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CENTER DISCUSSION PAPER NO. 494

THE IMPACT OF IMPROVED NUTRITION ON LABOR PRODUCTIVITY  
AND HUMAN RESOURCE DEVELOPMENT: AN ECONOMIC PERSPECTIVE

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### Abstract

Isolating the effects of improved nutrition on labor productivity and on health, education and other human capital investments is proving to be very difficult. A major problem has been that statistical analysis, both of experimental and survey data, consists of correlations between variables which economic analysis suggests are influenced by household decisions. Examples include correlations between measured labor productivity and current nutrient intakes. Since such associations may result from an income-calorie consumption relationship, causality cannot be inferred. With sufficiently rich economic data it is sometimes possible to infer causality using instrumental variables techniques. A very small number of studies have attempted to this, with promising results.

This paper reviews the methodologies which have been used in the empirical literature, explains why the conclusions drawn from these methodologies don't always make sense when the economic behavior of individuals and households are considered, and points out corrective measures traditionally used by economists that have only begun to be used to analyze nutrition-productivity-health interactions.

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John Strauss, "The Impact of Improved Nutrition on Labor Productivity and Human Resource Development: An Economic Perspective"

1. Introduction

Isolating the effects of improved nutrition on labor productivity and on health, education and other human capital investments is proving to be very difficult. The research done to date has concentrated more on health-nutrition linkages, and has been carried forward mostly by nutritionists and medical doctors, however an increasing number of economists have become involved. Two types of evidence have been presented: experimental (or quasi-experimental) and epidemiological. The experimental evidence usually examines the effects of diet supplementation programs on such variables as labor productivity, physical growth or morbidity. Ex-ante, ex-post comparisons are made, sometimes showing an effect, sometimes not. While some of the conclusions drawn seem reasonable, many are overdrawn: either because of a faulty design which is not corrected for by statistical analysis, or because the analysis itself is faulty even though the design may be adequate. Almost all statistical analyses of non-experimental data, as well as some analyses of experimental data, consist of correlations between variables which economic analysis suggests are chosen, or at least influenced, by households. Examples include correlations between measured labor productivity and current nutrient intakes. Since nutrient intakes are influenced by many factors, for instance income, which are also related to productivity, these correlations shed little or no light on causality. Unfortunately they have been widely interpreted as causal in the literature.

It is sometimes possible, provided certain data are available, to infer causality using appropriate statistical techniques. A very small number of studies have attempted to do just this, with promising results. When combined with the very few reliable experimental studies they indicate that current

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nutrient intakes, particularly calories and iron, as well as body size (in terms of weight) can have a positive impact on work productivity, and even when workers are above starvation intake levels.

The main purpose of this paper is to review the methodologies which have been used in the empirical literature, explain why the conclusions drawn from these methodologies don't always make sense when the economic behavior of individuals and households are considered, and point out corrective measures traditionally used by economists that have only begun to be used to analyze nutrition-productivity-health interactions. Questions of data collection strategies are also addressed. The paper treats separately nutrition impacts on productivity from those on health or other human capital. This is done for convenience only since the methodological issues of analysis are identical. Exactly what those issues are is discussed in the following section.

## 2. Nutrition, Health and Productivity Interactions in a Household Model

### A. Nature of Household Decisions in Producing Nutrition and Health Outcomes

Households not only consume goods and leisure but produce and consume non-marketed commodities as well. Among these are nutritionally related outcomes such as anthropometric measurements (or changes in those measurements) and health outcomes such as infant birthweight or individual morbidity. These outcomes are "produced" by inputs, some of which are chosen by the households. In the case of adult standardized weight (or changes in weight) the outcome, change in weight, reflects an energy imbalance. The degree of energy imbalance in turn depends upon nutrient intake, infection, and activity levels by type in addition to variables affecting basal metabolic rate such as age, sex and weight. Individual nutrient intake, activity levels, and infection incidence result from current household decisions (infection being produced by such

inputs as nutrient intake, water consumption -- including a quality dimension, activity levels, and medical treatment).

In turn, current nutrient intake, stature (height and weight-for-height), and health may affect worker market or farm-productivity. That is holding labor hours and non-labor inputs constant, output may vary as current nutrient intakes, body size (weight or weight-for-height), and worker health vary through the mechanism of maximal oxygen consumption ( $VO_2^{MAX}$ ), which is associated with greater work efficiency and endurance on standardized tests (see Spurr, 1983, for example). If the market recognizes a nutrition-productivity effect then better nutrition may also result in higher market earnings. This might come about by being paid more for a given time unit of work or by being able to work at particularly taxing, and well rewarded, activities, or both.

Higher caloric intake may also raise non-marketed household production in addition to farm or market activities. This point has been made in the nutrition literature, for instance by Viteri (1974), who studies two groups of Guatemalan agricultural workers, one of which had received nutritional supplementation for the previous three years. Viteri records that the unsupplemented group was largely inactive after working hours while the supplemented group remained active in household activities. If this was indeed a result of increased nutrient intake, the benefits from higher intakes would be understated by only the measuring effects on work productivity and earnings.

An economic model will predict that household members try to equate the marginal benefits (measured in a money metric or in satisfaction) between different activities. While various market imperfections may prevent marginal benefits from being completely equated, an increase in nutritional intake should lead individuals to allocate their time to those activities with the

highest marginal returns. In consequence, the pattern of time use in different activities will be directly affected by nutritional intakes as well as health and nutrition outcomes.

B. Implications of Household Decisions for Estimating the Impact of  
Nutrition on Productivity

Of very major significance is the implication from economic analysis that individual food consumption (thus nutrient availability), other health inputs and time allocation all result from household choices. Among the factors which will affect these outcomes are unobserved variables, such as farm managerial ability or land quality, as well as observed variables such as prices. This greatly complicates any potential interpretation of empirical correlations from non-experimental data between measures of worker productivity or labor market earnings and current nutritional intakes or stature. In particular causality running from better nutrition to measured worker productivity should not necessarily be inferred from observed positive correlations between the two measures since both are being "caused" by other observed and unobserved variables. For instance, sugarcane cutters who are more able cutters should have higher measured productivity than less able cutters, holding constant observable factors which may affect productivity, such as height and age. Yet caloric intakes may well also be higher for the more able cutter group if they earn more income. Thus a positive correlation between caloric intake and sugarcane cut per day may simply reveal an income-caloric consumption curve, not necessarily a nutrition-productivity effect.

Caloric intake is a flow variable. Nutritional outcomes such as weight or height are stock variables in that they represent the accumulation of past flows. It might be thought that using lagged values of stock variables such as weight-for-height might avoid the problem of simultaneous determination of

variables. However this is not likely to be the case. Take the case of weight (or weight-for-height) and productivity. Clearly current weight changes and productivity are both affected by current household choices. Moreover past weight changes may be correlated with current "random" errors, which affect both current weight changes and productivity, provided that these "errors" represent in part individual and household specific variables which persist over time and which are unobserved to the analyst but known to the household or individual. Examples again include farm management ability, land quality, or inherent (genetic) healthiness. Such variables may be expected to affect the same household or person over a period of time, and to have impact on all household choice variables. For instance, better farmers from a low income community may show both higher labor productivity and weight-for-height than less able farmers. Hence a positive empirical correlation may be entirely spurious.

The case for treating height-for-age as being uncorrelated with unobserved factors which affect current decisions is stronger, especially to the extent that adult heights are largely determined by parental investments made when the current adults were children. Here the argument is that unobserved factors which the parents took into account may be uncorrelated, or only weakly correlated, with unobserved factors which the children as adults take into account. Counterexamples would result from factors specific to the individual which persisted from childhood to adulthood, for example inherent "healthiness". How common such very long-lived factors are has to be determined from empirical evidence.

Given the foregoing critique, it is of interest to discern the direction and magnitude of the statistical bias (inconsistency) incurred when using statistical methods of analysis, such as ordinary least squares, which do not



correct for the simultaneity of variables used in regression analysis. In general this cannot be done, however in some very simple special cases it can be. In particular, if there is only one explanatory variable which is endogenous, then the direction of the bias will depend on the sign of the correlation between the endogenous explanatory variable and the unobserved disturbance term. The magnitude of the bias will depend upon the strength of that correlation (see the appendix). For instance, suppose measured labor productivity were to be regressed on current caloric intake and an exogenous variable, age of the worker. It is quite likely that unobserved characteristics of the worker, such as "ability" are correlated through income with current caloric intake. This would lead to an upward bias in the estimated coefficient of current caloric intake. Indeed it would be possible that a positive coefficient might be found even when no effect existed of caloric intake on productivity, simply because of the positive income-calorie intake relationship, reflected in a positive correlation between the calorie variable and the unobserved error term ("ability") in the productivity equation. Unfortunately the strength of the income-calorie intake correlation is likely to be strongest for very low income households, who have members consuming at low intake levels. Yet it is precisely for such individuals that the nutrition-productivity relationship is hypothesized to be the strongest. Thus when using data for such individuals the statistical bias is likely to be the most.

With more than one endogenous explanatory variable the direction of the bias is more difficult to judge because it will also depend on the correlations between the second endogenous variable and the unobserved disturbance, and between the two observed endogenous variables (see the appendix). Useful generalizations are thus difficult to generate because they depend on what

other endogenous variables are used. Nevertheless biases may still be expected to be present, thus results based on such regressions are suspect if they are used to support claims of causality.

#### C. Consistent Estimation of a Nutrition-Productivity Effect

For a nutrition-productivity effect to be consistently estimable, from nonexperimental data, data must be available on variables (instruments) which influence household choices, but have no direct influence on labor productivity. One class of variables which prove to be extremely useful in this regard are prices which a household faces: prices of foods, of nonfoods, of non-labor farm inputs (for farm households), and of health inputs. Distance to various program centers will be among the price variables for program service inputs. However, to the extent that migration is prevalent and that program service availability helps to determine whether and where to migrate, then distance to community services will also result from household choices and thus be an inappropriate set of instrumental variables (see Rosenzweig and Wolpin, 1984). Prices faced in the market will in general be independent of household choices. Other variables which are outside of the household's control and which affect current behavior, but not directly productivity, will be candidate instrumental variables. Among these may be characteristics of the parents, such as education, job history, and height. Care has to be taken with stock-like household level variables, such as assets, because although they may be predetermined they may well be correlated with unobserved individual and household characteristics which persist over time.

Having data on prices and other community variables, effects of these variables can be traced onto current intake and other nutritional variables which would vary in consequence, without productivity directly varying. By then examining statistically how production varies when nutrition or health outcomes

(as well as other household choice variables) change as a result of variation in exogenous factors it may be possible to gain some weight on the potential effects of an (imaginary) exogenous change in these choice variables on productivity. This is, of course, simply the method of instrumental variables.

In order to obtain reasonably precise estimates from this method it is necessary to have larger samples than is usual in the nutrition literature. In addition one needs variation in the values of instrumental variables. Since commodity prices vary only over time or over large regions, data should ideally span both. Thus panel data are potentially quite useful. In a cross section, data will have to be over a large enough area to insure real price variation (that is for the same characteristics of a commodity and for identical time periods).

#### D. Implications of Household Decisions for Experimental Design

Analysis of experimental data may also be subject to simultaneity bias if explanatory variables are used which have not been controlled for experimentally, and which are endogenous to household decisionmaking. Even without this problem, an issue of analysis, individual and household choices can contaminate the data through attrition or refusal to participate in the first place. For example, if in a diet supplementation experiment it is the workers with lowest caloric intakes who drop out and if the impact on productivity declines drastically with higher intakes, as it is thought to, then only a very weak positive impact may be measured. This problem is appreciated in the experimental literature, though awareness does not always prevent occurrence. For example Popkin's (1978) study of iron supplementation on road construction worker productivity in the Bicol region of the Philippines had to be discontinued because of an enormous exodus of workers (119 out of 157) apparently caused by a change in the payment system during the

experiments. Even if sample attrition is not a problem, non-random assignment to control and treatment groups may be. Several of the experimental studies summarized below suffer from this problem.

Even experiments which are well designed and do not suffer from attrition bias or simultaneity bias in the data analysis may have difficulty in properly measuring the impacts of nutrition on productivity, again because of household choices. Most experiments attempt to measure the impact of diet supplementation (of calories or of iron) on average worker productivity. However typically the entire diet is not controlled, but only the portion eaten on the job. Since the supplement may substitute for food consumption at home the total change in nutrient intake is apt to be considerably less than the amount given in the supplement. Strong evidence of such substitution is found in numerous studies, for instance in Akin, Guilkey and Popkin (1983). This point is also understood in the nutrition literature, with attempts usually being made to measure food consumption of the individual at home (by 24 hour recalls) as well as at work. What is less well appreciated is that substitution may occur between household members as well as for the member in the experiment. In particular both food consumption and activity levels of other household members will likely be reallocated so as to re-equate the marginal returns of food consumption, time use, health and other commodities across household members. Change in household welfare will depend upon these reallocations, which have not been measured in any of the experiments to date. The consequence of following only the individual in the experiment rather than the entire household is that the benefits of supplementation are likely to be understated, though by how much is difficult to judge.

### 3. The Appropriate Concept of Productivity and Difficulties in Measuring It

The question of observability of productivity measures is an important one. The appropriate concept here is marginal, not average, productivity. In the case of market work, under standard economic assumptions wages will reflect marginal productivity. Since individual wages can be observed, carefully examined, they might shed light on the existence of a nutrition-productivity effect. It is possible, however, for nutrition to raise labor productivity without affecting market wages. This might occur if it were costly, or difficult, for employers to monitor the food consumption of individual workers. If body-size, not current intakes, is responsible for the enhanced productivity this should be less likely since body size can be observed easily. For nonmarket family labor, for which no direct remuneration is provided, marginal productivity is not observable but must be inferred indirectly. This poses difficulties in general, requiring knowledge of the technical relationship between inputs and outputs, that is information about the production function. For this reason most nutritionists' studies of nutrition-productivity relationships have used data from industries in which outputs of individual laborers can seemingly be directly observed. Sugarcane cutters and dirt diggers on road construction crews have been among the most intensively studied groups. Even in these cases there are non-labor inputs into production which need to be measured in order to estimate the marginal productivity of increased current nutrient intake or greater weight-for-height. For example different sugarcane fields may have differing qualities or have received different levels of preharvest inputs. Unless laborers are randomly assigned to fields the effect of working on different fields needs to be accounted for when analyzing the data, whether it is experimental or nonexperimental. This issue has not always been addressed in the nutritionists' literature. Exceptions are some of

the regressions reported by Immink and Viteri (1981 a, b) Wolgemuth et. al. (1982), and Popkin (1978). Immink and Viteri control for field conditions in explaining the response of average productivity of sugarcane cutters to direct supplementation. Wolgemuth et. al holds constant road assignment in looking at road construction workers productivity response to diet supplementation, and Popkin holds constant rain conditions when analyzing road construction workers response to iron supplementation.

#### 4. A Review of Empirical Evidence on Nutrition-Productivity Linkages

##### A. Overview

Reliable empirical evidence on the existence of a nutrition-productivity relationship is not abundant, particularly for individuals above starvation or semi-starvation levels of caloric intake. What little useful evidence does exist suggests some positive impact of increased caloric intake, and possibly weight or weight-for-height, on market or farm labor productivity for such individuals who are at what might be considered low, but certainly not starvation, levels of intake. Iron deficiency also seems to have some negative impact on productivity, even without deficiencies in caloric intake. However it is necessary to be rather cautious in the claims made for this evidence since it is not voluminous and there are still many issues which are unexplored.

A number of studies, both experimental and non-experimental do not find supporting evidence of a nutrition-productivity link. However, as explained below most of these studies suffer from some of the difficulties discussed in section 2. On the other hand many analyses do show positive empirical correlations between measures of worker productivity and nutrition related variables. In light of the discussion in section 2, however, not much should be made of these either.

The evidence seems much more substantive at starvation or semi-starvation levels. The experiments of Keys et al. (1950) at the University of Minnesota show that activity levels drop precipitously when males are subjected to dramatic decreases in caloric intake from moderate intakes (3500 calories daily) to extremely low ones (1500 calories daily). While basal metabolic rates dropped, they did not do so sufficiently to offset the fall in nutrient intake. These experiments controlled the total diet of the subjects, and randomly assigned them to treatment groups. Thus they would appear to be free of many of the problems discussed earlier. Other starvation experiments may also be free, or relatively so, of confounding effects (Spurr, 1983, contains a very useful survey).

One issue which has been raised elsewhere in the nutrition literature, e.g., Sukhatme and Margen (1982), is whether over a more moderate range of intake changes, basal metabolic rates may adjust enough to avoid having to change activity levels by much in order to reequilibrate energy intake with energy expenditure. If true this would imply a very weak or nonexistent nutrition-productivity relationship at higher levels of caloric intake. Indeed it is argued that this hypothesis suggests a threshold of rather low intake above which it makes no difference to productivity. Then the issue becomes how low such a threshold might be. None of the evidence cited below directly tests for such a threshold, although some of it does test for a continuously declining impact of calories on productivity as intake rises. In the limit, of course, it is very difficult to distinguish between a discontinuous threshold and a sufficiently nonlinear continuous effect.<sup>1</sup>

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<sup>1</sup>The direct empirical evidence on Sukhatme-Margen hypothesis is much too scant as yet to be conclusive, involving as it does only a handful of studies with incredibly small samples (15 persons for example). In addition, there is even less evidence on the speed of adjustment. If the transition to a new equilibrium is slow enough, then productivity losses during the transition period could be important.

## B. Specific Studies

Wolgemuth et al. (1982) compare gains in productivity in earth moved per hour between a group of workers whose diet was supplemented by 1000 Kcal/day and workers with only 200 Kcal/day supplementation. The study is unusually careful in randomizing a number of relevant characteristics between groups. For instance the daily attendance record for the first month of the study and initial productivity measurements were among the variables which were stratified before random assignment to groups. Randomizing over the first variable should have helped to avoid selective dropping out of the sample, while the second variable would control for many unobservable individual effects. They also take care to measure food consumption at home, finding a net increase of 500 kcal/day for the highly supplemented group, and no net change for the low-level supplemented group. They then compare mean gains in productivity between highly and weakly supplemented groups, finding a 12.5% gain in productivity by the highly supplemented group (more for the low calorie supplementation group), which was statistically significant at about the .075 level. Unfortunately this result must be qualified because only 47 individuals out of the 224 initially in the study are used, with no explanation provided. This raises the question of the representativeness of those workers included in this comparison.

Basta et al. (1979) compare gains in productivity of adult male tree tappers and weeders working on rubber plantations in Indonesia, between workers getting an iron supplement and those receiving a placebo. Workers were randomly assigned to treatment groups. Basta and his colleagues find an increase in productivity among all groups, potentially related to an incentive



wage scheme linked to participation in the experiment, but an especially large increase for anemic workers who received iron supplements. Some effort was made to limit the productivity comparisons to workers working on trees of similar quality. However, this matching of workers plus other, unstated, reasons resulted in only half of the sample of tree tappers being used in the comparison. The impact of this reduced sample, only 77 workers, on the results is unclear.

In a major diet supplementation study done at INCAP, Immink and Viteri (1981 a, b) compare the gains in productivity between sugarcane cutters in one Guatemalan village receiving a high energy supplementation and cutters living in a village who received a low energy supplementation. Since all workers in each village received the identical supplement there was not randomization of assignment to treatment groups. Initial measurements indicate similarity of workers between the two villages in such dimensions as caloric intake and cutting productivity, though there may have been differences in field quality or non-labor inputs applied between the two villages. The study lasted 28 months, the first 15 of which have been analyzed, which raises the question of differential sample attrition, perhaps because of migration or for other reasons. Caloric intake at home was measured by 24 hour recalls with the result that the workers receiving the high energy supplement were observed to increase their caloric intake over baseline levels, while the workers receiving the low supplement did not. In comparing changes of daily cane harvest by the two groups over time Immink and Viteri find that productivity of both groups rose during the supplementation period. They test differences between the two dummy variable coefficients, to see if the rise in the more highly supplemented group was significantly higher, but their tests are incorrect because of serial

correlation in the data which they measure but do not correct for. These comparisons are confounded by seasonal patterns in production associated with both villages. This variation is not completely captured by the analyses in these papers, although some attempt is made by running separate regressions for each of two seasons. When the sample is split any differences between the two supplementation dummy coefficients disappear, the major variation over time being captured by village level variables measuring days worked in the fields and mill capacity. Since the sugar-company regulates total labor used, the village days worked variable may be taken as exogenous to the worker.

A different type of time series comparison is that made by Kraut and Muller (1946). They report changes in productivity of different groups of German workers when daily food rations were increased. The workers were living in special camps, so their total diet was controlled. In the three cases reported of worker or plant level response, output per worker hour increased dramatically following an increase in food rations. This must be interpreted cautiously since it may represent a morale effect (Stiglitz, 1984) rather than a nutrition effect. Also no non-labor inputs or institutional changes were measured. It is interesting that worker weight generally remained unchanged, the increased caloric intake apparently being fully expended. This is consistent with findings of Viteri (1982). The one case when a short run weight loss was recorded was when a cigarette premium was offered to workers dumping debris out of railway cars for attaining a given level of productivity. Productivity did indeed jump, workers being willing to endure a loss (perhaps temporary) in weight.

The foregoing comparisons have comparatively fewer problems than most of the literature, since they do not look at correlations between two or more household choice variables and infer causality, for instance between current

productivity and current flows or stocks (past flows) of nutrition intakes. The published literature attempting to establish nutrition-productivity links is replete with just such regressions (or correlations). As an example, Wolgemuth et al. (1982) report a regression of gains in road construction worker productivity on total caloric intake from the supplement and days worked. The total calories variable has a positive coefficient which is weakly significant (t-statistic of 1.81 with 44 degrees of freedom) while days of labor supply has a negative and highly significant coefficient (t-statistic of -3.93). The authors imply that causation running from labor supply to productivity changes is driving the correlation. While this is certainly possible it is not the only plausible interpretation since labor supply can certainly be varied by households, and much recent empirical evidence indicates that labor supply does respond to prices (see for example Bardhan, 1984; Rosenzweig, 1980; Singh, Squire and Strauss, 1986). In this case, if work was paid by piece rate the diet supplement would raise earnings (provided it raised productivity). Labor supply might decline because of an income effect, negating some of the effect on earnings, and leading to a negative coefficient on days worked.

Wolgemuth et. al. also report a pure cross-section regression using the pre-supplementation data. The experimental nature of the data is thus not used in this regression, making it comparable to other analyses using non-experimental, cross-sectional data. Productivity measurements are regressed on a set of variables including arm circumference and hematological values. Likewise Popkin (1978) regresses daily productivity of road construction workers in Bicol, Philippines on hemoglobin levels. Baldwin and Weisbrod (1974) and Weisbrod and Helminiak (1977) regress daily and weekly earnings of plantation workers on St. Lucia on, among other things, dummy

variables indicating presence of parasitic infections such as schistosomiasis. These "explanatory" variables reflect current period and past period investments in nutrition and health as argued earlier in the paper (Baldwin and Weisbrod are aware of these concerns but do nothing to correct the problem). Even with estimates which are probably biased upwards, they find little, if any, effects of infections on earnings. Behrman, Wolfe and Blau (1985) separately regress male and female earnings of workers in Nicaragua on variables including one measuring the proportion of a protein standard satisfied by food consumption at the household level, and one measuring days of illness. They also estimate probit equations to explain the probability of working in the market, again using the nutrition and health variables. The measure of protein adequacy is found to have important positive effect on both earnings and the probability of working in the market, however the meaning is in doubt.

Immink and Viteri (1981 a, b, 1982) regress the change in sugarcane cut per day (and per hour) on daily energy intake in addition to variables controlling for field conditions and whether the worker was in the high supplementation group. The trouble with the energy variable is that it measures total daily intake, not calories from the supplement. Total intake is endogenous because of substitution of food at work for food at home. Even then they find that the calorie variable has a very low t-statistic, although the statistic is incorrect given the simultaneity problem. They also use energy intake in a regression trying to explain tonnage of cane cut per day using only the pre-supplementation data.

In an earlier study Viteri (1971, 1974) reports that time-motion studies of agricultural field work done by two groups of agricultural workers, one group having a higher caloric intake and having had a supplemented diet for

three years, shows that the higher intake group expended more energy per task, completing them in a shorter period of time, and also expending more energy on household activities. The trouble with this finding is that there is no information on inherent differences between the two groups. The groups were not formed randomly, indeed the supplemented group consisted of workers who were paid higher than average wages, had an adequate current caloric intake and worked on the same farm, apparently a better managed one. The second group by contrast was from one of the poorer areas of Guatemala, and had much lower caloric intakes. While the nutrition-productivity explanation is certainly possible it is by no means the only one. Different field conditions between the two areas might well have led to the difference in timing (though that wouldn't explain different energy expenditures) as might differences in ability or motivation (the samples were extremely small, 19 for the supplemented group and 20 for the unsupplemented group). Given that the higher productivity group had higher earnings it is not surprising that their caloric intake might be higher.

Studies relating body size to output are also plagued by the problem of simultaneously determined explanatory variables. Martorell and Arroyave (1984) cite six studies which calculate correlations between a measured productivity variable and weight, or weight-for-height. These are Davies (1973), Spurr et. al (1977), Immink et. al. (1982), Heywood (1974), Brooks et. al. (1979), and Satyanarayana et. al. (1977) (also see Rao, 1970). Typically a sample of workers is taken and productivity measurements made. The sample is then divided by level of productivity and group average anthropometric measurements taken and compared. Martorell and Arroyave conclude on the basis of these studies that body size, particularly weight or weight-for-height seems to be an important predictor of productivity, especially for demanding work tasks.

Since these coefficients are probably upwardly biased it is not clear what to make of them.

Two studies, Strauss (1984, forthcoming) and Deolalikar (1984), have attempted to account both for the endogeneity of explanatory variables subject to household choice and for non-labor inputs which affect productivity, in estimating the effects of higher current nutrient intake and stature on labor productivity in subsistence family farms. Strauss uses cross-section data on farm households in Sierra Leone, households practicing hoe agriculture, while Deolalikar uses household data from a semi-arid part of south India. Both find positive and statistically significant effects of nutrition related variables, even after accounting for their endogeneity. In Strauss' study current caloric intake is controlled for while in Deolalikar's case it is weight-for-height and height, with only the former having a significant coefficient. These studies are not only the first to attempt to control for input simultaneity, but also they seem to be the only studies other than Viteri's (1974) flawed analysis trying to measure the impact of better nutrition on productivity of family farm laborers, this despite the overwhelming importance of family semi-subsistence farms in developing country agriculture. Both studies use the same basic idea, estimating an agricultural production function while using instrumental variables to control statistically for endogenous inputs. Variables treated as endogenous include not only nutrient intakes and body size (at least weight-for-height) but also variable farm inputs such as hours of family and hired labor use.

The instruments used by Strauss fall into three categories: prices, farm assets and household size and age distribution, with prices and certain household characteristics, such as family size, being excluded from the farm production function. Deolalikar only uses farm asset, household size and age

distribution variables, not prices, as instruments. He also excludes from the production function some of the farm asset variables, such as the value of productive assets.

Since it is arguable that even quasi-fixed factors such as capital stock, land cultivated and family size are correlated with unobserved variables, such as land quality or management ability, they may be inappropriate instruments. Strauss examines the robustness of his estimates to dropping these variables, using only prices as instruments, finding his results to be reasonably robust to this specification. Deolalikar, on the other hand, finds that the impact of weight-for-height on agricultural output rises sevenfold when simultaneity is accounted for, compared to when it is not. Unfortunately the data Strauss uses are not ideal for testing the nutrition-productivity hypothesis. Data are only available for current nutrient availability at the household, not individual, level and no anthropometric measurements were taken, so the effect of body size cannot be separately estimated. The most which can be done under this circumstance is to make differing assumptions concerning how households distribute food among its members and examine the sensitivity of the results to these changes. Strauss does this, finding almost no changes in the results. In Deolalikar's study, by contrast, data are available for individual heights and weights and even individual level food consumption. The latter variable has not been used in the current version of the study but the former two have.

Strauss models current caloric availability as augmenting hours of family labor into "effective" hours of family labor. This is done by multiplying labor hours by a function which relates units of effective labor time to units of clock time. This function depends upon current nutrient intake at the individual level. Strauss finds a high degree of curvature in

this function (see Figure 1), it being approximately quadratic in the range observed in the Sierra Leone data. The estimated efficiency of an hour of work relative to a male consuming 3000 Kcal daily is estimated to be 60% for a male consuming 1500 Kcal per day, and 117% at a daily consumption 4500 Kcals. This efficiency function is rising up until 3750 Kcal per day, but only very gently after that, until it finally falls after 5200 Kcal. Thus it would appear, at least in this sample, that nutrition-productivity relationship exists even for individuals with relatively high levels (compared to starvation) of caloric intake.

Strauss estimates that output increases by nearly .5 percent for every 1 percent increase in calories consumed for low-income workers (who consume at 1500 Kcal daily). This figure is almost identical to the figure of .5 percent found by Wolgemuth et al. for the Kenyan road construction workers having an average daily intake of 2000 Kcals.

The potential economic importance of the nutrition-productivity relation is calculated to be high in the Sierra Leone data. The marginal product of a unit of a particular food can be shown to be a estimate of the proportion by which the shadow price of that food is less than the market price. Strauss puts bounds on this figure being between 20% and 40% for the representative household in the sample (having daily caloric availability per consumer equivalent of 3060 Kcal), rising to a very high 75% to nearly 100% for a very poor household (with daily per consumer equivalent availability of 1500 calories), and falling to between 15% to 18% for households with a daily per consumer equivalent availability of 4500 calories. While these figures are only meant to be illustrative of the order of magnitude potentially involved, given the crudeness of the data, they are nevertheless striking.



Deolalikar finds that raising the weight-for-height of an individual from 85% to 100% of Indian standards would raise the daily value of labor's marginal product by Rs 0.4 (rupees). This compares with an average daily agricultural wage of Rs 2.45. Deolalikar also estimates market earnings functions including both standardized weight-for-height and height as endogenous variables, and using similar instruments as in the production function equation. He again finds that weight-for-height but not height matters. Accounting for the endogeneity of weight-for-height raises its coefficient sevenfold. It may be that households with persons working on the market are poor and have lower anthropometric scores. That would tend to bias downwards the anthropometric coefficients (see the appendix), which is what Deolalikar finds. The marginal increment to earnings of a percentage increase in standardized weight-for-height is calculated to be almost exactly the same as the increment in labor's marginal value productivity on the farm. In this case weight-for-height may both raise earnings for a given job and enable workers to engage in more taxing, higher paid jobs.

These two studies can be taken as suggestive. In the Sierra Leone case the data are too crude to do otherwise, and in the India case the work is still preliminary. Nevertheless they are the only studies to date to try to grapple with the difficult issue of how to detect nutrition-productivity relationships in the face of household choice, and they do show some positive results.

Pitt and Rosenzweig (1985) in a different type of analysis relate farm profits (net of family labor valuation) and male labor supply of households in Indonesia to days sick by adult family members. They find no statistically significant effects of family illness on profits, but do find such an effect on male labor supply. The absence of an effect of illness on profits may reflect recourse to an active labor market, through which family labor can be replaced

at a constant wage, not necessarily absence of a productivity effect. If family and hired labor are perfectly substitutable (in efficiency units) in farm production and if households face a given wage for an efficiency unit of labor, then households demand a certain amount of labor in efficiency units. If household members are sick, laborers can be hired in the market with the opportunity cost, in terms of efficiency units, being equal between household and hired laborers. Farm profits will therefore remain unchanged. Of course the potential (or full) income of the household has declined because of the illness, since the sick members are unable to work on the days they are bedridden, should they wish to.

#### 4. A Partial Summary of Studies of Nutrition-Health-Education Linkages

The discussion thus far has concentrated on the limited question of effects of nutrition related variables on direct labor productivity or earnings. Nutrition also potentially affects time use, and such human capital as health (morbidity and mortality) and schooling (both attendance and achievement). Selowsky and Taylor (1973) hypothesized an important impact of better nutrition on the human capital development of children which directly and through more schooling would raise future productivity. The evidence directly testing this is nonexistent, however, certain individual links have been explored, especially between health and nutrition. The literature in this area is vast (see for instance Habicht and Butz, 1979; Martorell and Ho, 1984; and Chandra, 1982). It is similar to the much smaller nutrition-productivity literature in that two types of studies have been conducted: quasi-experiments ("quasi" because complete randomization is usually too difficult to achieve) in which a diet supplementation is given to one group and both treatment and control groups are observed over time, and epidemiological studies in which

correlations are measured using cross-sectional data. The quasi-experimental literature, for example the Narangwal nutrition study (Kielmann et al., 1983) or the INCAP supplementation study (Martorell, Habicht and Klein, 1982), is subject to the same questions as raised concerning the quasi-experimental literature on worker productivity effects (Chernichovsky, 1979, makes some of these same criticisms of the health-nutrition literature). For example in the Narangwal experiment whole villages were assigned to treatment groups. The control group villages were on the whole poorer, so the fact that their populations show less growth in young children is of uncertain meaning without controlling for differences in economic variables. Likewise empirical correlations computed from cross-section data are plagued by the same problem of household choice leading to endogenously chosen "explanatory" variables.

Not many studies have attempted to link nutrition to human capital development. Moock and Leslie (1982) use data from Nepal, regressing child school enrollment and grade performance on variables such as height-for-age (which for young children is likely to be endogenous), weight-for-height and hemoglobin levels. Likewise Popkin and Lim-Ybanez (1982) analyze school test scores of children in Manila using their standardized weight-for-height and hemoglobin levels and, Jamison (1983) examines how the number of grades children are held back in China relates to weight and height-for-age. In a somewhat different study Kielmann et al. (1983) regress indices of child psychomotor development on birthweight and average weight-for-age over the first 9 months of life, finding positive effects which decline as the child gets older.

The problem in interpreting those studies is again that the correlations are between variables influenced by household choices. Schooling attendance (and achievement) as well as current nutrient intakes and stature are outcome

variables of processes. For the health and nutrition variables the processes can be thought of as production functions which relate certain inputs to these outputs. Some of these inputs may also be outputs, such as diarrheal disease affecting child growth and mortality. The major point, however, is that levels of many of the inputs are chosen by the household. Thus as was true in the nutrition-productivity literature, correlations between inputs and outputs may simply represent the influence of unmeasured factors on both. For instance, in the psychomotor development regression, there are probably family variables which help determine the degree of stimulation a child gets at home as well the food eaten. If these are not being held constant in the regression the measured "influence" of the average weight (or birthweight) variables may simply convey the influence of those unmeasured variables. While some such factors, such as mother's education or income, can be measured and included in a regression, others such as the inherent "healthiness" of the child cannot.

Far more common than studies of schooling or cognitive outcomes of nutrition are analyses of the determinants of health, nutrient intakes or body size. For example Heller and Drake (1979) analyze standardized anthropometric scores and morbidity for children living in Candelaria, Colombia. Equations explaining standardized weight-for-height of children are estimated which include many inputs, hence look like production functions. In particular illness and diarrheal disease dummy variables are included, both for current and past periods. Heller and Drake even recognize the simultaneity problem for the current disease dummies, using predicted values from a logit equation for disease. However, endogeneity of other inputs is left unaccounted for. Among these are use of health inputs such as length of breast feeding, and food expenditures. Also left unaccounted for is sickness last period (year). As explained before, the usual argument of predetermination may be inappropriate

here, particularly if parents respond in their input allocations to individual characteristics which change only slowly and which are not measured in the data set and thus unknown to the analyst. The same problem exists with Drake and Heller's equations explaining morbidity. Change in relative weight-for-height is treated as endogenous, but variables such as past malnourishment, birthweight, immunizations received, and a dummy indicating whether weaning from the breast occurred suddenly or gradually are not.

In a related study Wolfe and Behrman (1982) examine determinants of standardized child weight, height as well as child mortality and average length of breastfeeding using a sample from urban Nicaragua. Their equations are supposed to represent reduced forms but they also include variables such as average household caloric intake, length of breastfeeding and household use of refrigeration in addition to community characteristics and family background variables. In their study the only variable which is predicted from a reduced form is the individual wage rate.

Longhurst (1984) examining farm households in Zaria, Nigeria predicts children's weight-for-height using the child's medical history, immunization record, a dummy for breast feeding and birth order. He then notes that economic status variables such as assets have little additional explanatory power when medical and demographic variables subject to household choice are held constant. What power these variables would have (assuming they were appropriately defined which unfortunately they are not in this study) in a reduced form equation, in which the endogenous health and demographic variables are not included is not clear from Longhurst's results. This is a relevant question, however.

Martorell, Leslie and Moock (1984) attempt to estimate a production function for anthropometric measurements and hemoglobin levels for children in

Nepal. Regressors including types of foods eaten in the past week, value of crop output, and morbidity outcome variables, all endogenous to household choices.

Two very useful studies which are not subject to the criticisms made in this paper are Rosenzweig and Schultz (1983) and Pitt and Rosenzweig (1985). These studies also look at the determinants of nutritional and health outcomes, but use instrumental variables techniques of analysis. Rosenzweig and Schultz estimate a production function for birthweight using a very large (nearly 10,000) national probability sample for the U.S. They did not examine the effects of maternal nutrition, but rather the delay in seeking medical care from a doctor, mother smoking while pregnant, birth order and mother's age at birth. All are treated as choice variables with instruments including individual characteristics such as education of the baby's parents, race, income and community characteristics ranging from availability of health services to variables representing regional economic activity. The production function estimates show important differences between coefficients when input endogeneity is accounted for versus when it is not. In particular the estimated effect on birthweight of visiting a doctor earlier in the pregnancy is found to rise tenfold when instrumental variables are used to correct for mothers deciding when to first see a doctor. This is not surprising since the promptness in first visiting a doctor is probably positively correlated with potentially low birthweight, a hypothesis substantiated by Rosenzweig and Schultz' analysis. This would lead to an underestimate of the impact of early doctor visits if not corrected for.

In a different study Pitt and Rosenzweig estimate household-level morbidity production functions in which consumption of different foods is explicitly investigated. Prices of foods, some household assets as well as

community health characteristics are used as instruments. The food consumption data are at the household not the individual level, which is the reason for aggregating morbidity across household members. This is not ideal, but nevertheless is suggestive. The estimates show that a ten percent increase from sample mean values in the consumption of fish, fruit, and vegetables reduce the probability of illness by nine, three and six percent respectively, whereas a ten percent increase in the consumption of sugar increases the probability of illness by almost twelve percent.

In addition to estimating the health production function Pitt and Rosenzweig estimate a reduced form equation that provides a direct link between prices and health. They show that a ten percent reduction from mean values in the prices of vegetables and vegetable oil will decrease the probability of the household head being sick by four and nine percent, respectively, whereas the same reduction in the price of sugar will increase the probability of illness by twenty percent, albeit from a low base. Presumably the price effects result from changes in nutrient intakes which result from the price changes.

Two other studies which use instrumental variables techniques should also be mentioned. Chernichovsky et al. (1983) and Blau (1984) estimate structural equations for weight and height respectively. Chernichovsky used data from the Narangwal experiment, while Blau uses survey data from Nicaragua. Instruments used include socioeconomic variables, such as land cultivated, which do not directly affect body size. The Indian data show a positive effect of caloric intake on child weight after controlling for age and sex, while the Nicaraguan data highlight that larger families tend to have smaller children.

## 5. Conclusions

The major point of this paper has been to point out the great difficulties in interpreting much of the existing evidence concerning the

magnitude and directions of interactions between nutrition, productivity and other human capital development. While most of the concerns relate to the non-experimental evidence, even experiments exhibit some of these flaws. Yet there is a small body of more reliable evidence which does suggest that higher current nutrient intakes and perhaps larger body size (reflecting past intakes) do enhance labor productivity when nutrient intakes are low, and for activities which use little capital. A few of these studies have been experimental, although more recently the beginnings of an econometric approach to analyzing survey data has emerged.

To date the evidence has concentrated on what economists call a pure worker effect. That is output rises as nutrition improves, holding other inputs constant. To the extent that better nutrition and health, both in the past and currently, enhance decision making capabilities (the allocation of inputs) then the existing results will understate the economic impact of better nutrition. There is clear evidence (e.g., Jamison and Lau, 1982) that education raises farm profits, and by more when farmers face major changes. To the extent that better childhood nutrition raises both the likelihood and the learning outcomes of schooling, as hypothesized by Selowsky and Taylor (1973), the payoffs could be higher. However as of now there seems to be no reliable evidence on this question. Likewise there is as yet no convincing evidence concerning potential effects of better nutrition on the allocation and productivity of adult activities performed in the home, particularly females.



Appendix: A Simple Example of Simultaneity Bias

Consider the following, simple example. Let the production, or earnings, function be represented by

$$Q = \beta_0 + N\beta_1 + X\beta_2 + \varepsilon$$

where  $Q \equiv$  output (earnings) or log thereof,  $N \equiv$  nutrient intake,  $X \equiv$  a vector other inputs. The  $\beta$ 's are unknown parameters to be estimated and  $\varepsilon$  is the unobserved disturbance. Decompose the disturbance into two components, one ( $v$ ) specific to that observation, which represents firm (individual) specific characteristics which affect production (earnings), and the other ( $u$ ) which represents pure randomness, not specific to an individual observation. As suggested in the text the individual-specific component might include management ability or land quality. The critical point is that it consists of variables known to the household, but not to the analyst, while the pure noise component is unobserved both to the household and the analyst.

In this set-up nutrient intake is considered to be a household choice variable, while the vector of other inputs may or may not be. It is then possible to solve for the household's choice of nutrients in terms of all of the exogenous variables the household faces, including the individual-specific error term, which the household knows, but not including that part, the pure noise, not known to the household. Doing this we can express  $N$  as:

$$N = \gamma_0 + Z\gamma_1 + V\gamma_v + e$$

where  $Z$  represents all the observed variables taken as given by the household

(such as prices and community characteristics, and which may include all or a subset of X), and  $e$  is a random disturbance representing variables both unknown to the household and the analyst. The elements of X which are endogenous may be similarly expressed.

Now to simplify the algebra assume that X is a scalar. If ordinary least squares is used to estimate the production (earnings) function, the estimate of  $\beta_1$  can be expressed as

$$\beta_1 = \beta_1 + \frac{S_{xx} \sum_i (N_i - \bar{N}) \varepsilon_i - S_{nx} \sum_i (X_i - \bar{X}) \varepsilon_i}{S_{xx} S_{nn} - S_{nx}^2}$$

where the  $i$ 's subscript observations  $i = 1, \dots, T$ , the bars ( $\bar{\quad}$ ) represent sample averages,  $S_{xx} = \sum_i (X_i - \bar{X})^2$ , with  $S_{nn}$  and  $S_{nx}$  similarly defined. If the non-nutrient variable, X, is not a household choice variable, it will be uncorrelated with both  $v_i$  and  $u_i$ , that is with  $\varepsilon_i$ . Nutrients, however, will be correlated with  $\varepsilon_i$  since the unobserved component  $v_i$  will influence the households; choice of N. Moreover the correlation will very probably be positive since households with higher output (earnings), holding measured inputs constant, will have higher incomes, some of which will be consumed as food. Since  $S_{xx}$  and  $S_{xx} S_{nn} - S_{nx}^2$  (and their probability limits) are necessarily positive one can obtain

$$\text{plim } \beta_1 = \beta_1 + \frac{\bar{S}_{xx}}{\bar{S}_{xx} \bar{S}_{nn} - \bar{S}_{nx}^2} \gamma_v \sigma_v^2 > \beta_1$$

where  $\bar{S}_{xx} = \lim_{T \rightarrow \infty} \frac{1}{T} S_{xx}$  (which is assumed to exist and be finite),  $\sigma_v^2$  is the variance of  $v_i$  and  $\gamma_v$  is the coefficient of  $v_i$  in the nutrient intake equation, and is positive as argued above.

Now consider the case where  $X$  is endogenous. It too can be expressed as a function of the  $Z$  variables and  $v$ . In this case the probability limit of  $\beta_1$  has another term, which is proportionate to the product of the correlation of nutrients and  $X$ ,  $\bar{S}_{nx}$ , and the correlation between  $X$  and  $v$ . The signs of these correlations will obviously depend on exactly which variable(s) is used as  $X$ . Therefore no generalizations are possible except to note that the bias (inconsistency) can be reduced (or enhanced) when other endogenous variables are included in the equation.

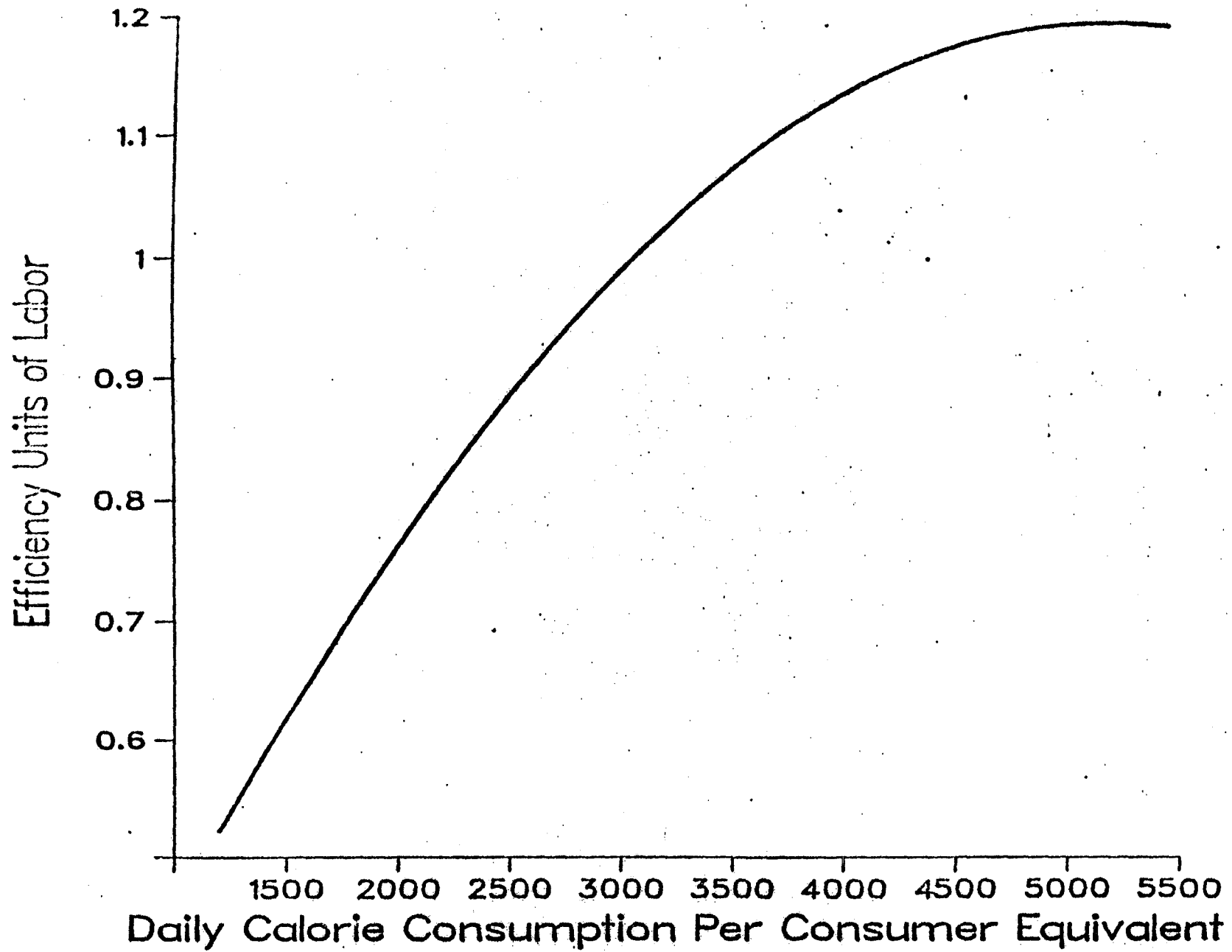
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Estimated Efficiency Labor Function for Sierra Leone Farm Households