

Attrition Bias in Experimental and Panel Data: The Gary Income Maintenance Experiment



Jerry A. Hausman; David A. Wise

Econometrica, Vol. 47, No. 2 (Mar., 1979), 455-474.

Stable URL:

<http://links.jstor.org/sici?sici=0012-9682%28197903%2947%3A2%3C455%3AABIEAP%3E2.0.CO%3B2-T>

Econometrica is currently published by The Econometric Society.

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/econosoc.html>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is an independent not-for-profit organization dedicated to creating and preserving a digital archive of scholarly journals. For more information regarding JSTOR, please contact jstor-info@umich.edu.

ATTRITION BIAS IN EXPERIMENTAL AND PANEL DATA: THE GARY INCOME MAINTENANCE EXPERIMENT¹

BY JERRY A. HAUSMAN AND DAVID A. WISE

CAREFUL ATTENTION TO SAMPLE DESIGN is an important consideration in both social experimentation and in panel surveys of individuals. Techniques of randomization and response surface design have been highly developed with the aim of obtaining the maximum amount of information from a given experiment or survey. In practice, however, social experimentation and panel data differ in one important respect from classical design assumptions as exemplified in the pioneering analysis of R. A. Fisher [4]. This difference arises from the fact that each individual in panel data is his own best control. In a classical experiment, seed might be planted in different plots at random and fertilized at different intensity levels chosen at random. Differences in yield would then be used to assess the effectiveness of the fertilizer. A characteristic of recent² social experiments is that individuals are surveyed before the experiment begins, and their pre-experimental behavior is then compared to their behavior after receipt of the experimental "treatment." Information on controls, persons who receive no experimental treatment, is also obtained. However, it has been found that much more information is gained from the change in a given individual's behavior than by comparing differences in the average behavior of experimentals and controls. The reason for this finding is the presence of significant, unobserved individual effects. For instance, in a previous study of the earnings response of white males in the New Jersey negative income tax experiment (Hausman and Wise [10]) the authors found that about 85 per cent of the total variance in response was due to the variation in individual specific terms that persisted over time.

It is because of the importance of individual effects that the design of social experiments includes pre-experimental observations of individuals, and corresponding data collection, and then the observation of the same individuals subject to experimental treatment over an extended period of time (ranging from two to fifteen years). But the inclusion of the time factor in the experiment raises a problem which does not exist in classical experiments—attrition. Some individuals decide that keeping the detailed records that the experiments require is not worth the payment, some move, some are inducted into the military. In some experiments, persons with large earnings receive no experimental treatment benefit and thus drop out of the experiment altogether. This attrition may negate

¹ The research reported herein was performed pursuant to Contract Number HEW 100-76-0073 from the Department of Health, Education, and Welfare, Washington, D.C. The opinions and conclusions expressed herein are solely those of the authors and should not be construed as representing the opinions or policy of any agency of the United States government.

This study was part of continuing analysis of the Gary Experiment at MATHEMATICA POLICY RESEARCH. The authors also acknowledge research support of the National Science Foundation. Research assistance was provided by G. Burtless. We thank K. Kehrer, C. Mallar, and Zvi Griliches for their comments. An editor also provided helpful comments on an earlier draft of the paper.

² For example, the New Jersey, Gary, and Seattle-Denver income maintenance experiments and the health insurance experiment currently in progress, all sponsored by HEW.

the randomization in the initial experimental design. If the probability of attrition is correlated with experimental response, then traditional statistical techniques will lead to biased and inconsistent estimates of the experimental effect.

Attrition is a problem in any panel survey, not only those conducted in conjunction with social experiments, where individuals are followed over time. Two important bodies of panel data, the Michigan Income Dynamics Survey and the National Longitudinal (Parnes) Surveys, for example, followed people for 5 and 10 years, respectively. While the attrition in these surveys has typically not been as severe as in social experiments, the same problems of potential bias arises, if attrition is not random.

In this paper we propose a method that uses a probability model of attrition, in conjunction with a traditional random effects model of individual response, to correct for attrition bias. The maximum likelihood procedure used provides consistent and asymptotically efficient estimates of the parameters of a structural model, including experimental response; and allows a test of whether or not non-random attrition has occurred. These procedures are closely related to previous models based on non-random samples by Hanoch [7], Hausman and Spence [9], Hausman and Wise [11], Heckman [14], Madalla and Nelson [17], and Nelson [20]. All of these models except Hausman and Wise considered the problem of non-random samples in the single period context. We consider the problem in a multi-period framework, due to its importance in both panel data and social experimentation. A modified scoring algorithm, first employed by Berndt, Hall, Hall, and Hausman [3], provides estimates at a reasonably small computation cost.

After formal discussion of the problem and statistical specification of our model, the method is used to estimate the earnings response of black males in the Gary Income Maintenance Experiment. Attrition bias is a potentially important problem in this experiment, but the extent of the bias seems to depend crucially on the specification of the model used to evaluate the experimental effect. Empirical results indicate a much greater bias with simple analysis of variance models than with behavioral specifications incorporating more exogenous variables. Attrition bias in a structural model estimating only a single experimental effect was found to be small although statistically significant. No attrition bias was found in a structural specification that allowed estimation of the effects of all four treatments. Simple analysis of variance estimates, however, were substantially affected by attrition.

1. STATISTICAL SPECIFICATION

Two statistical models are commonly used to analyze individual behavior over time. In this paper we will use the random effects specification, although the techniques can be applied in a straightforward manner to the fixed effects specification as well. Initially, we will concentrate on a two-period model. Later we will indicate the appropriate extension for more periods. The "linear regres-

sion" model used for individual behavior has the form

$$(1.1) \quad y_{it} = X_{it}\beta + \varepsilon_{it} \quad (i = 1, \dots, N; t = 1, 2),$$

where i indexes individuals and t indexes time periods. In a social experiment X_{i1} may differ from X_{i2} because of experimental treatment, along with changes in individual characteristics which occur with the passage of time. Such changes, of course, may also occur in panel survey data. The residual in the specification is then decomposed into two orthogonal components, an individual effect μ_i , which is assumed to be drawn from an iid distribution and to be independent of the X_{it} 's, and a time effect, η_{it} , which is assumed to be a serially uncorrelated random variable drawn from an iid distribution. Thus, the assumptions on ε_{it} are:

$$(1.2) \quad \begin{aligned} \varepsilon_{it} &= \mu_i + \eta_{it}, & E(\varepsilon_{it}) &= 0, & V(\varepsilon_{it}) &= \sigma_\mu^2 + \sigma_\eta^2 = \sigma^2, \\ \varepsilon_{it} &\sim N(0, \sigma^2). \end{aligned}$$

The contribution to the variance of the individual component σ_μ^2 is typically greater than σ_η^2 which highlights the importance of letting individuals serve as their own controls. The correlation between ε_{i1} and ε_{i2} , $\rho_{i2} = \sigma_\mu^2 / (\sigma_\mu^2 + \sigma_\eta^2)$, often ranges from .4 to .9. The correlation coefficient indicates the proportion of total variance explained by the unobserved individual effect.

If attrition occurs in the sample, a common practice is to discard those observations for which y_{i2} is missing. But suppose that the probability of observing y_{i2} varies with its value, as well as the values of other variables. Then the probability of observing y_{i2} will depend on ε_{i2} and least squares will lead to biased estimates of the underlying structural parameters and the experimental response.

To develop a model of attrition, define the indicator variable a_i and let $a_i = 0$ if attrition occurs in period two, so that y_{i2} is not observed, and let $a_i = 1$ if attrition does not occur, so that y_{i2} is observed. Suppose that y_{i2} is observed if $A_i = \alpha y_{i2} + X_{i2}\theta + W_i\gamma + \omega_i \geq 0$, where W_i is a vector of variables which do not enter the conditional expectation of y but affect the probability of observing y_{i2} , θ and γ are vectors of parameters, and the ω_i are iid random variables. Substituting for y_{i2} leads to $A_i = X_{i2}(\alpha\beta + \theta) + W_i\gamma + \alpha\varepsilon_{i2} + \omega_i$. But since α and θ enter the specification in an equivalent manner, we combine them to form a "reduced form" specification which is $A_i = X_i\xi + W_i\gamma + \varepsilon_{i3}$. Define the vectors $R_i = [W_i, X_{i2}]$ and $\delta = [\xi, \gamma]'$. We assume that ε_{i2} and ω_i are normally distributed, and normalize by setting the variance σ_{33} of ε_{i3} equal to 1. Then the probabilities of retention and attrition are probit functions given, respectively, by

$$(1.3) \quad \begin{aligned} \text{pr}(a_i = 1) &= \Phi[R_i\delta] & \text{and} \\ \text{pr}(a_i = 0) &= 1 - \Phi[R_i\delta], \end{aligned}$$

where $\Phi[\cdot]$ is the unit normal distribution function.³ We could estimate the parameters of equation (1.3) as it is. However, our primary goal is to correct for the effects of attrition on estimates of the parameters in equation (1.1) by

³ The specification of A_i and the normalization described in this paragraph were used by Hausman and Spence [9] in modeling non-random missing data. A comparable formulation, using an alternative normalization and specification for A_i , was suggested by Hausman and Wise [11, fn. 8, 9, and 10].

integrating it with the probability of attrition.

Suppose we estimate the model of equation (1.1) using only complete observations. The conditional expectation of y_{i2} , given that it is observed, is,

$$(1.4) \quad E(y_{i2}|X_{i2}, a_i = 1) = X_{i2}\beta + \rho_{23}\sigma \frac{\phi(R_i\delta)}{\Phi[R_i\delta]},$$

where ρ_{23} is the correlation between ε_2 and ε_3 . Thus, this procedure will lead to biased and inconsistent estimates of β unless $\rho_{23} = 0$.⁴ Least squares estimates based on complete observations but using first period data only will also be inconsistent, even though attrition occurs only in the second period, if ε_{i1} and ε_{i2} have a common component. For then ε_{i1} and ε_{i3} will also be correlated. The expected value of y_{i1} , given that individual i is in the sample in the second period is given by

$$(1.5) \quad E(y_{i1}|X_{i1}, a_i = 1) = X_{i1}\beta + \rho_{12}\rho_{23}\sigma \frac{\phi(R_i\delta)}{\Phi[R_i\delta]},$$

where $\rho_{13} = \rho_{12}\rho_{23}$.

The second term in equation (1.5) is smaller than the second term in the conditional expectation of y_{i2} in equation (1.4). But so long as individual effects exist across periods, attrition in one period will affect the estimates of all earlier periods, if only complete observations are used.

To recapitulate, we gather together the following definitions:

$$(1.6) \quad \begin{aligned} y_{i1} &= X_{i1}\beta + \varepsilon_{i1}, \\ y_{i2} &= X_{i2}\beta + \varepsilon_{i2}, \\ A_i &= R_i\delta + \varepsilon_{i3}. \end{aligned}$$

Attrition occurs if the index $A_i \leq 0$. From the conditional expectations of equations (1.4) and (1.5), we see that the critical parameter in the determination of attrition bias is the correlation ρ_{23} between ε_{i2} and ε_{i3} . We want a method of estimation that will yield asymptotically efficient and consistent estimates of the structural parameters of (1.6) and will allow a convenient test of the hypothesis that $\rho_{23} = 0$. We shall use a maximum likelihood procedure. The joint normal terms, ε_{i1} , ε_{i2} , and ε_{i3} have mean zero and covariance matrix

$$(1.7) \quad \Sigma = \begin{bmatrix} \sigma^2 & \rho_{12}\sigma^2 & \rho_{12}\rho_{23}\sigma \\ & \sigma^2 & \rho_{23}\sigma \\ & & 1 \end{bmatrix},$$

where $\sigma_{11} = \sigma_{22} = \sigma^2$ and we have normalized by setting $\sigma_{33} = 1$. We need to consider two possibilities: $a_i = 1$ and $a_i = 0$. If $a_i = 1$, both y_{i1} and y_{i2} are observed

⁴ A variance components estimator will also be inconsistent.

and the joint density of a_i , y_{i1} , and y_{i2} is given by

$$(1.8) \quad \begin{aligned} f(a_i = 1, y_{i1}, y_{i2}) &= \text{pr}[a_i = 1|y_{i1}, y_{i2}]f(y_{i2}|y_{i1})f(y_{i1}) \\ &= \Phi\left[\frac{R_i\delta + (\rho_{23}/\sigma)(y_{i2} - X_{i2}\beta)}{(1 - \rho_{23}^2)^{\frac{1}{2}}}\right] \frac{1}{(\sigma^2(1 - \rho_{12}^2))^{\frac{1}{2}}} \\ &\quad \cdot \phi\left(\frac{y_{i2} - \rho_{12}Y_{i1} - (X_{i2} - \rho_{12}X_{i1})\beta}{(\sigma^2(1 - \rho_{12}^2))^{\frac{1}{2}}}\right) \frac{1}{\sigma} \phi\left(\frac{y_{i1} - X_{i1}\beta}{\sigma}\right), \end{aligned}$$

where the first term follows from the fact that the conditional density $f(\varepsilon_{i3}|\varepsilon_{i2})$ is $N((\rho_{23}/\sigma)\varepsilon_{i2}, 1 - \rho_{23}^2)$. If $a_i = 0$, y_{i2} is not observed and must be "integrated out." In this instance the fact that $f(\varepsilon_{i3}|\varepsilon_{i1})$ is $N(\rho_{12}\rho_{23}/\sigma\varepsilon_{i1}, 1 - \rho_{12}^2\rho_{23}^2)$ leads to the expression,

$$(1.9) \quad \begin{aligned} f(a_i = 0, y_{i1}) &= \text{pr}[a_i = 0|y_{i1}]f(y_{i1}) \\ &= \left\{ 1 - \Phi\left[\frac{R_i\delta + (\rho_{12}\rho_{23}/\sigma)(y_{i1} - X_{i1}\beta)}{(1 - \rho_{12}^2\rho_{23}^2)^{\frac{1}{2}}}\right] \right\} \frac{1}{\sigma} \phi\left(\frac{y_{i1} - X_{i1}\beta}{\sigma}\right). \end{aligned}$$

⁵ An alternative formulation, suggested previously by Hausman and Wise [11], is to let

$$A_i = \alpha y_{i2} + R_i\delta + \omega_i,$$

where $R_i\delta = X_{i2}\theta + W_i\gamma$ and the ω_i are iid normal random variables assumed to be independent of ε_1 and ε_2 . If we normalize by setting the variance of ω equal to 1, the covariance matrix of ε_1 , ε_2 , and ω is given by

$$\Sigma = \begin{bmatrix} \sigma^2 & \rho_{12}\sigma^2 & 0 \\ & \sigma^2 & 0 \\ & & 1 \end{bmatrix}.$$

If we now substitute for y_{i2} in the expression for A_i , we obtain

$$A_i = X_{i2}(\alpha\beta + \theta) + W_i\gamma + \alpha\varepsilon_{i2} + \omega_i = R_i\delta + \varepsilon_{i3},$$

with the covariance matrix for ε_1 , ε_2 , and ε_3 given by

$$\Sigma = \begin{bmatrix} \sigma^2 & \rho_{12}\sigma^2 & \rho_{12}\alpha\sigma^2 \\ & \sigma^2 & \alpha\sigma^2 \\ & & \alpha^2\sigma^2 + 1 \end{bmatrix}.$$

Expressions comparable to equations (1.8) and (1.9) are then given by

$$f(a_i = 1, y_{i1}, y_{i2}) = \Phi[R_i\delta + \alpha(y_{i2} - X_{i2}\beta)] \cdot f(y_{i2}|y_{i1}) \cdot f(y_{i1})$$

and

$$f(a_i = 0, y_{i1}) = \left\{ 1 - \Phi\left[\frac{R_i\delta + \alpha\rho_{12}(y_{i1} - X_{i1}\beta)}{(1 + \alpha^2\sigma^2(1 - \rho_{12}^2))^{\frac{1}{2}}}\right] \right\} \cdot f(y_{i1}),$$

where explicit expressions for $f(y_{i2}|y_{i1})$ and $f(y_{i1})$ are the same as in (1.8) and (1.9).

In this formulation, attrition bias depends on the value of α and is zero only if α equals zero. A test for attrition bias is, of course, straightforward. To see the relationship between α and ρ_{23} in the specification used in the body of the paper, note that $\varepsilon_{i3} = \alpha\varepsilon_{i2} + \omega_i$, where ω and ε_2 are independent, can also be written as $\varepsilon_{i3} = \rho_{23}(\sigma_3/\sigma)\varepsilon_{i2} + \omega_i$, where $\alpha = \rho_{23}(\sigma_3/\sigma)$. Thus, $\alpha = 0$ if and only if $\rho_{23} = 0$. Normalizing by setting $\sigma_3^2 = 1$, instead of $\sigma_\omega^2 = 1$, would make the two specifications the same. In this specification, however, we have explicitly assumed that ω is independent from ε_1 and ε_2 . But since we have not in the text specification attempted to identify the covariance between ω and ε_2 , the two specifications are equivalent. We have not tried to distinguish correlation between ε_2 and ε_3 due only to the fact that ε_2 shows up in ε_3 from correlation between ω and ε_2 .

The log likelihood function follows from equations (1.8) and (1.9). Order the observations so that the first s correspond to $a_i = 1$ and the remaining $T - s$ to $a_i = 0$. Then with k a constant the log likelihood function contains the unknown parameters β , δ , σ^2 , ρ_{12} , ρ_{23} . It is given by

$$\begin{aligned}
 l = k + \sum_{i=1}^s & \left\{ -\frac{1}{2} \log \sigma^2 - \frac{1}{2\sigma^2} (y_{i1} - X_{i1}\beta)^2 - \frac{1}{2} \log (\sigma^2(1 - \rho_{12}^2)) \right. \\
 & - \frac{1}{2\sigma^2(1 - \rho_{12}^2)} (y_{i2} - \rho_{12}y_{i1} - (X_{i2} - \rho_{12}X_{i1})\beta)^2 \\
 & + \log \Phi \left[\frac{R_i\delta + (\rho_{23}/\sigma)(y_{i2} - X_{i2}\beta)}{(1 - \rho_{23}^2)^{\frac{1}{2}}} \right] \\
 & + \sum_{i=s+1}^N \left\{ -\frac{1}{2} \log \sigma^2 - \frac{1}{2\sigma^2} (y_{i1} - X_{i1}\beta)^2 \right. \\
 & \quad \left. + \log \left[1 - \Phi \left(\frac{R_i\delta + (\rho_{12}\rho_{23}/\sigma)(y_{i1} - X_{i1}\beta)}{(1 - \rho_{12}^2\rho_{23}^2)^{\frac{1}{2}}} \right) \right] \right\}.
 \end{aligned}$$

While it may appear complicated, the likelihood function has a simple structure defined in terms of normal density and distribution functions. It combines the variance components specification of the dependent variable y in equation (1.1) with the probit formulation of equation (1.3). The critical parameter for attrition bias is ρ_{23} ; and inspection of the likelihood function demonstrates that if $\rho_{23} = 0$, the likelihood function separates into two parts corresponding to the variance components specification for y and the probit specification for attrition. Thus, if attrition bias is not present, generalized least squares techniques used to estimate equation (1.1) will lead to asymptotically efficient and consistent estimates of the structural parameters of the model, as expected.

We pause for a moment to consider identification of the parameters of A_i . Because of the specification of the equation determining A_i in equation (1.6), $A_i = \alpha y_{i2} + X_{i2}\theta + W_i\gamma + \omega_i$, all variables included in the conditional mean of y_{i2} , the vector X_{i2} , should also be included in R_i , the attrition specification vector. However, for (local) identification it can be shown that no variables "excluded" from X_{i2} need to be included in R_i . That is, the vector W_i need not appear in the specification of A_i . A heuristic argument for identification follows from noting that if the attrition bias parameter, ρ_{23} , is plus one or minus one and $\theta = 0$, then the second period attrition probability is identical to a Tobit specification where W_i does not appear in R_i . On the other hand, if $\rho_{23} = 0$, then the likelihood function factors into two distinct parts, a normal regression model and a probit equation. A consideration of the Hessian of the likelihood function for intermediate values of ρ_{23} establishes nonsingularity and thus local identification. When additional variables are included in W_i , the analysis remains the same.

The specified model of attrition extends in a straightforward manner to more than two periods. An attrition equation is specified for each period; it may include time effects. If once attrition occurs the individual does not return to the sample, then a series of conditional probabilities analogous to equations (1.8) and (1.9)

result. The last period for which the individual appears in the sample gives information on which the random term in the attrition equations is conditioned. For periods in which the individual remains in the sample, an equation like (1.8) is used to specify the joint probability of no attrition and the observed values of the left hand side variables.⁶

Maximization of the likelihood function (1.10) yields estimates of β , δ , σ^2 , ρ_{12} and ρ_{23} .⁷ Numerical estimates based on the Gary experiment are presented in the next section.

2. ATTRITION IN THE GARY INCOME MAINTENANCE EXPERIMENT⁸

The primary goal of the income maintenance, or "negative income tax," experiments is to obtain estimates of potential labor supply and earnings responses to possible income maintenance plans.⁹ Individuals in the experiments are surveyed to obtain retrospective data for a pre-experimental ("baseline") period, normally just prior to the beginning of the experimental period. Two groups are distinguished during the experimental period: controls and "experimentals." Controls are not on an experimental treatment plan, but receive nominal payments for completing periodic questionnaires. Experimentals are randomly assigned to one of several income maintenance plans. The Gary (Indiana) experiment had four basic plans defined by an income guarantee and a tax rate. The two guarantee levels were \$4,300 and \$3,300 for a family of four and were adjusted up for larger and down for smaller families. The two marginal tax rates were .6 and .4. The behavior of experimentals during the experiment can be compared to their own pre-experimental behavior and to that of the control group to obtain estimates of the effect of the treatment plans.

Persons received payments under the experimental plans according to a moving average scheme that took into account income in the previous six months in the determination of payments for a given month. This was to insure that payments did not vary widely with fluctuation in monthly income so long as average monthly income remained stable.¹⁰

⁶ A similar model can be used for analysis of panel data in which missing an interview does not result in terminal attrition. A probability model similar to equation (1.3) is specified for each period. State dependence can be introduced in the probability model by conditioning on status in the previous period. Missing observations are then "integrated out" by the same procedure used to derive equation (1.9).

⁷ We have used an algorithm proposed by Berndt, Hall, Hall, and Hausman [3]. It uses only first derivatives. It is similar to the method of scoring discussed by Anderson [1]. Nelson [20] reported difficulty in using second derivative methods (Newton-Raphson) in a similar problem. We began with least squares estimates of the parameters and our algorithm always converged to the global optimum. This procedure is computationally easier than using initial consistent estimates that could be obtained, for example, using methods discussed by Heckman [15].

⁸ In addition to attrition, a potential problem is created because the sample is stratified according to our endogenous variable. We have found, however, that this problem does not lead to significant bias in parameter estimates. A paper on this topic, Hausman and Wise [13], or an appendix to this paper that considers the subject will be provided to the reader upon request to the authors.

⁹ This summary of NIT experiments is only a brief outline. More detail is contained in Watts and Rees [23] and McDonald, Moffitt, and Kehrer [18]. For a discussion of the econometric theory of the response to a NIT, both Hall [6] and Hausman and Wise [10] are relevant.

¹⁰ For a more detailed discussion of this procedure, see Kehrer, et al. [16].

Two broad groups of families were studied in the Gary experiment: black, female-headed households and black, male-headed households. There was little attrition among the first group, but the attrition rate among male-headed families was substantial. (See Moffitt [19].) Of our sample¹¹ of 585 black males for whom we had baseline data, 206, or 35.2 per cent, did not complete the experiment.¹² Among the 334 experimentals, the attrition rate was 31.1 per cent, while 40.6 per cent of the 251 controls failed to complete the experiment. This difference in attrition rates is not surprising since the experiment is much more beneficial to experimentals than to controls. Other characteristics of individuals may also affect attrition. The effect of these characteristics will be estimated using the model specified in Section 1.

We emphasize again that non-random attrition does not necessarily lead to biased estimates of a structural model of the type presented in equations (1.1) and (1.2). Attrition which is related only to the exogenous variables in a structural model does not lead to biased estimates, since these variables are controlled for in the statistical analysis. However, if attrition is related to endogenous variables, biased estimates result.

Attrition related to endogenous variables is easy to imagine. Beyond a "breakeven" point, "experimentals" receive no benefits from the experimental treatment. The breakeven point occurs when the guarantee minus taxes paid on earnings is zero. Thus, individuals with high earnings receive no treatment payment and may be much like controls vis á vis their incentive to remain in the experiment. But since high earnings are caused in part by the unobserved random term of the structural equation (1.1), attrition may well be related to it. In particular, attrition may be related to the random term in the earnings function for period 2, leading to correlation between ε_2 and ε_3 in equation (1.6).

We will present our empirical analysis in stages beginning with a simple analysis of variance model and proceeding to more elaborately parameterized structural models. To estimate the effect on earnings, say, of the treatment plans, it would appear that a straightforward and simple method is all that is necessary. We need only estimate experimental effects by comparing the mean responses of experimentals and controls; or, equivalently, by estimating the parameters in a simple analysis of variance model. There are, however, several reasons for using a more elaborate specification with more exogenous variables. If assignment to treatment groups is not in practice completely random, then we may want to control for other variables that affect earnings in order to obtain unbiased estimates of treatment effects. In addition, we may want to "parameterize" the experimental treatments in terms of income and wage effects in order to be able to predict the effect of plans not included among the treatment ones. (This, of course, may not make much sense with only two income guarantees and two tax rates.) Finally, we may want to

¹¹ The sample was put together for us by Mathematica Policy Research, who have primary responsibility for analysis of the Gary experiment. Additional information on data availability can be obtained from Mathematica.

¹² While this attrition rate is high, attrition of black males in the New Jersey negative income tax experiment was so high that analysis of the experimental data for blacks was highly suspect. See Peck in Watts and Rees [23, Vol. 2, Part 6, Ch. 1].

use the experimental data just like any other survey data to estimate traditional earnings functions. We will see as we proceed that the possibility of attrition adds another dimension to consider in choosing a method of analysis.

We begin with a straightforward analysis of variance model because under usual assumptions underlying randomized controlled experiments it would be the most natural and appropriate method to obtain estimates of experimental effects. Controlled experiments are in fact designed to permit this method of analysis; they presumably obviate the necessity of controlling for individual characteristics other than experimental treatments.¹³ We will see, however, that it may not be the most appropriate method of analysis when non-random attrition occurs.

A. A Simple Analysis of Variance Model

A simple analysis of variance specification is of the form:

$$(3.1) \quad E_{it} = \alpha + \delta_2 + \xi + \varepsilon_{it} \quad (i = 1, \dots, N; t = 1, 2)$$

where E is the logarithm of monthly earnings, α is the average of E over the pre-experimental period, δ_2 is a time (inflation) effect for period 2, ξ is the experimental effect, ε is a random term with zero mean for each i and each t , i indexes individuals, and t indexes time. The parameters of this model may be estimated by comparison of mean values of E for controls and experimentals for the two time periods.

The relevant information and parameter estimates are presented in Table I. Two important simplifications have been made for purposes of estimation. First, since only three observations are available during the experiment, each for a one month period, their average has been used to obtain a monthly earnings figure for the experimental period.¹⁴ Second, the four experimental treatment groups have been treated as one. They will be distinguished in subsequent analysis. The average of the logarithms of earnings of controls increased by .1108 between the baseline and the experimental periods, while the increase for experimentals was only .0492. The time effect, δ_2 , has been estimated by the difference between the average for controls in period 2 and the average over both controls and experimentals in period 1. The estimate is .1180 with a standard error of .1673. The

¹³ A good treatment of analysis of variance estimation within the context of a social experiment is presented by Hall [6]. The analysis of variance models we have used closely parallel those suggested by Hall in analyzing the effect on white males of treatments in the New Jersey NIT experiment. Attrition among white males in that experiment was much less severe than among black males in this one (and in the New Jersey experiment, as well). We have argued verbally ourselves that simple analysis of variance models should be the preferred method of analyzing data from social experiments, at least to estimate initial experimental effects, because this method does take advantage of basic experimental design. Many of the efforts to obtain labor supply effects based on the New Jersey experiment, for example, seemed to fail largely because, in addition to ignoring truncation in sample selection, they also were overparameterized to control for many individual characteristics or as a concomitant of parameterization of experimental treatments. This more structural approach in many ways runs counter to the spirit and *raison d'être* of elaborate social experiments.

¹⁴ This averaging severely attenuates the unobserved individual effect in equation (3.2) below due to the high variance (transitory effect) in weekly observations. Average annual observations are much preferred, but were not available from the experiment.

TABLE I
 AVERAGE EARNING FOR EXPERIMENTALS AND CONTROLS,
 AND ESTIMATES OF PARAMETERS IN THE MODEL:

$$E_{it} = \alpha + \delta_2 + \xi + \varepsilon_{it}$$

Average earnings		
	Period 1	Period 2
Experimentals	6.2584	6.3176
Controls	6.2710	6.3818

Parameter	Estimates	(standard errors)
Pre-experimental average, α	6.2638	(0.4517)
Time effect, δ_2	0.1180	(0.1673)
Experimental effect, ξ	-.0642	(0.0826)

experimental effect is estimated by the difference in the average for controls and experimentals in period 2. It is $-.0642$ with a standard error of $.0826$. Thus, the estimates do indicate a negative effect of the experimental treatments on labor earnings, but this method yields rather imprecise estimates. We also found, as in the New Jersey experiment, that hourly wages of experimentals and controls did not differ. Thus $-.0642$ per cent is a reasonable indicator of the effect of the experimental treatment on hours worked.

This method of estimation uses information for all persons in our sample of 585 by including data for those who dropped out to obtain the baseline means. (About one-third of the sample dropped out between periods 1 and 2.) But the experimental effect is calculated using only period 2 data; individual specific effects are not allowed.

B. An Analysis of Variance Model with Individual Specific Terms

An alternative analysis of variance specification is of the form:

$$(3.2) \quad E_{it} = \alpha + \delta_2 + \xi + \mu_i + \eta_{it},$$

where the μ_i are random individual specific terms, and the η_{it} are independent and identically distributed with mean zero and a common variance. This formulation takes advantage of the correlation between the "random" component, $\mu_i + \eta_{it}$, of earnings in the two time periods. It essentially allows each individual to serve as his own control. But this advantage is gained at the expense of calculating the time effect δ_2 using only data for persons who did not drop out of the experiment—379 of the original 585 observations. It leads, however to a more precise estimate of the experimental effect ξ , the parameter of primary interest. Both methods yield unbiased estimates if the assumptions of equations (3.1) and (3.2) are correct.

An asymptotically efficient generalized least squares method has been used to estimate the parameters of equation (3.2).¹⁵ The results are shown in Table II. The

¹⁵ This estimator is the mixed estimator of combined variance components and fixed effects models. See Scheffé [22, Ch. 8].

standard error of the experimental effect is only about one-half as large as that obtained in the specification that ignores individual specific terms. The proportion of the total variance explained by the individual effects is .2212; it serves as an indicator of their importance. The estimate of the experimental effect remains about the same—a reduction in earnings of just over 6 per cent.¹⁶ But it is still not significantly different from zero by conventional standards.

TABLE II
ESTIMATES OF PARAMETERS IN THE RANDOM INDIVIDUAL
EFFECTS MODEL: $E_{it} = \alpha + \delta_2 + \xi + \mu_i + \eta_{it}$

Parameter	Estimates	(standard errors)
Pre-experimental average, α	6.2947	(0.0214)
Time effect, δ_2	0.0860	(0.0361)
Experimental effect, ξ	-.0621	(0.0419)

C. Analysis of Variance Model Corrected for Attrition

Although analysis of variance is the classical statistical method for analyzing the results of an experiment, the results may be biased by attrition. From the calculations in Section 1, we can see that attrition will lead to bias if either μ_i or η_i is correlated with the probability of attrition. We argued above that experimentals with higher than average income might be expected to have a higher attrition rate since they receive little or no benefit from the experiment. To check for possible attrition bias, the analysis of variance model of equation (3.2) was combined with the probability of attrition specification of model (1.3). Since analysis of variance has a straightforward regression interpretation, the likelihood function of equation (1.9) is maximized using the technique discussed in Section 2 with "dummy variables" associated with the analysis of variance effects. The attrition specification allows attrition to depend on variables that enter the structural model of earnings (discussed below) as well as other variables. They are:

Constant.

Experimental Effect: One for experimentals and zero for controls.

Education: Years of education.

Experience: Years of experience since starting work.

Income: Log of non-labor family income. It includes foodstamps, AFDC payments, public assistance, and earnings of other family members.

Union: A dummy variable that is one for union members and zero otherwise.

Poor Health: A dummy variable that is one if the individual said that his health was poor in relation to "others" and it limited the amount of work he did; otherwise the variable is zero.

The results are shown in Table III. The experimental effect is now estimated to be about 11 per cent and is significantly different from zero at conventional levels of significance. We found in Section 1 that attrition bias would be zero only if ρ_{23}

¹⁶ The alternative fixed effects estimator, which takes μ_i to be a non-stochastic individual constant, yields an estimate of the experimental effect of $-.0568$ and a time effect of $.0828$.

TABLE III
PARAMETER ESTIMATES FOR THE ANALYSIS OF VARIANCE SPECIFICATION COMBINED WITH
THE ATTRITION MODEL

Analysis of variance		Attrition	
Parameters	Estimates (standard errors)	Variables	Estimates (standard errors)
Pre-experimental average, α	6.2636 (0.0265)	Constant	-.9210 (.2608)
Time effect, δ_2	.1064 (.0408)	Experimental Effect	.2361 (.1131)
Experimental effect, ξ	-.1098 (.0453)	Education	.0172 (.0195)
		Experience	-.0002 (.0050)
		Income	.0934 (.0290)
		Union	1.2018 (0.1100)
		Poor Health	.2715 (.1013)
Attrition bias parameter ρ_{23}	-.8213 (.0449)	Earnings correlation ρ_{12}	.1697 (.0350)
Likelihood value	36.24	Earnings variance σ_n^2	.2147 (.0006)

were zero. Here we find a very precisely measured estimate of $-.8213$.¹⁷ That is, persons with higher earnings, given other measured characteristics, are more likely to drop out of the experiment. Another method to test for attrition bias is to compare differences in estimates of α , δ_2 , and ξ when a "correction" is made for attrition with estimates under the hypothesis that there is no attrition bias. Since under the null hypothesis that $\rho_{23} = 0$ the analysis of variance estimates for equation (3.1) are asymptotically efficient, the lemma of Hausman [8] can be applied to perform a specification test. The lemma states that the variance of the difference of the estimates is the difference of the respective variances. Concentrating on the experimental effect estimates, we see that the difference between the analysis of variance and maximum likelihood estimates is $-.0477$, with a standard error of $.0171$. The χ^2 statistic relative to the hypothesis of no difference has a value of 7.75. The hypothesis of no difference is rejected at any reasonable level of significance. Analysis of variance techniques which do not account for possible attrition bias lead to parameter estimates that differ substantially from the maximum likelihood estimates that take account of such bias. Furthermore, the maximum likelihood estimate of the experimental effect is significantly different from zero at usual levels of significance.

Finally, we note that experimentals appear to have a lower probability of attrition than controls. Higher non-labor income, poor health, and union membership are also associated with lower attrition rates, and the relevant

¹⁷ The null hypothesis of $\rho_{23} = 0$ is rejected using a (Wald) χ^2 test. The χ^2 statistic with one degree of freedom is 334.6.

estimates are rather precisely measured. More education is estimated to be associated with less attrition and more work experience with more, but neither effect is measured with much precision.

We also estimated an analysis of variance model with a slightly more complex attrition specification. The estimates in Table III imply a probability of attrition for experimentals that is .047 less than for controls, if the probabilities are evaluated at the mean values of the other variables. To permit more general differences in the attrition behavior of experimentals and controls, we estimated a model with separate experimental and control coefficients on each of the attrition variables. That is, we allowed complete interaction between all variables and experimental status. However, none of the interactions was found to have a noticeable effect on attrition. None of the interaction terms was greater than one-fourth the size of the corresponding main effect. The attrition bias term ρ_{23} was estimated to be $-.8147$, nearly identical to the estimate of $-.8213$ found for the less complex specification, while the estimated experimental effect, $-.1098$, was identical to the one in the previous model. The maximum likelihood value of 36.62 barely exceeds the value of 36.24 found in Table III. The appropriate χ^2 likelihood ratio statistic (with five degrees of freedom) provides no evidence that the more complicated specification adds to our ability to predict attrition.

We have to this point been referring loosely to the difference between estimates that are corrected for attrition and those that are not as resulting from "attrition bias." This seems to be a correct interpretation since without attrition the analysis of variance model would presumably give an unbiased estimate of the experimental effect. We will see below, however, that the experimental effect estimated from a structural model is not altered much when a correction is made for attrition. Thus, it might be more appropriate to say that analysis of variance estimates of the experimental effect are less robust with respect to attrition than structural model estimates. Why this result might be expected is explained below.

D. *A Structural Model of Earnings Corrected for Attrition*

Structural models have been widely used in the analysis of income maintenance experiments. Such models permit estimation of the income and substitution effects which are needed to predict the response to plans which have not been included in the experimental design. However, to estimate a simple experimental response, it might be argued that only analysis of variance models are needed, given appropriate randomization in the original experimental design. If, in fact, allocation to treatment groups is completely random so that variables indicating treatment group are orthogonal to other exogenous variables that might influence earnings, addition of these variables will affect neither the experimental effect estimates nor their standard errors. If, however, treatment group assignment is not orthogonal to other exogenous variables, we cannot predict a priori whether estimates of the treatment effect from the structural model will be more or less precise than the simple analysis of variance estimates. On the one hand, the variance σ_n^2 is reduced by controlling for other determinants of earnings such as

education and experience. On the other hand, additional variables use up degrees of freedom, thereby tending to increase the variance of parameter estimates.

We have, however, already found strong evidence of attrition bias within the context of the analysis of variance model, and are led to consider an alternative approach. Recall that the bias results from correlation between attrition and earnings in the second period. It may, in turn, be thought of as resulting from correlation between the error in the second period earnings equation and the probability of attrition. If exogenous variables that affect earnings, as well as attrition, are left out of the earnings equation, the correlation between attrition and the error in the earnings equation is magnified. Thus, if attrition is primarily related to exogenous variables in the structural model which are included in the stochastic term in the analysis of variance model, the structural model may be much less affected by attrition than the analysis of variance model.

We have estimated a variance components specification of the structural model $E_{it} = X_{it}\beta + \varepsilon_{it}$, with $\varepsilon_{it} = \mu_i + \eta_{it}$, as discussed in Section 1. Estimates are presented in Table IV. For comparison, generalized least squares estimates of the structural parameters (that are not corrected for attrition) have been included

TABLE IV^a
PARAMETER ESTIMATES OF THE EARNINGS FUNCTION STRUCTURAL MODEL WITH AND WITHOUT A CORRECTION FOR ATTRITION

Variables	With attrition correction: maximum likelihood estimates (standard errors)		Without attrition correction: generalized least squares estimates (standard errors)
	Earnings function parameters	Attrition parameters	Earnings function parameters
Constant	5.8539 (0.0903)	-.6347 (.3351)	5.8911 (.0829)
Experimental effect	-.0822 (.0402)	.2414 (.1211)	-.0793 (.0390)
Time effect	.0940 (.0520)	—	.0841 (.0358)
Education	.0209 (.0052)	-.0204 (.0244)	.0136 (.0050)
Experience	.0037 (.0013)	-.0038 (.0061)	.0020 (.0013)
Income	-.0131 (.0050)	.1752 (.0470)	-.0115 (.0044)
Union	.2159 (.0362)	1.4290 (0.1252)	.2853 (.0330)
Poor health	-.0601 (.0330)	.2480 (.1237)	-.0578 (.0326)
	$\hat{\sigma}_\eta^2 = .1832$ (.0057)	$l^* = 64.35$	$\hat{\sigma}_\eta^2 = .1236$
	$\hat{\rho}_{12} = .2596$ (.0391)	$\hat{\rho}_{23} = -.1089$ (1.0429)	$\hat{\rho}_{12} = .2003$

^a As an indication of computational costs for our sample of 585 observations, the GLS estimation which does not take account of attrition costs about \$4.50 using TSP on the MIT 370-168 computer. The cost of maximum likelihood estimation of the attrition model ranged between \$7 and \$13 dollars, depending on the initial guesses of the parameters.

together with the maximum likelihood estimates that incorporate the effects of attrition. From the last column of the table, we see that the random effects model yields an estimated negative experimental effect of about 7.9 per cent.¹⁸ The individual specific terms account for only 20 per cent of the total variance of the error term, as indicated by the estimated value of ρ_{12} in this model. As mentioned previously, this relatively low value probably results from using the average of only three monthly observations to calculate earnings. Annual figures would presumably include much less random noise. The coefficients on the right-hand side variables all have the expected sign and are measured rather precisely. In fact, the results agree closely with the estimates of Hausman-Wise [10, p. 429] based on data from the New Jersey experiment, where a primary consideration in estimation was correction for truncation bias introduced by the sample design.

Estimates of the parameters in the attrition model of equations (1.6) and (1.7) are presented in the first two columns of Table IV. The attrition bias parameter ρ_{23} is estimated to be $-.1089$. It indicates a small but statistically significant correlation between earnings and the probability of attrition. Although the estimate of the experimental effect is very close to the generalized least squares estimate, some of the other estimates differ substantially from the least squares values. The effect of income on earnings decreases by 23 per cent, while the effect of another year of education increases by 43 per cent. The experimental effect increases in magnitude from $-.079$ to $-.082$, an increase of 3.6 per cent.¹⁹ Thus, within the context of a structural model, some attrition bias seems to be present, but not enough to substantially alter the estimate of the experimental effect. This is in marked contrast with the analysis of variance case, where attrition seems to affect the estimates significantly.²⁰

Finally, we observe that non-labor income, poor health, and union membership are statistically significant and are estimated to reduce the probability of attrition. Experimentals are less likely to drop out than controls. The relevant estimates are not, however, precisely measured. Education and years of work experience are estimated to have small and statistically insignificant negative influences on retention in the sample. Recall that these are "reduced form" estimates in that the direct effect of these variables on attrition cannot be distinguished from their indirect effects through earnings.

Within the context of this structural model we also estimated a more complicated model of attrition, the same one used within the analysis of variance context.

¹⁸ The experimental effect using a fixed effects model was estimated to be minus 6.4 per cent.

¹⁹ Using the lemma of Hausman [8], the difference of .003 has an estimated standard error of .0097. Thus, the difference in the two experimental effect estimates is not statistically different from zero.

²⁰ Comparison with the analysis of variance model yields a likelihood ratio of 56.22 with 5 degrees of freedom, which is significant at all reasonable test sizes. Note, however, that if it were not for attrition, unbiased estimates of the experimental effect would result from an analysis of variance if a correct experimental design were used. In fact, the experimental effect is just as precisely estimated in Table III as in Table IV, which indicates that while the coefficients of the additional variables are significant they do not help to obtain a more precise estimate of the experimental effect.

It allows for full interaction between the determinants of earnings and experimental status. Instead of allowing merely for an experimental effect as indicated by the estimates in Table IV, separate coefficients for experimentals and controls were distinguished for education, years of experience, non-labor family income, health, and union membership. As with the analysis of variance model, no significant differences in these coefficient estimates were found. None of the estimated interaction terms exceeded one-fourth the magnitude of the main effect terms.²¹ The estimate of the attrition parameter ρ_{23} , however, decreased to only $-.040$. The experimental effect was estimated to be $-.0790$, almost identical to the generalized least squares estimate of Table IV.

E. A Structural Model of Earnings with Treatment Groups Distinguished

Because the more complicated model of attrition does not add much to the explanation of attrition, we returned to the non-interaction specification to estimate a final structural model. Instead of specifying a simple experimental effect, we allowed separate effects for each of the four experimental plans. The results are presented in Table V. The likelihood value increased to 71.59 as compared with a value of 64.35 in Table IV. The relevant likelihood ratio statistic, distributed as χ^2 with 6 degrees of freedom, has a value of 14.48. It is significant at the 2.5 per cent level. Although the individual experimental effects are not estimated precisely, their magnitudes are of interest. For convenience, the relevant estimates from the table have been reproduced in the tabulation below. Keep in mind that these estimates are rather imprecise.

		Tax Rate	
		High	Low
Guarantee	High	-.115	-.093
	Low	-.001	-.083

The effect of the guarantee seems to be large relative to the effect of the tax rate. For the high guarantee level, increasing the tax rate does not alter earnings substantially. For the low guarantee level, in fact, persons with a high tax rate are estimated to earn more than persons on the low tax rate plan.

Although it is normally assumed that the effect of an increase in the guarantee should be to reduce labor supply and thus earnings, the *average* effect to be expected from a decrease in the tax rate is not clear. While for an individual already receiving experimental payments (those "on" the experiment), the effect of a decrease in the tax rate may be to increase labor supply, it also brings onto the experiment some persons who were not receiving payments before—some of those above the initial breakeven point. These persons are likely to work less. The

²¹ The maximizing value of the likelihood function increased to only 66.32 relative to the value of 64.35 without these interactions. The two values yield a likelihood ratio statistic of 3.94. This statistic under the null hypothesis of no interactions is distributed as χ^2 with 5 degrees of freedom, and has an expected value of 5.0. Thus, no significant interaction is found.

TABLE V
PARAMETER ESTIMATES FOR STRUCTURAL MODEL WITH FOUR TREATMENT
EFFECTS AND CORRECTION FOR ATTRITION

Variables	Earnings function parameters (standard errors)	Attrition parameters (standard errors)
Constant	5.8503 (0.0702)	-.6692 (.3417)
High guarantee-High tax	-.1148 (.0720)	.5042 (.2167)
High guarantee-Low tax	-.0930 (.0610)	.3990 (.1774)
Low guarantee-High tax	.0009 (.1027)	.1255 (.1601)
Low guarantee-Low tax	-.0831 (.0746)	.1843 (.1483)
Time effect	.0831 (.0533)	—
Education	.0209 (.0052)	-.0212 (.0248)
Experience	.0083 (.0013)	-.0050 (.0062)
Income	-.0129 (.0056)	.1785 (.0488)
Union	.2186 (.0363)	1.4277 (0.1273)
Poor Health	-.0606 (.0335)	.2843 (.2483)
	$\hat{\rho}_{12} = .2614$ (.0396)	$\hat{\rho}_{23} = -.0562$ (.0487)
	$\hat{\sigma}_\eta^2 = .1821$ (.0003)	$l^* = 71.59$

number brought onto the experiment by a decrease in the tax rate may be larger when the guarantee is low than when it is high.

As might be expected, the experimental treatments have different effects on the probability of attrition. Individuals with high guarantees are estimated to have a substantially lower probability of attrition than persons with low guarantees. Persons with high guarantees, of course, receive greater benefits from the experiment.

To recapitulate a bit: We have used a model incorporating the probability of attrition to estimate the treatment effect of the Gary income maintenance experiment. First, we found a significant negative experimental effect on earnings of about 8 per cent. This effect is due almost entirely to a decrease in hours worked. We also found weak evidence that the guarantee level had a greater effect on earnings than the tax level. (To estimate income and substitution effects, the treatment plans would have to be parameterized in terms of implied net wage rates and non-labor income and incorporated into a structural model of hours and wages.) Second, while significant attrition bias is found in both the analysis of variance and the structural models, it is much more serious in the analysis of

variance case. The analysis of variance estimate of experimental effect changes substantially when a correction is made for attrition. However, when a structural model is used, the experimental effect estimated by generalized least squares is found to be very close to the maximum likelihood estimates that incorporate the probability of attrition. Thus, the structural model seems more robust with respect to attrition bias.

3. CONCLUSION

We have specified a model of attrition and have proposed a maximum likelihood method of estimating its parameters. The model yields efficient estimates of structural parameters in the presence of attrition, as well as an estimate of a parameter that indicates the presence or absence of attrition bias. While the method was demonstrated using data from the Gary income maintenance experiment, it is applicable to any panel data. For instance, in the initial years of the National Longitudinal (Parnes) Survey, about 15 per cent of the young males "dropped out" of the survey. The majority of the dropouts entered the military either by the draft or through enlistment. It might well be the case, for example, that the random term in a model of earnings for these young men would be correlated with the dropout probability. Possibly persons with unusually low earnings are more likely to enlist in the armed forces than those with high earnings. This would lead to attrition bias if least squares estimators were used. Because attrition occurs from almost all samples of individuals who are followed through time, techniques which test for possible bias and correct for it when it is present should find many applications in the analysis of panel data, whether collected by traditional survey methods or in conjunction with social experiments.

*Massachusetts Institute of Technology
and
Harvard University*

Manuscript received June, 1977; final revision received January, 1978.

REFERENCES

- [1] ANDERSON, T. W.: "Some Scaling Models and Estimation Procedures in the Latent Class Model," in *Probability and Statistics: The Harold Cramer Volume*, edited by O. Grenander. New York: 1959.
- [2] AMEMIYA, T.: "Regression Analysis when the Dependent Variable is Truncated Normal," *Econometrica*, 41 (1973), 997-1016.
- [3] BERNDT, E., B. HALL, R. HALL, AND J. HAUSMAN: "Estimation and Inference in Nonlinear Structural Models," *Annals of Economic and Social Measurement*, 3 (1974), 653-665.
- [4] FISHER, R. A.: *The Design of Experiments*. Edinburgh: Oliver and Boyd, 1935.
- [5] GOLDFELD, S. M., AND R. E. QUANDT: "The Estimation of Structural Shifts by Switching Regressions," *Annals of Economic and Social Measurement*, 2 (1973), 475-485.
- [6] HALL, R. E.: "The Effects of the Experimental Negative Income Tax on Labor Supply," in *Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment*, edited by J. A. Pechman and P. M. Timpane. Washington: The Brookings Institution, 1975.

- [7] HANOCH, G.: "A Multivariate Model of Labor Supply: Methodology for Estimation," September, 1976, mimeograph.
- [8] HAUSMAN, J. A.: "Specification Tests in Econometrics," forthcoming in *Econometrica*, 46 (1978), 1251-1273.
- [9] HAUSMAN, J. A., AND A. M. SPENCE: "Non-Random Missing Data," mimeograph, 1977.
- [10] HAUSMAN, J. A., AND D. A. WISE: "The Evaluation of Results from Truncated Samples: The New Jersey Negative Income Tax Experiment," *Annals of Economic and Social Measurement*, 5 (1976), 421-445.
- [11] ———: "Attrition and Sample Selection in the Gary Income Maintenance Experiment," mimeograph, 1976.
- [12] ———: "Social Experimentation, Truncated Distributions, and Efficient Estimation," *Econometrica*, 45 (1977), 319-339.
- [13] ———: "Stratification on Endogenous Variables and Estimation: The Gary Income Maintenance Experiment," mimeograph, 1977.
- [14] HECKMAN, J.: "Shadow Prices, Market Wage, and Labor Supply," *Econometrica*, 42 (1974), 679-694.
- [15] ———: "The Common Structure of Statistical Models of Truncation, Sample Selection, and Limited Dependent Variables and a Simple Estimator for Such Models," *Annals of Economic and Social Measurement*, 5 (1976), 475-492.
- [16] KEHRER, K. C., E. K. BRUML, G. T. BURTLESS, AND D. N. RICHARDSON: "The Gary Income Maintenance Experiment: Design, Administration, and Data Files," mimeograph, 1975.
- [17] MADDALA, G. S., AND F. D. NELSON: "Switching Regression Models with Exogenous and Endogenous Switching," *Proceedings of the Business and Economics Statistics Section, American Statistical Association*, 70 (1975), 423-426.
- [18] McDONALD, J. F., R. A. MOFFITT, AND K. C. KEHRER: "The Negative Income Tax and Labor Supply: Methodological Issues and Analytic Strategy," mimeograph, 1976.
- [19] MOFFITT, R. A.: "Selection Bias in the Analysis of Experimental Data: Empirical Results in the Gary Negative Income Maintenance Experiment," mimeograph, 1976.
- [20] NELSON, F. D.: "Censored Regression Models with Unobserved, Stochastic Censoring Thresholds," mimeograph, 1976.
- [21] QUANDT, R. E.: "The Estimation of the Parameters of a Linear Regression System Obeying Two Separate Regimes," *Journal of the American Statistical Association*, 53 (1958), 878-880.
- [22] SCHEFFÉ, H.: *The Analysis of Variance*. New York: John Wiley and Sons, Inc., 1959.
- [23] WATTS, H. W., AND A. REES (EDS.): *Final Report of the New Jersey Graduated Work Incentives Experiment*. Madison, Wis.: Institute for Research on Poverty, University of Wisconsin, Madison, 1974.

