(1.09)	c		-2374	(-1.48)	1190	(0.729)	-1248	(-0.475)	-1903	(-0.926)
Small SMSA	331	(0.266)	-3279	(-1.75)	-1862	(-1.06)	-6408	(-2.26)	-2130	(-0.704)	-3658	(-1 34)
Medium SMSA	-2669	(-2.28)	-4811	(-2.77)	- 829	(-0.582)	-2992	(-1.68)	-1481	(-0.730)	-3663	(-1.88)
Home status (1 = rent)	-6630	(-8.82)	-6612	(-7.10)	-7625	(-7.34)	-7747	(-7.73)	-7325	(-4.50)	-9445	(-5.22)
Husband's education	36	(0.212)	578	(1.35)	671	(1.28)	1481	(1.79)	570	(0.507)	- 61	(-0.140)
Wife's education	359	(2.57)	589	(2.71)	503	(2.16)	678	(2.37)	391	(0.982)	1118	(3 26)
Children (square root)	-1524	(-2.91)	-1642	(-2 71)	-2317	(-3.48)	-1997	(-2.64)	- 386	(-0 387)	-1292	(-1 10)
Age 30-34	765	(0.667)	2731	(2.52)	3283	(2.56)	1512	(1.20)	493	(0.266)	1843	(0.865)
Age 35-39	2506	(2.21)	4023	(3.22)	3660	(2.75)	4462	(3.32)	5076	(2.57)	5396	(2 37)
Age 40-44	4006	(3 43)	4923	(3.92)	6465	(4.50)	6364	(4.48)	5612	(2.68)	9843	(3.96)
Age 45-49	2956	(2.35)	7301	(5.24)	6473	(4.59)	10457	(6 66)	13836	(6.00)	14325	(5 13)
Age 50-54	6299	(5.10)	9828	(5.36)	10186	(5.49)	8480	(4.28)	7135	(2.54)	10471	(3 39)
Intercept	5650.6		389.43		-3004.6		-16227.9		-2648 6		1951.7	
R^2	0.37		0.41		0.35		0.37		0.39		0.23	
Sample size	387		261		343		333		175		411	

[a] Sample divided by the experience-adjusted imputed wage.

[b] See Table 6 for definitions of variables.

[c] Variable dropped from equation because of small cell size (see text)

Table E-3

NET WORTH REGRESSIONS: SECOND SPECIFICATION

Independent Variables[b]	Imputed Wage Groups[a] <2.00 Coeff.	<2.00 (t)	2.00-2.24 Coeff.	2.00-2.24 (t)	2.25-2.49 Coeff.	2.25-2.49 (t)	2.50-2.74 Coeff.	2.50-2.74 (t)	2.75-2.99 Coeff.	2.75-2.99 (t)	>3.00 Coeff.	>3.00 (t)
Town	-1875	(-1.30)	-3291	(-1.40)	2524	(1.04)	c		c		-2178	(-0.76)
Rural	- 341	(-0.23)	-3158	(-1.40)	1810	(1.07)	550.	(0.17)	-1641	(-0.41)	c	
Suburb	1785	(1.54)	- 401	(-0.27)	4213	(3.69)	3130	(2.77)	2021	(0.90)	1562	(0.95)
Poor city	-1450	(-0.97)	-4367	(-2.09)	c		c		c		c	
Poor rural South	656	(0.46)	- 63.9	(-0.034)	c		-7327	(-1.53)	c		c	
Rest of South	361	(0.29)	- 90.9	(-0.069)	-1713	(-1.32)	614	(0.43)	-3767	(-1.24)	-4806	(-2.14)
North Central	305	(0.25)	- 173	(-0.14)	119	(0.10)	2805	(1.83)	-4250	(-1.63)	-1827	(-0.92)
West	2697	(1.92)	c		-1470	(-0.85)	428	(0.24)	-3154	(-1.15)	-2036	(-0.96)
Small SMSA	1172	(0.86)	-2257	(-1.09)	- 572	(-0.31)	-6336	(-2.06)	- 470	(-0.15)	-4607	(-1.64)
Medium SMSA	-1760	(-1.36)	-3471	(-1.82)	409	(0.27)	-2321	(-1.20)	15.7	(0.0075)	-3068	(-1.53)
Home status (1 = rent)												
Husband's education	67.5	(0.36)	909	(1.94)	580	(1.02)	1910	(2.12)	9.31	(0.0079)	-1.28	(-0.0029)
Wife's education	589	(3.87)	695	(2.90)	791	(3.19)	1004	(3.27)	202	(0.48)	1203	(3.42)
Children (square root)												
Age 30-34	327	(0.26)	3842	(3.26)	3948	(2.92)	958	(0.70)	2580	(1.36)	2732	(1.26)
Age 35-39	3688	(2.98)	5526	(4.10)	4544	(3.26)	5458	(3.84)	8331	(4.34)	7280	(3.26)
Age 40-44	5407	(4.22)	6540	(4.85)	8172	(5.41)	7466	(4.89)	8467	(4.01)	12580	(5.10)
Age 45-49	3602	(2.60)	8420	(5.55)	8728	(5.85)	11984	(7.09)	16884	(7.25)	16690	(5.99)
Age 50-54	8159	(6.05)	12454	(6.25)	13806	(7.10)	9564	(4.44)	9040	(3.11)	13349	(4.26)
Intercept	-3941		-11418		-13876		-31180		3233		-6439	
R^2	0.23		0.27		0.23		0.25		0.30		0.18	
Sample size	387		261		343		333		175		411	

[a]Sample divided by the experience-adjusted imputed wage.

[b]See Table 6 for definitions of variables.

[c]Variable dropped from equation because of small cell size (see text).

presented in Table E-3. The adjustment process consists of setting the relevant variable equal to 0 for each family in the sample regardless of that variable's actual value. For example, to age-adjust imputed net worth, all five age dummies are set equal to 0. This is equivalent to subtracting from the initial imputed net worth value that proportion estimated to be due to age alone. A similar procedure was used to adjust the age-adjusted imputed asset figures for numbers of children and then for differences between renters and nonrenters. Summary statistics on the results of these imputations and adjustments are given in Table E-4.

Table E-4

SUMMARY STATISTICS: IMPUTED NET WORTH

Variable[a]	Mean	Standard Deviation	Maximum	Minimum
NW	10,788	19,003	308,000	-129,470
INW	9,477[b]	6,597	35,890	-5,754
INW_A	5,445	4,718	21,565	-7,799
$INW_{A,C}$	7,759	4,605	21,565	-4,750
$INW_{A,C,R}$	9,915	2,927	21,565	-950
INW2	9,619	5,715	33,444	-3,987
$INW2_A$	4,476	3,204	16,754	-7,151

[a]Variable definitions are:
NW: Net worth
INW: Imputed net worth
Subscript A: Age adjusted
Subscript C: Children adjusted
Subscript R: Adjusted for renters
INW2: Imputed net worth based on second form of imputing equation (Table E-3).

[b]Actual and imputed means differ because they are based on different samples; actual measure includes the outliers that were excluded from the imputing samples.

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