The Economic Theory of the Household and Impact Measurement of Nutrition and Related Health Programs

World Bank Staff Working Paper No. 302

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Development Economics Department
Research Project No. 671-38

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WORLD BANK

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THE ECONOMIC THEORY OF THE HOUSEHOLD AND IMPACT MEASUREMENT OF NUTRITION AND RELATED HEALTH PROGRAMS

This paper conveys in broad terms an economist's approach to concepts and measurement of nutrition and related health programs. It is an outgrowth of an interdisciplinary research project involving the School of International Health of the Johns Hopkins University and the Development Economics Department of the World Bank. This project, which summarized the Narangwal intervention experiment in India, and policy interventions in general gave rise to the need for laying out informally the conceptual framework of the economic theory of the household and the relevance of this theory to evaluation research in nutrition. This paper is an attempt to meet this need by outlining, with no loss of generality, how this theory and its statistical adjunct—econometrics—can be used for suggesting testable hypotheses concerning program impact, defining outcome measures, and measuring program impact.

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THE ECONOMIC THEORY OF THE HOUSEHOLD AND IMPACT
MEASUREMENT OF NUTRITION AND RELATED HEALTH PROGRAMS

Dov Chernichovsky*

World Bank

INTRODUCTION

Household economics—as a theory of choice—offers a conceptual framework in which to investigate the family's responses to changes in its environment. Since such changes can be induced, for example, by nutrition and health intervention programs, this framework can be useful for policymakers and planners in formulating hypotheses about the effects of such programs. Econometrics, the complementary statistical extension of economic theory, furnishes a versatile statistical framework for testing these hypotheses and quantifying the effects of such programs, as well as increasing our basic knowledge about the interactions between the programs and their social environments.

This paper conveys—in broad terms—an economist's approach to concepts and measurement of nutrition and related health programs. It emphasizes the close link between economics as a behavioral science and the measurement of intervention programs. In the first section, the basic working assumptions and framework of household economics are introduced and related to the concept of an intervention program. This relationship serves to highlight the economist's conceptual point of departure in analyzing

*/ My colleagues A.A. Kielmann, M.D., and D. Coate, as well as participants in the PAHO conference for which this paper was prepared, should share in any credit for this presentation. However, neither they nor the World Bank should be held responsible for the views expressed here.
nutrition and health related interventions and measuring their effects. In
the second section, the household's behavioral objectives that are used to
specify and measure the outcomes of intervention programs are discussed.
These objectives provide the conceptual framework for considering various
properties of outcome variables and measurement problems in the third
section. The econometric approach to socioeconomic studies in nutrition is
discussed in the fourth section. It serves as a basis for discussing in the
fifth section the basic differences between an experimental approach and an
econometric approach in measuring program impact.

I. INTERVENTION PROGRAMS AND HOUSEHOLD ECONOMICS

Health-related environmental programs like malaria eradication and
largely mandatory programs like smallpox vaccination apparently have been
relatively successful in meeting their objectives (World Bank 1975: 38-39),
while many personal health and nutrition programs appear to be less success-
ful. A common feature of the environmental programs and the mandatory
programs is that their implementation does not require individuals or
households to choose how to respond. The environmental programs do not deal
directly with individuals or households, and mass vaccinations may leave
little or no room for individual or household choice. The apparent failure
to detect success in personal health and nutrition programs can be attrib-
uted to many factors. It can result from the lack of data and statistical
tools to measure success. However, it can also result from the target
populations' particular response, or lack of response, to the programs.
Malnutrition results largely from a combination of individual and household consumption behavior and hygienic decisions and practices. This fact limits the feasibility, economic and otherwise, of mandatory and effectively controlled nutrition programs; that is, these programs are most likely to leave individuals and households choices of (a) whether to participate in a program and (b) how to use resources that become available through it. It is the existence of these choices that makes the economic theory of the household a useful evaluation tool, the importance of which corresponds to the potential degree of voluntarism in individual and household responses to a program.

The household, whether a nuclear or an extended family, is the basic socioeconomic unit that makes most decisions about investment in human beings and about consumption. 1/ The significance of a household and an individual to the community is not limited to their role as components of a sum. An individual's education, and particularly his health, often affect the well-being of others in the community; communicable diseases exemplify the interdependence between an individual and his community. This interdependence and certain moral norms concerning the distribution of well-being among households in the community provide much of the basic rationale for health-related intervention programs.

Economics usually views the household as a harmonious microcosm that makes deliberate and rational decisions. This is a basic working assumption employed to identify the systematic part of human behavior by using conceptual parameters and measurable variables. This assumption is eventually

1/ It is important to realize, however, that in some traditional cultures, tribal or village governing entities might make important decisions about investment in human capital.
"modified" in econometric analyses by taking into account unsystematic variations of behavior. The economic analysis of household behavior can be summarized as follows. 2/ Households and individuals engage in activities to produce "ends," or consumption commodities, that render utility. 3/ These ends compete for the household's scarce human and nonhuman resources because producing more of one commodity implies producing less of others. A change in the household's environment can change (a) the household's income or wealth, which determines how much a household can produce, (b) the commodities' relative costs or prices, which determine the relative attractiveness of different commodities, and (c) the household's tastes and preferences structure. Modifications in household behavior are derived from changes, which an intervention program can promote, in one or more of these three. An economic conceptualization of the household's decision making process is sketched in a simplified manner in Figure 1.

From the viewpoint of household economics, an intervention program has at least one inherent problem: such a program "disputes" the household's ability and even willingness to realize the social consequences of its choice, and to meet some specified social objectives. This is equivalent in many instances to questioning the rationality and social adequacy of the household's decision-making process and objectives. There are two critical

2/ The approach presented here, in broad terms, is based on traditional demand theory and some extensions of this theory by Becker (1965) and Lancaster (1966). Differences in approach, which bears on the conceptual framework, do (and should) not affect more practical measurement issues.

3/ Those commodities can be abstract as are, for example, "good health" and "services from children."
FIGURE 1: AN ECONOMIC CONCEPTUALIZATION OF THE HOUSEHOLD'S DECISION MAKING PROCESS

Period One ($T_1$)
- **Household Endowments**
  - Time
  - Leisure Time
  - Market Prices
  - Market Wages
  - Work Time
  - Income
- **Determinants of Choice**
  - Human Capital
  - Tastes and Preferences
  - Capital Assets
- **Choices (Outcomes)**
  - Intervention
  - (Mandatory)
  - Consumption and Investment in Human Capital
  - Savings

Period Two ($T_2$)
- **Household Endowments**
  - Time
  - Nutrition, Health and Education
  - Human Capital
  - Capital Assets
implications of this inherent problem. First, an effective demand for, or utilization of, program services by the target population cannot be guaranteed; second, even when adequate demand exists, it may stem from private objectives that are not congruent with program or social objectives.

Therefore, a basic requirement for evaluating program impact is to identify the "non-program" parameters that determine the household's demand for program services. This identification should help to indicate how much a household will use the program—when it will do so—and what use it may make of the program's resources.

Program utilization is a function of (a) the degree the program serves the household's objectives, (b) the degree it draws on household resources, and (c) the relative attractiveness of other—subjectively conceived—substitutes for program services that meet the household's objectives. These points can be illustrated by common examples. Preventive medical care programs, particularly nutrition programs, are often hard to implement because the target populations may not recognize their usefulness. In many situations, health services may compete with traditional practices that households perceive as substitutes. Program services may not be used, even when people recognize their usefulness, because of the relatively lower costs, or higher benefits, of presumed or proven substitutes, and because of the burden the program may impose on a household's immediate welfare, as when people cannot afford the time and transportation costs to go to a "free" clinic.

Program utilization need not be consistent, however, with program objectives. A target household has the capacity to reallocate program
resources to reach its own rather than the program's objectives. For example, a mother may feed a particular child less than she feeds other family members because that child participates in a school feeding program. Thus, the mother may attenuate and offset the program's specific objectives and may "spread" its impact. Furthermore, through the effect on one aspect of household life, say child mortality, the program may have an impact on other aspects, say fertility behavior. Although these other aspects may not be among the original objectives of a particular program, nonetheless, one should consider them as possible benefits or costs of the intervention.

The economic analysis of the household—by relating program services to household resources and objectives—can help to identify (a) the potential uses of program services, (b) program substitutes, (c) the relative attractiveness of the program, and (d) the extent the program draws on household resources. One can thereby hypothesize who program users will be, how they may utilize the program, and subsequently, what the program's impact may be. The formulation of testable hypotheses about program impact aids in identifying variables and relationships that may measure program impact.

II. AN ECONOMIC FORMULATION OF HEALTH-RELATED OUTCOMES

Program "impact" or "outcome" variables must be identified and discussed in conjunction with households' objectives or ends. This section deals with the household's behavioral objectives, around which we can model family responses to health intervention programs, and on the basis of which we can identify and discuss appropriate outcome measures.
Economists have long sought to establish adequate measures of welfare, which is taken as the ultimate goal of man's economic activity. In the absence of better measures, monetary income has been used as a second best measure of welfare. Income, which flows from labor and accumulated stocks of assets or capital, serves as a proxy for welfare because higher levels of income mean higher levels of production and thus more commodities that render utility. Measured incomes, however, ignore many utilitarian non-market household activities such as leisure. Furthermore, higher levels of income do not necessarily correlate with greater lifetime well-being when all dimensions of human welfare are considered; the health and nutrition problems of affluent societies are evidence of this last point (Eckholm and Record, 1977).

Health status is itself a key element, as well as a good proxy measure of other aspects of human welfare; it can be considered both a consumption item and an investment item (Mushkin, 1962; Grossman, 1972). Good health is "consumption" because it is an end in itself, accounting for a considerable part of that human welfare not measured by income. Health is also an "investment" because it is a key component of human capital that determines the level and duration of one's market and nonmarket activities. As such, it can be linked to some measurable components of earnings and nonmonetary income.

This discussion concentrates on the investment aspect of health because this aspect sets the lower limit of potential benefits from health programs. 4/ The discussion is structured around the concept of expected

4/ A rate of return on a health program as an investment will always understate the actual return by excluding the unmeasurable (consumption) utility derived from good health.
lifetime earnings. In a simplified way, one can define for an individual of age A the present value of his lifetime expected earnings for N years henceforth by

\[ E = \sum_{i=1}^{N} P_i^S \cdot P_i^H \cdot W_i \cdot (1 + r)^{-i} \]

where \([P_i^S, P_i^H, W_i (1 + r)^{-i}]\) states the present value of the earnings an individual "expects" at period i. It is given by his (conditional) probability to survive to that period, \(P_i^S\); by the probability of his being physically able to work that year, \(P_i^H\); and by his average anticipated productivity—or returns, psychic and others, from his time—during that year, \(W_i\). Given a particular time discount rate \(r\), each time-specific expected earnings component is discounted by \((1 + r)^{-i}\), which is the present value of a unit of earning at a particular future period. 5/ Each term \(P_i^S, P_i^H\), or \(W_i\) can be regarded as a health outcome; together they encompass the "investment" dimensions in health. 6/ \(P_i^S\) is based on age-specific mortality rates, \(P_i^H\) is based on age-specific morbidity rates measurable by lost working days, and \(W_i\) can be measured by one's average daily wage rate.

The productivity measure or wage rate, \(W_i\), may warrant more attention because, unlike the other terms, it is not usually assumed a direct outcome of nutrition and health. In a given economic setting, defined by the available technology, land, and capital, an individual's wage rate can be taken as a function of his innate physical and mental abilities, and of his physical and mental capacities acquired through education and work experience (Mincer, 1974.) Since educational achievement and work experience are

5/ \(W_i\) can be regarded as a term net of investment in health and education that affects the levels of all three terms.

6/ For simplicity, we ignore the interdependence among the three terms.
outcomes of a process that depends partially on health and nutrition, man's productivity, or $W_i$, can be viewed as a function of his health and nutrition, at least in situations of severe malnutrition.

Once expected earnings, as specified, are regarded as an individual's or a family's objective, the economic analysis can derive certain predictions about the household's behavior. The common approach is a maximization procedure by which the household is assumed to enhance $P_i^s, P_i^H, W_i$ and other utilitarian ends, subject to various constraints. This procedure sets the tradeoffs among different ends, defines behavioral optima for each, and thereby provides the analytical framework to deal systematically with the relevant aspects of household behavior and to generate a set of refutable hypotheses. 

III. IMPACT MEASURES: BASIC PROBLEMS AND SOLUTIONS

The specification of the ultimate outcome variable—expected lifetime earnings—and its components is also important for discussing some key statistical issues that relate to impact measurement and to some basic properties of impact measures.

Time lags are critical in measuring program impact and present a key statistical problem inherent in intervention programs. The longer the time between the intervention and the measurement of its outcome, the

__/ Anthropological insights into, as well as prior evidence about, the household's view of the underlying investment process are essential for adequate modelling. That is, for example, particular household members may get substantially better diets and "health investment" than others because their (lifetime) earnings are important from the household's viewpoint. Some discrimination in feeding among household members to protect the actual or potential breadwinner is apparent in subsistence settings.
harder it is to link the program to its hypothesized impact, because one must also account for environmental, biological, and behavioral changes that may also affect the outcome. This issue, which is discussed in more detail in the next section, is frustrating because the impacts of health and nutrition programs take time to manifest and are often spread over individuals' lifetimes.

This problem of time lags warrants both a conceptual and a practical distinction between two types of programs. The first type is a program aimed at enhancing the stock of human capital by an intervention during some critical period of human physical and mental development. Programs involving mothers and children are of this type. The second type is a program aimed at increasing the efficiency of a flow of services from an existing stock of human capital. Programs involving adult workers are of this type (Basta and Churchill, 1974).

The first type of program has long-run outcomes that are often not practical to measure because of the long time lag. Consequently, one must resort to proxies for measurements. The second type has shorter-run objectives, primarily increasing productivity and reducing absenteeism, that are immediately observable for adult workers. Termination of a program of the second type should end its effect and thereby provide another means of testing impact. Thus, selecting appropriate outcome measures is more problematic for the first, and more common, type of program. Consequently, this discussion focuses on outcome measures for the more general type of program with long-run impact.

In evaluating a potential outcome variable, one should consider these questions:
i. How does the variable relate conceptually to the ultimate outcome, or any component thereof, and to the program under study?

ii. How does it relate statistically to the ultimate outcome variable, or any component thereof, and to the program under study?

iii. How reliably can it be measured?

iv. What complementary data are needed?

v. What are the costs of obtaining and using those data?

The first question is important because it relates an observed variable to the conceptual framework and to the specific hypotheses to be tested. The other questions bear largely on the statistical aspects of potential data for testing program impact. When all these questions are considered, trade-offs among particular types, or categories, of variables may appear.

We can classify outcome variables by three categories: inputs, intermediate outcomes, and ultimate outcomes. The use of program inputs as proxy measures of program impact is common; when, for example, the impact of a school feeding program is estimated by the amount of calories and protein the program delivers to the target population. This approach has the merit of being directly related to the program, and also is probably least costly since it is integrated with the program. However, it also tends to be the most presumptive since it may depend on hypotheses yet to be tested about relationships between inputs and eventual impact, and it may ignore program-induced behavioral changes beyond program control. This last issue depends critically on the delivery method; an income transfer to the household is easier for the household to divert from program objectives than, say, a directly administered vaccine.
Intermediate outcomes can be measured by a variety of variables: child morbidity, intellectual development, school achievement, and anthropometric measures including birthweight. These measures apply largely to children because theory and evidence suggest that they predict, and thus approximate, eventual health outcomes (St och and Smythe, 1976). Although they are not ultimate outcomes, such intermediate variables are "outputs" and in most cases they approximate ultimate outcomes better than program inputs do. Birthweight, for example, predicts relatively well a child's physical growth, at least during the first years and can be used as an outcome measure for maternal care programs; (Lechtig et al., 1975). A child's physical growth and morbidity at an early age may indicate his future morbidity, survival probability, and productivity. A child's intellectual development, which is to a degree nutritionally determined, is believed to be manifest eventually in his productivity and wages, primarily through mental development and school achievement (Selowsky, 1976; Taylor and Selowsky, 1973; Grossman and Edwards, 1977.)

These intermediate outcome variables raise the problem of lagged program impact since they basically are manifest over time. Hence, linking the intermediate outcomes to the program inputs is even more complicated than using inputs as proxies for outcomes. Complementary data on non-program variables that affect the outcome may become critical for identifying the impact of the program. Consequently, collecting data that relate to the intermediate outcome variables requires more elaborate data collection instruments and statistical tools than when program inputs serve to measure outcomes. Ultimate outcome variables involve issues similar to
those of the intermediate outcomes, but they are more difficult since they pertain to full lifetimes.

Outcomes and related variables can be measured on the basis of individuals, households, and communities. The choice of the measurement unit depends on the specific measurement objectives. Policymakers and program administrators are eventually interested in variables that summarize their efforts on a community level. Students of household behavior are also interested in understanding and explaining the distribution of outcomes in the household or the community. Or, they may seek to understand why identical program inputs have a varying impact across individuals and households of different characteristics.

While critical for identifying the circumstances under which programs are beneficial, differences in impact may be concealed when we aggregate or, at times, disaggregate data. This possibility must be recognized when we define the unit of measurement. One cannot always distinguish household variables from variables pertaining to individuals. At times a variable can be based on a particular household member; at other times, it can be based on a few members. The definition of a household variable based on more than one household member may be complicated because of the low incidence of health related events at the household level. Identification of the target population is a key criterion for the selection and definition of a household variable. If the target group consists of, say, mothers or potential mothers and we are interested in their nutritional status, then the observation is the mother—an individual, as well as the household because in most societies we observe one mother per household. This household then can be described by
other common household variables such as religion, income, size, or location, etc. The same reasoning applies when the target group consists of children of given age and sex.

A problem usually arises when one has to aggregate within households or other units that share the same socioeconomic endowments. This problem appears particularly acute in measuring some intermediate nutritional outcomes. For example, when the nutritional status of all school-age children in a household is of interest, one needs to define a summary variable summarizing the nutritional status of these children in that household. The problem is that the number of children as well as age and sex distributions vary across households. This problem can be handled in a few ways; however, while aggregating within the household, one must consider the possibility that not all children are treated equally in a given household.

The same problem applies to other household variables. For example, two mothers or two family units may live in one household and share a common income. Splitting such a group's income between the two family units, and subsequently treating them as separate units for statistical purposes, may be erroneous if behavioral patterns in extended households differ from the patterns in nuclear households. In most cases when data are treated by averages, a few critical behavioral issues are assumed: that individuals are not discriminated against, and that the behavior of aggregated social units is the sum of the behavior of some other individual units. Before aggregating or disaggregating data, even at the household level, one must see whether these assumptions lead to different predictions.
IV. AN ECONOMETRIC APPROACH

We turn now, before discussing program measurement, to outline some basic features of econometrics that draws on mathematics, statistics, and economic theory to delineate economic relationships empirically. The development of econometrics has been largely influenced by economists' traditional use of non-experimental data to test hypotheses and to verify economic relationships.8/

Economic theory attempts to describe the nature of particular causal relationships, or "structures", by identifying the parameters that make them up, and by indicating the particular functional relationships among those parameters. As indicated previously, these relationships attempt to account, however, only for what is believed to be the systematic part of man's economic behavior. In reality this behavior also has random elements and may be determined by parameters that we fail either to identify and observe, or to measure accurately. Consequently, the econometric equivalent of the relationships suggested by theory include a "disturbance" or error term added to the "systematic" part of a particular relationship. This term summarizes the random, omitted, and unidentified or inaccurately measured elements of man's behavior. For example, if a particular simple economic relationship is characterized by

\[ Y = f(X), \]

its econometric equivalent is

\[ Y_i = f(X_i) + v_i \quad (i = 1 \ldots \ldots n \text{ observations}) \]

where \( v_i \) is the disturbance or error term.

8/ The term "econometric approach" used here is substantially the same as the "structural equations approach", see Duncan (1975). That is, it is a general application of mathematical statistics not necessarily restricted to "economic" variables. The reader familiar with this approach may bypass this section.
Thus, econometric relationships also are causal relationships and are stated with one or more equations, depending on the underlying structure, each having a single dependent, or "outcome", variable and one or more independent variables. 9/ Estimating such equations involves obtaining usually, by means of "statistical control", at least unbiased estimates of the particular effect on the outcome variable of each independent variable. 10/ Such estimates are possible when there is no, or only a "small", correlation between the error term and any of the right hand variables, and between any two of these variables.

Given a particular economic model, at least three considerations are pertinent in specifying and interpreting econometric relationships: choosing explicit functional relationships, controlling for variables not suggested by theory, and dealing with different and, therefore, competing hypotheses consistent with a particular estimate. The specification of a particular functional relationship can be based on common sense, theoretical and empirical knowledge, and experimentation. For example, we expect caloric consumption normally to level off as income rises, because of saturation and also because of substitution away from calories and carbohydrates to more "luxurious" proteins. Hence, the specifications of a functional relationships between caloric consumption and income should allow for a nonlinear relationship.

The two estimated equations shown in Table 1 are a simple example of econometric equations; the effects of certain household variables on

9/ A common introductory textbook to econometrics is by Johnston (1972). See also the Annex, Sections A and B.

10/ The common estimation procedure is regression analysis, which is also a useful descriptive tool. For biological applications of regression analysis, see for example, Snedecor and Cochran (1967:381-418).
Table 1: REGRESSION COEFFICIENTS, CARBOHYDRATES (In Grams) AND VITAMIN A (In Int'l Units) AS DEPENDENT VARIABLES

(t statistics in parentheses)

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Independent Variables</th>
<th>Family</th>
<th>Family</th>
<th>North-Urban</th>
<th>Region</th>
<th>Urbanization</th>
<th>Education</th>
<th>Adjusted</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Income*</td>
<td>Size*</td>
<td>(Intercept)</td>
<td>South**</td>
<td>Non-Farm**</td>
<td>Rural-Farm**</td>
<td>High School**</td>
</tr>
<tr>
<td>Vitamin A*</td>
<td></td>
<td>0.1542</td>
<td>0.5130</td>
<td>10.7470</td>
<td>-0.1310</td>
<td>0.0229</td>
<td>-0.0030</td>
<td>0.1852</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(6.9459)</td>
<td>(4.1073)</td>
<td>(-4.6454)</td>
<td>(0.5313)</td>
<td>(-0.0864)</td>
<td>(4.4095)</td>
<td>(3.7289)</td>
</tr>
<tr>
<td>Carbohydrates*</td>
<td></td>
<td>0.0085</td>
<td>0.9135</td>
<td>7.8833</td>
<td>0.0579</td>
<td>0.2657</td>
<td>0.1489</td>
<td>0.0486</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.6250)</td>
<td>(11.8791)</td>
<td>(3.3276)</td>
<td>(9.9887)</td>
<td>(6.9906)</td>
<td>(1.5837)</td>
<td>(1.4597)</td>
</tr>
</tbody>
</table>

* The logarithms of the variable values were introduced in the estimated equation.

** Denotes the use of a "dummy" variable which takes the value of 1 when an event occurs and the value of 0 otherwise.
household consumption of vitamin A and carbohydrates are estimated. These estimates are based on a relationship like that shown as (2) in the Annex, Section B. This relationship allows for the expected nonlinear effect on consumption of nutrients by employing the logarithms of intakes of carbohydrates and vitamin A, and the logarithm of household income. This functional relationship was also chosen because it allows for a comparison between the sensitivity of the intakes of carbohydrates to income, and the intakes of vitamin A to income, regardless of the different units by which the two nutrients are measured, grams or international units. Such a relationship may impose various restrictions on the estimates; for example, the relationship specified presumes no consumption when the household has no current income. Nevertheless, such an unrealistic presumption and other restrictions do not outweigh the advantages of using this particular relationship.

Another example of choosing functional relationships involves measurement of child growth by weight and height. Again, nonlinearity concerning age is appropriate here. The estimates shown in Tables 2 and 3 use a quadratic functional relationship that allows for this nonlinearity. These examples show how particular functional relationships are chosen on the basis of prior knowledge as well as practical considerations.

Controlling for variables not originally suggested by theory may be a useful means for improving the precision of the estimates, standardization,

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11/ This table is drawn from Chernichovsky (1977) where an attempt is made to use economic theory and econometrics to predict and measure the effects of household characteristics on its diet. The coefficients on the logarithmic variables show the percentage change in household consumption of the particular nutrient due to a given percentage change in these variables. The coefficients on the dummy variables show the same but compared with variables which were "left out", or included in the intercept term. These estimates and others are used here for illustrative purposes only.
Table 2: Regression Coefficients, Child's Weight in Kg
As a Dependent Variable, Ordinary Least Squares (OLS)
and Two-Stage Least Squares (TSLS) Estimates
(t statistics in parentheses)

<table>
<thead>
<tr>
<th>Equation No.</th>
<th>Type of Estimation</th>
<th>Intercept</th>
<th>&quot;Biological&quot; Variables</th>
<th>Economic Variables: Occupation</th>
<th>Diet</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Age</td>
<td>Age^2</td>
<td>Sex-Male*</td>
</tr>
<tr>
<td>1</td>
<td>(OLS)</td>
<td>-0.63771</td>
<td>0.609</td>
<td>-0.008</td>
<td>7.533</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>(OLS)</td>
<td>3.173</td>
<td>0.245</td>
<td>-0.019</td>
<td>0.708</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>(TSLS)</td>
<td>1.788</td>
<td>0.112</td>
<td>-0.0001</td>
<td>0.169</td>
</tr>
</tbody>
</table>

* Dummy Variables.
and adding information. 12/ An economic model of behavior may ignore biological factors by assuming them constant. For example, a model that deals with parents’ choices of their children’s diets and, therefore, child growth (Chernichovsky and Coate, 1977) might assume that the analysis is confined to a hypothetical age and sex group and, therefore, disregard the age and sex variables in explaining variations in child growth. However, for an econometric analysis based on a relatively small sample of children of both sexes and across age groups, one should control for age and sex as illustrated in Table 2, Equation 2. Otherwise, the relationship between child weight and age will be entirely approximated by the correlation between diet and weight across children of different age groups. 13/ This means of control is an alternative to various standardization procedures, like weight for age, used by nutritionists. It is also informative because it depicts growth curves directly across age and sex groups—controlling for other effects—and can be based on relatively small samples.

The problem of dealing with competing hypotheses is major. The first and basic issue to address is whether the line of causality implied by a particular relationship is correct. For example, in Table 2, Equations 2 and 3, the effect on children’s weight of caloric intakes is estimated. However, the case can be made that the causality also operates the other way:

12/ Note that inclusion of omitted variables, which are not perfectly correlated with other independent variables, will increase the multiple correlation coefficient, or "explained variance" of the dependent variable. This increase should not be a goal by itself.

13/ This procedure can be elaborated to account for interaction between the age or sex variables and others variables; that is, when, for example, a given diet has a different effect on children of different age and sex.
heavier children consume more calories *ceteris paribus*. In a case like this it is reasonable to assume a structure where child weight and caloric consumption are co-determined. Therefore, one must use appropriate estimation techniques to allow for this codetermination or simultaneity. The estimates reported in these equations exemplify a case like this. The estimated effect of caloric consumption on weight in the third equation is based on a procedure that accounts also for the effect of weight on caloric consumption. This estimated effect differs from the estimate in the second equation where the estimation procedure does not account for simultaneity. 14/

The second issue is to ascertain that the variables indeed measure (or approximate) what they are meant to measure. In the context of nutrition interventions, a good example is the behavioral change in food consumption of families under observation due to presence of an interviewer. Thus, the variable that was supposed to measure the effect of program inputs measures instead another behavioral change induced by the program, thereby confounding the measurement of the program impact. Similarly, the estimated effects of the sex variable on children's nutritional status may measure parents' behavioral discrimination between sons and daughters, as well as genetic differences between sexes. In a case like this, it may be impossible to

---

14/ Two or more variables are simultaneously determined when they are outcomes or endogenous variables belonging to a structure where one of these variables affects the other in some relationships and vice versa in others. Failing to account for this codetermination while estimating one particular structural relationship, may result in a "simultaneity bias" which means that the estimated effect of one endogenous variable on the other way be under- or overstated. On how to deal statistically with such cases, see Johnston (1972: 341-423); for specific application in a nutrition study from which this table is drawn, see Chernichovsky and Kielman (1978).
discriminate statistically between the two competing hypotheses. Such cases put an added burden on the scientist beyond getting unbiased estimates; he must be careful in interpreting his findings in light of a variety of behavioral and other relationship that can produce particular statistical results.

Some cases of competing hypotheses are more predictable and statistically manageable than others. These cases can be generally characterized as those in which one has some notion or knowledge about correlations between only two independent variables. For example, the U.S. food stamp program is designed for low income groups. Consequently, a variable representing household participation in the program is expected to be negatively and highly correlated with the income variable in an analysis of this program's impact across income groups. Therefore, statistical discrimination between the effect of the program as opposed to the effect of income on a particular outcome variable may present a significant problem. A correlation between the disturbance term and one or more independent variables may have similar consequences.

Econometrics is concerned with the problems briefly described here and other related ones. The solutions offered are geared, however, to existing data and, consequently, are largely based on statistical control.

V. IMPACT MEASUREMENT: ECONOMETRICS AND SOCIAL EXPERIMENTS

An illustration based on one of the major "experiments" in nutrition can be helpful to the more general discussion that follows about the

15/ For a technical discussion on related issues in the context of a nutrition project, see Nasoetion (1975).
use of econometrics in measurement of program impact and the relationship between econometrics and social experiments.

The estimated regression coefficients shown in Table 3 are based on the Narangwal experimental project. 16/ The first equation—which is equivalent to comparing the mean outcomes of an experiment—shows the estimated effects on child weight of various interventions, without controlling for other variables that might affect the outcome (see annex, section E.) The second equation shows the same, but with statistical control for age, sex, and socioeconomic status approximated by caste. The first equation shows that nutritional supplementation has a negative and statistically significant effect on child weight, while medical care and a combination of medical care and nutritional supplementation had no effect. Once we control for the other variables, intervention by nutritional supplementation and by nutritional supplementation combined with medical care show a positive and statistically significant effect on child weight.

The Narangwal experiment was not a "true" experimental design, but for that matter, it probably could never be. 17/ Moreover, it represents well the constraints of the kind most field programs face. Treatments were

16/ The discussion here is brief and for illustrative purposes only. It does not represent the results of the entire experiment. For a description of the design of the experiment and its results, see Taylor et. al. (1978).

17/ "Experimental design" refers to use of a "control" group and "treatment" group (or groups) selected entirely by a random process. In the purest and simplest type of randomization or experimental design, as opposed to an econometric approach, one is not concerned with identifying and specifying particular causal and structural relationships and with estimating their various parameters. Full discussion of experimental design is beyond the scope of this paper. The discussion here is based on Campbell and Stanley (1966), Rose and Smith (1968), and Bennett and Lumsdaine (1975).
Table 3: REGRESSION COEFFICIENTS, CHILD’S WEIGHT
IN KG. AS A DEPENDENT VARIABLE
(t statistics in parentheses)

<table>
<thead>
<tr>
<th>Equation No.</th>
<th>&quot;Biological&quot; Variables</th>
<th>Socio-Economic Status</th>
<th>Types of Intervention</th>
<th>R^2</th>
</tr>
</thead>
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<td></td>
<td>Intercept</td>
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<td>(Age)^2</td>
<td>Sex</td>
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<tr>
<td>1.</td>
<td>7.96897</td>
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<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>2.</td>
<td>3.18148</td>
<td>0.00967</td>
<td>-0.00001</td>
<td>0.51932</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(10.8162)</td>
<td>(-4.59184)</td>
<td>(4.00037)</td>
</tr>
</tbody>
</table>

* Dummy variables.
applied to entire villages because random assignment of the treatments within villages was socially and practically inconceivable. Randomization on the basis of a population of villages was impractical because it would inflate the program to proportions beyond its financial and logistic means. Two issues had to be considered before estimating program impact. First, although similar, the villages may have been different also in aspects other than the treatments. Second, there was scope for behavioral "self selection"; different people could, by choice, avail themselves differentially of the services, and for that matter, could benefit differentially from given services.

Indeed, the combination of these factors is evident in the results presented here. The first equation shows merely a negative correlation between child weight and nutritional supplementation. This is consistent with either or a combination of the following hypotheses. First, the village where children received nutritional supplementation could have had children with a lower average weight, possibly because of nutritional reasons as well as the children's age and sex composition. Second, higher caste and better caring mothers, who have heavier children, could have used the nutrition program more, and more efficiently, than others. Once we control for these possibilities, by adding the other variables to the estimated relationship, we get better estimates of the interventions’ impact in the short run as measured by child weight.

18/ Almost none of the major other field experiments, like those undertaken by INCAP, meets the requirements of experimental design. The same is true, for that matter, in the field of family planning (Cuca and Pierce, 1977). It appears that researchers attempted, for practical and ethical considerations, to approach a situation where they have "matching groups".

19/ Other possibilities not discussed here are also plausible.
This example can be useful in outlining a general (and pragmatic) approach as well as specific criteria for assessing statistical requirements for evaluation research. However, we must first agree on the following points: (a) on pure statistical grounds, (randomized) experimental designs based on large samples are superior to any other approach for "netting out" impact; (b) quantitative evaluation of programs must be, however, socially efficient; and (c) to meet the efficiency requirement, we must always weigh the value of the additional (marginal) gain in statistical evidence and accuracy versus the marginal costs involved. The last point is a general efficiency criterion that may dictate the use of experimental design, econometrics, and a combination of the two. 20/

Returning to the Narangwal example with the above points in mind, we have to address, first, what it represents. And, second, what do the results mean in terms of the relative efficiency of social experiments. On the first question, in so far as the approach taken in Narangwal is representative of most field experiments, it implies what economists call a "revealed preference": the best that could and, probably, can be done, given objectives and constraints. This does not mean that we cannot have better field experiments. It merely puts a question mark on their economic and political feasibility.

On the second question, the results obtained by means of statistical control are most likely biased, or lack "Internal Validity", at least because of the nonzero correlations between any two of the right-hand variables. However, these biases do not appear serious, and the results are fairly

20/ The combined use of econometrics and experimentation is emerging in economic studies. For example, the New Jersey Graduated Work Incentive Experiment", see U.S. Department of HEW (1973).
"reasonable." That is, it is highly probable that a full fledged (and costlier) experiment might not improve substantially the results. While this is a testable proposition, which warrants an experiment, the author is willing to conclude that to determine the impacts of the Narangwal project, the mix of "semi experimentation" and a subsequent econometric approach prove sufficient.

To outline an econometrician's general approach to data requirements for impact measurement, let us consider three interventions aimed at increasing caloric consumption: (a) a national food subsidy program to reduce the prices of cereals to all consumers, (b) a national food stamp program for everyone, and (c) a national food stamp program with randomly selected participants. Assume that the three programs will give participants identical price reductions. Program (c) can be evaluated simply and unambiguously, just by collecting caloric consumption data, after the fact, if we are content merely to answer two questions: Did the program increase caloric intakes? If so, how much per family on the average? The answers to these questions could be unequivocal, assuming we gather accurate data and we draw a large enough sample. But this approach would tell us virtually nothing about why some families respond to the program more or less than the average or in the case of program failure, why there should

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21/ The results indicate that using the short term outcome, child weight, nutritional supplementation had a measurable effect. Due to this particular intervention child weight in the sample used here increased by .77 kg., or 9.7%, at the sample mean. The highest zero-order correlation between two of the right-hand variables was .34. It should be noted that a more careful econometric treatment of the data presented here, may improve the results.

22/ Other differences between the programs are ignored. For a discussion of the different implications of programs, like (a) and (b), see Reutlinger and Selowsky (1976).
have been no significant impact. In more general terms, the experimental program (c) lacks "external validity". Moreover, apart from cost considerations, the administration of this program poses social and other hazards that may jeopardize the validity of the experiment.

Programs like (a) and (b) are more realistic and common than an experimental program like (c). Taking this reality as a constraint, we stress how one can measure impact using an econometrician’s perspective. Although programs (a) and (b) cause identical reductions in prices, there is a fundamental difference between them. Under the first program, households can buy the cereals only at the subsidized price. Under the second, households can exercise choice in buying cereals with or without food stamps, and they can even sell the stamps. 23/ This difference between the programs is also of key statistical significance. For the first program, all we know is that it started at some particular point in time and existed to a subsequent point. But for the second, additional important information is potentially available: whether or not, and possibly to what extent, a household used the program stamps for buying cereals.

To determine impact in the first instance, panel data are necessary. Preprogram, or "baseline" data, should include the variables believed, on theoretical and other grounds, to affect the outcome. These data serve to estimate the basic relationship between, say, caloric consumption, and its non-program determinants. Once this relationship is estimated, we may also be

23/ Although the food stamps are available to all, one must consider the time and trouble of getting them. For example, a mother with five small children, living at a distance from where food stamps are sold, may find it less attractive to obtain those stamps than a mother of five grown children living nearby that selling station, ceteris paribus. See Annex, Section C.
able to estimate the added program effect. 24/ For example, suppose the food subsidy is introduced in an agricultural economy and reduces prices of cereals. But if farmers' incomes fall during the program period because of bad weather, for example, the farmers may consume less cereals even at the lower prices. Hence, such a fall in incomes may offset any positive effect of the program. Therefore, by accounting for changes in incomes (or even weather conditions), one can estimate program impact, provided no event significantly affecting the program outcome occurs simultaneously with the program in a way to confound our inferences.

In the second instance, the national food stamp program (Program B) baseline data may not be needed if we can analyze statistically the variance in households' utilization of the program. This possibility depends on the relationship between program utilization and other variables. At least from a behavioral standpoint, it is almost inconceivable that households of different characteristics will use, and even benefit from, the program identically. Therefore, it may be possible to study the determinants of program utilization as well as the interaction between program inputs and household characteristics. That is, what are the effects on utilization of various income and education levels? How do households of different levels of income and education benefit from given program inputs? If consistent answers should emerge, it might be relatively straight-forward to infer about the impact of the program itself. 25/

A program involving a target population identified by socioeconomic characteristics, geographical location, or nutrition status can be regarded as

24/ See Annex, Section D.

25/ See Annex, Section C.
a special case. Such a program differs from those already discussed in that it is for a population identified by one or more of the nonprogram variables affecting the outcome. Hence, there is a built-in correlation between the measure of participation in the program and some other determinant(s) of the outcome. Most policy programs fall into this category. Impact measurement in such cases must be confined to the target population and involves essentially identical considerations to those relating to the nonexperimental programs discussed above. When within the target population, there is no (or not enough) variation in the program (input or utilization) variable, panel data will be required to measure impact. For example, impact measurement in a particular intervention village of the Narangwal project is not possible if all that is known is that particular village was exposed to that intervention. However, when there is sufficient variance in the program variable, within the target population, this variance can be used to measure impact. For example, the U.S. Food Stamp Program is designed for low-income families. Within that population, however, some families do not use the program at all and other families use the program to different degrees. This variation may permit estimating program impact.

Within the perspective taken in the preceding paragraphs, an experimental approach is warranted when it is less costly, or when there is reason to believe that, given the available statistical techniques, panel data are insufficient to measure impact. Two basic situations may undermine the use of panel data. The first is when the underlying structural relationships are believed to change over time. Such a change may prohibit estimating the added effect of the program. The second situation is one where there is reason be believe that one or more of the nonprogram determinants
of the outcome will change during the monitoring period in a way that will confound any inference. Hence, experimental design may be used as a precautionary measure against foreseen and unforeseen circumstances. However, taking the view that social experiments are costly, financially and otherwise, we must look for ways (a) to reduce their costs and (b) to maximize their usefulness.

To reduce costs, we have to protect the measurement procedure against uncertainty rather than against ignorance because ignorance can be unnecessarily more costly. Uncertainty is defined here as a situation when one has good reason to believe that some known non-program determinants of the outcome may change significantly during the program and potentially have a confounding effect. Ignorance is defined here as a situation when one does not know the determinants of the outcome variable. In this case, one may wish to have an experiment by a random assignment of the intervention to "protect" the evaluation from all possible confounding effects. Unfortunately, this may be nearly impossible and unnecessarily costly.

When considering the use of experimental design, the economic theory of the household and econometrics may help to reduce the "costs of ignorance" by suggesting which nonprogram variables might confound the evaluation and need to be controlled for. Alternatively, prior considerations should help to stratify and reduce the evaluation sample. 26/ The first step is to understand, possibly by some modelling, the behavioral and other determinants of a particular outcome. This effort, should be coupled with anthropological observations as well as a pilot survey. Once key non-program determinants

26/ This approach comes close to a "quasi experimental" design.
of the outcome have been identified, the number of variables for which the experiment has to be controlled may become manageable. The second step is to protect against uncertainty by limiting the control variables to those that may change during the life of the program. This step may further reduce the cost of the evaluation. Obviously, in reality the researcher confronts both uncertainty as well as ignorance. However, given the costs of evaluation research based on experimental design, the benefits from careful a priori modelling may be substantial.

When experimental design is selected, an econometric approach to the data can help maximize its usefulness. As already mentioned, from a statistical viewpoint, impact measurement in a well-designed experiment can be satisfied just by comparing the means of the outcome variables between the treatment group(s) and the control group(s). This approach, however, reduces the acceptability and the universality of the results and has serious shortcomings in experimental contexts where policies are tested for future and wider applications. To increase the generality, or "external validity", of an evaluation based on experimental design, the researcher should include other variables or controls that affect the outcome measure and apply econometric techniques to the data. 27/ Thereby, one can test some basic behavioral hypotheses, and perhaps show explicitly how a program relates to its environment.

VI. CONCLUSION

The basic postulate of this paper is that although health related interventions aim to increase household and individual welfare, they may

27/ See Annex, Section E.
be inconsistent with the household’s and individual’s own objectives and opportunities. Where households can exercise choices vis-a-vis the program, studying these objectives and opportunities and relating them to particular interventions must be the first steps in understanding and predicting a program’s potential impact. While analyzing household behavior is no mean task, it is nevertheless essential. By relating the concept of expected earnings to age-specific productivity, morbidity, and mortality probabilities, the economic theory of the household offers a conceptual framework in which to integrate relevant aspects of household behavior with the basic outcomes of health programs. Thereby, this theory can suggest testable hypotheses concerning program impact, and can help to identify various outcome measures for programs.

Econometrics stresses the empirical aspects of the causal behavioral relationships outlined by the economic theory and, given the social and financial costs of social experiments, can provide a useful and efficient framework for measuring program impact. An econometric approach can, at times, substitute for experimental design, and at other times, complement it, depending on financial and other costs as well as statistical considerations. When experimental design is warranted on statistical grounds because statistical control techniques are insufficient to measure impact, the economic theory of the household may help to minimize the cost of the experiment by suggesting which variables should be controlled for. Furthermore, once data from experimental design are available, the use of an econometric approach to explore those data can increase the credibility and generality of the results.
Thus, the combination of theoretical and other \textit{a priori} considerations with econometric estimation techniques and experiments can prove the most efficient way to measure program impact.
ANNEX

A. A simple economic relationship—say between a household's caloric consumption ($Y$) and a vector $X$ of household characteristics including, for example, household income—can be stated by

$$Y = f (X).$$  \hspace{1cm} (1)

This is an "exact" relationship in that it does not account for random elements in human behavior. It is also a partial relationship in the sense that it may "ignore" other lines of causality; a more comprehensive underlying model may also deal, for example, with the effects of caloric consumption on household income. Given a relationship like (1) economic theory is usually concerned with the nature of the changes in $Y$ due to changes in $X$, within a relevant range.

B. Assuming that $X$ represents just income and that $Y$ and $X$ are observable, the econometric equivalent of relationship (1) can, for example, be

$$\log_e (Y_i) = a + b \log_e (X_i) + v_i, \hspace{1cm} (i = 1 ---- n),$$  \hspace{1cm} (2)

where the index $i$ denotes one of $n$ observations, and $v_i$ is a disturbance, or error, term which is usually assumed to be random, to have a normal distribution with an expected value of zero and a constant variance, and to be uncorrelated with other independent (right-hand) variables and with similar disturbance terms across observations. The term "$a" is a shift parameter or "intercept", indicating levels of caloric consumption that are independent of $X$ and $v$. The term "$b" indicates, in this case, the change in $\log (Y)$ due to a unit change in $\log (X)$. Alternatively, "$b" indicates the percentage change in $Y$ due to a given percentage change in $X$, or the "elasticity" of $Y$ with
respect to $X$. Assuming, no errors in the specification of (2), obtaining an unbiased estimate of "$b$" is possible when $X_1$ is not correlated with $v_i$.  

C. The following underlying structure is generally assumed here for discussing measurement of the impact of a program. First, in a given environment where such a program has been implemented, an outcome ($Y$) is a function of the household's utilization of the program ($U$) and particular subset of household characteristics ($X_1$),

$$Y = f(U, X_1),$$

(3)

Where $U$ and $X_1$ may be dependent; that is, for biological and behavioral reasons, particular levels of program utilization may have different effects in households of different characteristics; $\frac{\partial^2 Y}{\partial U \partial X_1} \neq 0$.

Second, the utilization of the program is a function of the supply of program inputs ($P$) indicating the intensity by which the program reaches different households, and of a subset of household characteristics ($X_2$); 

$$U = g(P, X_2),$$

(4)

where $X_1$ and $X_2$ are not necessarily mutually exclusive subsets at least conceptually.

This structure can be reduced, by substituting (4) into (3), to 

$$Y = f[g(P, X_2), X_1] = h(P, X_3),$$

(5)

where $X_3$ is a set of household characteristics combining $X_1$ and $X_2$.

---

28/ Some other properties of this double-logarithmic function are discussed in section IV in the text.

29/ When $X_1$ denotes a vector of variables, any two of those variables should also be uncorrelated.
Estimating relationship (5) may be sufficient to measure program impact. This alone is insufficient, however, for understanding this impact and thus for evaluating the program. In particular, such a relationship does not reveal whether variations in impact of given program inputs are due to different levels of utilization, to differences in impact of given inputs on households of different characteristics, or both. The problem can be stated as the researcher's inability to test various hypotheses concerning X₁ and X₂. This problem can be solved by estimating relationship (4), which may be crucial for designing a cost effective program because this relationship identifies the users and levels of utilization of a particular program. 30/

D. A simple approach to panel data pertaining to a program is to pool baseline and program data, and estimate an explicit function of

\[ Y_{it} = g(t, X_{it}, P_i, v_{it}), \quad (i = 1,...,n, \quad t = 0,1) \]  

(6)

where: \( t \) represents the time period a particular observation was obtained; 31/ \( Y_{io} \) and \( X_{io} \) represent baseline or preintervention values; \( Y_{il} = Y_{io} + dY_i \) and \( X_{il} = X_{io} + dX_i \) represent post-intervention values; and \( P_i \) indicates program inputs. 32/ Assuming, without much loss of generality, that the relationship between caloric intakes and the other variables is linear within the relevant

---

30/ Although conceptually different, \( P \) and \( U \) are often interchanged and relationship (3) is estimated instead of (5). In such a case the researcher should be aware that he may attribute undue failure or success to program services. That is, the program may succeed or fail because of particular household characteristics; some program may succeed in one setting and fail in another.

31/ \( t = 0 \) for preintervention and \( t = 1 \) for during or post intervention. This illustration can be generalized to include more than two time periods.

32/ Alternative, \( P_i = 1 \) for participation in, or prevalence of, the program and \( P_i = 0 \) otherwise.
range, and that changes in income bring about identical changes in consumption over time, one can estimate this equation as illustrated in Figure 2.

\[ y_{it} = a + b_1 x_{it} + b_2 t + b_3 p_i + b_4 p_i x_{it} + v_{it}; \]  

(7)

First, no interaction is assumed between the program (inputs) and household characteristics, \( b_4 = 0 \); that is, program impact is independent of the level of household income. Curve (i) in Figure 2 indicates the general relationship between income and caloric consumption, or the pre-program relationship. Curve (ii) indicates the added effect of time, measured by \( b_2 \) that is presumed to be independent of the other effects. For example, it can indicate that the effect of child growth on caloric consumption. Curve (iii) depicts the added effect of the intervention measured by \( b_3 \).

Second, we assume that \( Y \) measures child weight, for given age and sex, and that there is interaction between \( X \) and \( P \), or \( b_4 \neq 0 \); that is, for a given level of program utilization the impact of the program, measured by gain in child weight, differs across households of different levels of income. For example, children of better-to-do families may be less exposed to diarrhea than children of poor families. Consequently, the children of relatively richer families may gain more in weight from a given school feeding program, ceteris paribus. This particular effect is depicted by line (IV) of the figure.

Estimating program impact by using panel data is not feasible when "\( p \)" and "\( t \)" are statistically indistinguishable; that is, when some other events potentially affecting the outcome concur with the program.
(iv) \( Y = a + b_2 t + b_3 P + (b_1 + b_4) X \)

(iii) \( Y = a + b_2 t + b_3 P + b_1 X \)

(ii) \( Y = a + b_2 t + b_1 X \)

(i) \( Y = a + b_1 X \)

Figure 2
E. Suppose that the sample underlying relationship (7) and Figure 2, is drawn from an experimental design which consists of \( n \) observations, \( m \) in the control group and \( (n-m) \) in the experimental or program group. That is, \( P_i = 0 \) for \( i = 1, \ldots, m \), and \( P_i = 1 \) for \( i = m+1, \ldots, n \). To measure and test for intervention impact, the experimental design advocates measuring the difference \( dY_e - dY_c \) and testing for its statistical significance, where \( dY_c = \frac{\sum_{i=1}^{m} Y_i}{m} \) is the mean change in the outcome in the control group, and \( dY_e \) is the corresponding statistic for the experimental group. This difference will reflect correctly and fully program impact, only if the experimental design achieves complete randomization (or perfect matching) of all other potential effects; that is \( P_i \) does not correlate with any other determinant of the observed outcome.

To achieve the same goal an econometrician would estimate relationship (7), by either

\[
Y_{it} = G + b_3 P_i + \nu_{it}
\]

(8)
or

\[
dY_i = E + b_3 P_i + \omega_i
\]

(9)

where \( \omega_i \) is a disturbance term similar to \( \nu_i \). It can be shown that

\[
dY_e - dY_c = b_3 = b_3 = b_3
\]

(10)

when \( P_i \) does not correlate with any of the right-hand variables in equation (7). Hence, while econometric and experimental approaches should yield identical statistical results because of the underlying statistical rationale, the econometric approach specifies particular structural and functional relationships and tries to account for all relevant determinants of the outcome.
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