Food Security Technical Support Facility

DRAFT

Initial Specialist Comments for DFID on:

J-A Arcand, 2001
'Undernourishment and economic growth: The efficiency cost of hunger'
(FAO Economic and Social Development Paper 147)

March 2002

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This document contains three initial draft commentaries on the paper by Jean-Louis Arcand. Two were prepared by leading international food security and development policy experts, Simon Maxwell (ODI) and Stephen Devereux (IDS). They focus more on the relevance and validity of the overall argument and policy implications, in the context of wider contemporary food security and development debates. However, where relevant, the authors make comments on the adequacy of the data and analysis.

The third commentary was prepared by an expert in quantitative econometric analysis, Dr. Barry Reilly, (Senior Lecturer in Economics at the University of Sussex), who analyses the validity of the econometric analysis and quantitative methods used.

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Undernourishment and economic growth

Comments by Stephen Devereux (IDS Fellow)

February 2002

The analytical problem that this paper sets out to address is one of attribution. It is well known that a correlation exists between a country's economic performance (as measured by GDP per capita) and its population's average nutritional status (proxied, say, by per capita food availability). Or, more succinctly: "the correlation between income and nutrition is a well-established empirical regularity" (page 9). But in which direction does the causality run?

The policy relevance of this question is both significant and topical. In a recent speech (in Bonn in September 2001) Clare Short argued that the eradication of hunger is best achieved indirectly, through tackling poverty: if poverty is eradicated, hunger and food insecurity will necessarily follow. Short's argument supports Easterly's empirical finding (cited by Arcand, page 5) that a 1 per cent increase in GDP per capita raises per capita calorie intake by 538 kcal/day. It also motivates a concentration of development assistance resources on income generation programmes rather than on nutrition interventions.

Arcand's paper does find evidence of the reverse causality: "there is a statistically significant, and quantitatively important impact of nutrition on growth [which] operates in part directly, probably through its impact on labour productivity, as well as indirectly, through improvements in life expectancy" (page 5). In quantitative terms, Arcand finds that simply raising dietary energy supply in developing countries to 2,770 kcal/day (the level that would eliminate global food deficits, according to the Sixth World Food Survey) would raise GDP per capita by 1.23 per cent per annum. The policy implication is that economic growth and poverty reduction can be stimulated through increases in food intake and investments in nutrition – particularly through targeted nutrition support to the poor, whose productivity is likely to be most severely constrained by inadequate food intake and low nutrition status. As Arcand notes, this aggregate finding supports a large body of microeconomic literature linking improvements in nutrition to higher labour productivity.

Much of the paper is concerned with testing for robustness of the results obtained early on. Arcand concludes that the key results do stand up under a battery of econometric tests. Importantly, given Peter Svedberg's recent strong attacks on the FAO methodology for estimating national dietary energy supplies – one of two datasets used by Arcand for this analysis – Arcand demonstrates that his results are "robust to the Svedberg critique" (page 20).[1]

Although this is an impressive paper that reaches some important conclusions, I was left with some reservations and unanswered questions. For instance, it is not clear to me why the paper discusses *indirect* transmission mechanisms of nutrition on growth (life expectancy and schooling) at some length, but does not give comparable attention to *direct* transmission mechanisms (impacts of nutrition status on labour productivity). Possibly Arcand feels that sufficient theoretical and empirical attention has been given by other authors to nutrition-productivity linkages, while life expectancy and schooling impacts on economic growth remain open to debate. However, for development policy purposes it is "the efficiency cost of hunger" – to cite the paper's subtitle – that is most critical.

In this context, there are some surprising omissions in the References, especially in the 'micro-level' rather than the 'macro-level' literature. Economists such as Michael Lipton have considered the links between labour productivity and nutrition intake in terms of the 'efficiency wage' debate (Lipton wrote a classic paper on this topic for the World Bank in 1983, which Arcand might consider

[1] Arcand's defence is based on econometric exposition, which is beyond my competence to verify. An econometrician is needed to assess the methods selected and the accuracy of their application.

too dated to be of relevance). Nutritionists such as Nevin Scrimshaw have worked on the long-term productivity impacts of nutritional deficits in early childhood, and Amartya Sen has written eloquently on this topic from within the rubric of his 'capabilities' approach. There is also more work out of IFPRI's Food Consumption and Nutrition Division worth citing than the single paper (Haddad and Bouis 1991) cited here. On the other hand, Arcand's paper is concerned with cross-country econometric comparisons, so perhaps these omissions do not compromise the quality of this paper.

Another significant omission in the theoretical exposition and empirical estimation is the level of within-country *inequality*. There is one reference to the anomalous cases of Botswana, Thailand and the Philippines, all of which suffered persistently high levels of PFI ("prevalence of food inadequacy") despite enjoying high GDP growth rates, a contradiction which Arcand attempts to resolve by suggesting that "inequality and the ensuing high rates of malnutrition have constrained what might have otherwise been even higher rates of growth" (page 39). The fact remains that economic growth does not translate into widespread nutrition enhancement if it is narrowly based (as in Botswana's enclave diamond sector) so that its benefits are concentrated in few hands. Conversely, targeted interventions to raise the nutrition status of the poorest and most undernourished citizens should achieve significant benefits in terms of economic growth, because the poorest tend (a) to have lowest food intakes; (b) to be more exposed and most susceptible to debilitating diseases; and (c) to expend most physical energy on work (e.g. low-input low-out farming). This fact would strengthen Arcand's argument for reversing the conventional focus of policy attention, and makes a powerful case for prioritising 'feeding the poor' for developmental as well as welfarist reasons.

As Arcand finds at *national* level, the greatest potential for improving economic growth through increasing food availability is in countries with the highest initial food deficits. What Arcand does not point out, though, is that improved food *availability* must translate into improved food *access* for the nutritionally deprived. It is not a country that is 'food deficit' but some sections of its population. Also, Arcand does not recognise that nutrition status (and hence labour productivity) is a function of health status as well as food intake. Enhanced food intake can reduce susceptibility to certain debilitating diseases and chronic conditions, but not all (malaria, for example, which reduces GDP in Africa by an estimated 0.25 per cent per annum). Without complementary interventions to address the severe health problems that afflict the poor tropical countries, in particular, the benefits of improved food availability are likely to be less than optimal. This point illuminates a possible limitation of the cross-sectional approach adopted in Arcand's paper. Most of the persistently 'high PFI regime' countries listed in Table 10 (page 38) are tropical, while most of the persistently 'low PFI regime' countries are not (e.g. Iceland, which clearly does not face the same adverse health environment as Uganda).

Another general critique often levelled against this kind of analysis is that the focus on a single relationship – in this case, between, income and nutrition – is overly deterministic and reductionist. There are many explanations for low growth and high undernutrition in developing countries. Both are symptoms and causes of poverty, and attempting to explain one in terms of the other may be looking for marginalist policy solutions to profoundly structural problems. The concluding paragraph of this paper hints at a recognition of this interconnectedness between poverty and nutrition.

A few final quibbles. It is sometimes unclear whether Arcand is using *levels* of GDP per capita and *growth* of GDP per capita interchangeably. Also, despite devoting several pages to the contribution of schooling to economic growth, it remains unclear to me why this variable is found to make little contribution, when many studies of school feeding programmes (not cited) find significant improvements in school enrolment, attendance and learner performance as a direct consequence of this intervention. Conversely, why increasing life expectancy should emerge as a significant positive causal variable is never explained. Living longer is surely a consequence of economic growth, not a cause, and extending longevity post-retirement is increasingly modelled as a drag on economic growth in wealthy countries.

To sum up: this paper is an impressive piece of work which demonstrates empirically an extremely important positive relationship between nutrition status and national economic growth. To a non-econometrician, the methodology appears technically competent and the econometric tests appear to have been rigorously applied. However, in many places the paper is simply unreadable to non-econometricians, who therefore have no way of assessing the validity of either the approach adopted by the author or the conclusions reached. The paper could have benefited from wider reading and deeper analysis: on the empirical relationship between nutrition status and labour productivity, for instance, and on the effects of intra-country inequality.

Undernourishment and economic growth

Comments by Simon Maxwell (Director, ODI)

February 2002

Summary and Overview

1. There are few people in the world fully equipped to comment on this paper, and I am not one of them. However, Jacques Vercueil captures the essence of why the paper is useful in his foreword, viz. there has been much analysis of the impact of nutrition at the micro level, but little at the macro level which the paper attempts. The macro analysis is said to demonstrate a significant relationship between better nutrition and growth - and if that can be sustained the paper is indeed a useful contribution.
2. The key results are summarised in Tables 14 and 15, and in Figures 7 and 8 on pages 51–54. For countries suffering from food inadequacy, eliminating it would raise the growth rate by between 0.47 percentage points and over 4 percentage points (Table 14 col 3). For Africa, the figures range from 0.6 to 5.8 percentage points (Table 14 col 4). The range is stretched even further at the bottom end if different assumptions are used, and the Abstract cites a range from 0.16 to 4.0 percentage points. The author comments that 'if the range of figures generated by the econometric work makes one uncomfortable and one wishes to stick to a single number, . . ., then it is seen that the annual growth rate of GDP per caput was reduced by 1.6 percentage points by an inadequate level of dietary energy supply (DES)' (pages 52-53). Note this is not the same figure as the one given in the Foreword, which cites a figure of 1.13 percentage points, but is consistent with the Abstract.
3. Vercueil summarises the main policy implication: 'policies which reduce or eliminate under-nourishment in developing countries should not be viewed only in terms of their welfare and humanitarian benefits, but also in terms of their growth-promoting dimensions'.
4. The paper also claims that under-nutrition explains why growth in Africa is lower than in other regions, other things being equal, which would solve a long-standing econometric riddle. And it provides some analysis of the pathways by which under-nutrition hampers growth, both directly (through productivity effects) and indirectly (through life expectancy and (more problematically) schooling).
5. There is an important caveat at the end of the paper, which is that the same results could have been obtained using a measure of poverty. The author comments that 'computing a measure of the prevalence of food inadequacy is equivalent, analytically, to computing a poverty rate, since establishing a calorie cut-off point is equivalent to setting a poverty line. The consequence is that the empirical results reported in this paper suggest that eliminating, or at least significantly reducing, poverty, will have an important quantitative impact on the growth rate of GDP per caput.'

Detailed Comments

1. The main comments will doubtless focus on the measures of food inadequacy used, and on the econometrics. I am not competent to comment on either. The paper recognises the problems with the FAO measures of food inadequacy and dietary energy supply, especially as expressed by Svedberg, and explicitly attempts to correct for them. I cannot say whether the adjustments are correct, but it is worth noting that the author claims that the growth effect is enhanced as a result of the corrections.
2. There are three other points to make. First, the analysis of the pathways is only partial. See the two figures attached, the first of which illustrates the impact of under-nutrition on morbidity and intelligence as well as productivity and life expectancy, and the second of which illustrates the

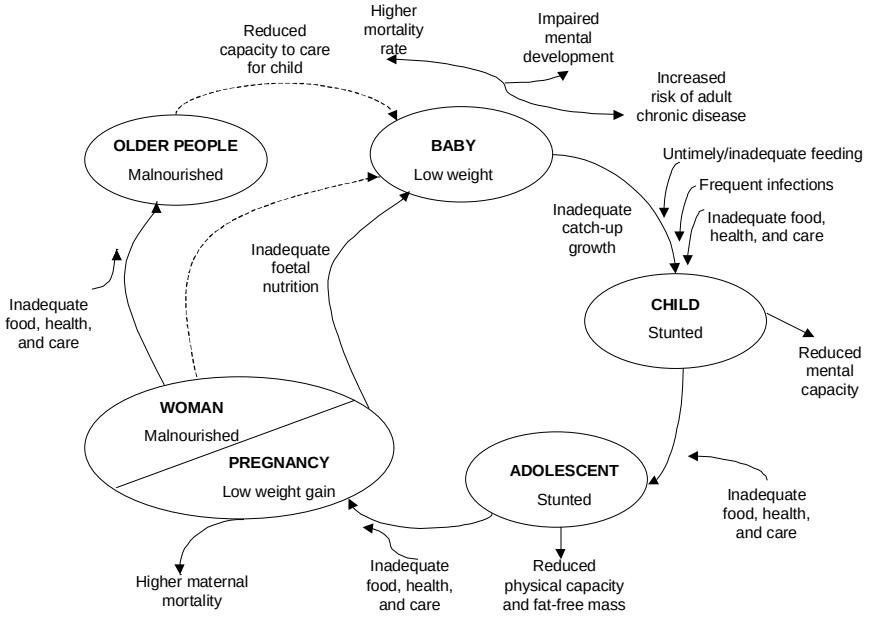
impact of malnutrition through the life-cycle, including adults and old people. It would have been useful to have an analytical framework in the paper based on this wider understanding of nutritional effects, in order to identify which pathways might usefully have been studied in the macro analysis.

3. Second, the paper says very little about transitory food insecurity and its effect on growth. The food security literature is rich in its analysis of coping and adaptation strategies, which could impact on growth – for example, holding unproductive assets as security, or planting low-yielding but drought-resistant crops as protection against rain failure. In effect, the high cost of insurance is another possible transmission mechanism.

4. Third, the policy implications are not entirely clear. What the paper says is that food inadequacy has a negative impact on growth, and that this effect is analytically indistinguishable from poverty. The effect is either quite large or not very large, depending on the specification chosen. It certainly follows that reducing food inadequacy or poverty would boost growth, and that is important. The paper concludes as much. It does not follow that any particular policy is desirable, whether this is increasing food production, supporting agriculture, liberalising or not liberalising food trade etc . . . The paper does not claim this, though it might well be used in this way.

5. To conclude, the proof of this particular pudding is in the robustness of the econometrics. If they stand, and there is no reason to suppose that they do not, the paper is a very useful contribution.

Nutrition throughout the life cycle



Source: 4th Report on The World Nutrition Situation, January 2000. UN ACC/SCN in collaboration with IFPRI

Why nutrition matters (2)

- **Survival**
  - 54% of all deaths of children under five associated with low weight for age; low birthweight children are ten times more likely to die.
- **Growth**
  - Low birthweight babies become small adults: 5 cm shorter and 5 kg lighter.
- **Health**
  - Malnutriton is the highest risk factor in the global burden of disease: 12% of all deaths, 16% of total loss of DALYs.
  - Foetal malnutrition causes health problems in later life (hypertension, diabetes).
- **Intelligence**
  - Iron deficiency delays psychomotor development and impairs cognitive development, lowering IQ by 9 points.
  - Non-stunted children score 25% higher than stunted children in non-verbal intelligence tests.
- **Productivity**
  - A low body mass index (BMI) reduces work output and productivity.
- **And . . .**

A Review of the Econometric Analysis in:

Undernourishment and Economic Growth: the Efficiency Cost of Hunger
by Jean Louis Arcand

Comments by Dr Barry Reilly (AFRAS, Sussex)

March 2002

The following commentary raises the following key points:

- The author uses at least eight different estimation procedures to examine the relationship between economic growth and nutritional status. The use of the different procedures is motivated by the author's interest in verifying the robustness of the relationship between economic growth and nutrition. The various procedures used provide estimates for the effect of the nutritional variables on economic growth and these are ultimately found to vary in range by a factor of 20. This lack of precision is not comforting and the estimates are subsequently used to provide the basis for the author's calculations of output loss (or the efficiency costs) associated with under-nourishment.
- It is not contentious to suggest the existence of an important causal relationship that runs from nutritional status to economic growth. The important issue is to quantify the magnitude or size of this effect. In the author's view, the econometric evidence presented is not helpful in informing the magnitude of this size effect. The concerns the author has about the analysis are detailed more fully in the review. However, they can be grouped into three broad categories:
  (i) The econometric analysis is undertaken in a disjointed and piece-meal fashion
  (ii) There is nothing persuasive in the econometric analysis to indicate that the nutritional status variables are not simply proxying for the effects of other important variables omitted from the growth equations (e.g., physical capital).
  (iii) This paper persists in treating the nutritional status variables as exogenous to the economic growth process throughout. The key question that arises, and is not addressed, is the extent to which the estimated effects reported are a consequence of an inappropriate exogenous treatment of the nutritional status variables within the growth equations.

The review provides brief explanations for the techniques used, and why they are used in this context. It then analyses the six sections of Arcand's paper, highlighting in more detail specific econometric points that emerge in turn.

Introduction

The above paper examines the impact of two measures of nutritional status on per capita growth using panel data for approximately 100 countries over three decades spanning the period 1960 to 1989. The empirical approach adopted largely augments conventional economic growth equations (popularised by Barro and others) with variables that capture a country's nutritional status. A theoretical model embedded in neo-classical theory is outlined to motivate a relationship that causally relates economic growth to nutritional status. This treatment reverses the more standard causal relationship usually examined in the literature – that of nutritional status being determined by income. The author uses a variety of conventional panel econometric methods to estimate the relationships of interest.

A stated objective of the paper is the evaluation of the robustness of the relationship between economic growth and two comparable nutritional variables (i.e., Daily Energy Supply (DES) and Prevalence of Food Inadequacy (PFI)). In regard to this objective the author exploits the fact that some panel procedures allow model mis-specification problems (e.g., country heterogeneity and measurement errors in the nutritional variables) to be explicitly addressed.

The author uses at least eight different estimation procedures to examine the relationship between economic growth and nutritional status. The use of the different procedures is motivated by the author's interest in verifying the robustness of the relationship between economic growth and nutrition. The various procedures used provide estimates for the effect of the nutritional variables on economic growth and these are ultimately found to vary in range by a factor of 20. This lack of precision is not comforting and the estimates are subsequently used to provide the basis for the author's calculations of output loss (or the efficiency costs) associated with under-nourishment.

It is not contentious to suggest the existence of an important causal relationship that runs from nutritional status to economic growth. The important issue is to quantify the magnitude or size of this effect. In my view, the econometric evidence presented is not helpful in informing the magnitude of this size effect. The concerns I have about the analysis are detailed more fully in the subsequent sections of this review. I could briefly classify my general reservations into three broad categories.

(a) The econometric analysis is undertaken in a disjointed and piece-meal fashion. For instance, the concern about robustness is confined to the early sections of the paper and is completely neglected when the econometric analysis focuses on 'growth traps' in part 4 and in the estimation of structural models in part 5. Issues relating to measurement errors in the nutritional variables that the author apparently identifies (incorrectly in my view) as a problem in part 2 are conveniently ignored in the analysis reported in parts 3, 4 and 5. In addition, by the time we get to part five PFI as disappeared as an independent determinant of economic growth and, the threshold effects identified in part one as of some significance are subsequently ignored – aside from their implicit treatment in the switching model of part 4. A more persuasive approach would have been to develop and estimate a smaller number of econometric models that addressed all model specification concerns in a unified and coherent way. This paper chooses not pursue this approach. It is not altogether surprising that after an extensive deployment of a vast range of econometric techniques, the author's preferences in undertaking the quantitative analysis on the efficiency costs of hunger relies in large part on the basic pooled OLS estimates.

(b) There is nothing persuasive in the econometric analysis to indicate that the nutritional status variables are not simply proxying for the effects of other important variables omitted from the growth equations (e.g., physical capital).

(c) The author argues that a paper closest in spirit to this current paper is Wheeler (1980). There is a fundamental difference, however, and it is this difference that provides a significant weakness of this paper. The Wheeler model treated nutritional status as endogenous. This paper persists in treating the nutritional status variables as exogenous to the economic growth process throughout. This treatment is all the more odd given that the author goes to some length to endogenise life

expectancy and schooling to capture indirect effects of nutrition on growth. The key question that arises, and is not addressed, is the extent to which the estimated effects reported are a consequence of an inappropriate exogenous treatment of the nutritional status variables within the growth equations.

The next section of this review endeavours to provide brief explanations for what techniques are being used, and why the author is actually using them in this application. The remaining sections then concentrate in turn on the six parts of the paper and highlight in more detail specific econometric points that emerge in each section.

1. Review and Brief Description of the Econometric Techniques Used

1.1 It might be useful in the first instance to briefly outline four of the major estimation techniques used by the author. These will explain them more fully as they are encountered in subsequent sub-sections but it is useful to flag them at the start. The four main estimation procedures used in part one of the author's analysis are:

(i) A Within Estimator: This exploits the 'within' dimension of the data (i.e., differences within countries over time) and uses the OLS procedure. The consistency (or unbiasedness) of the estimator requires that the explanatory variables are strictly exogenous but does not impose any restrictions on the relationship between the explanatory variables and the country specific effects.

(ii) A Between Estimator: This exploits the 'between' dimension of the data (i.e., differences between countries) and uses the OLS procedure. The consistency (or unbiasedness) of the estimator requires that the explanatory variables are strictly exogenous and uncorrelated with country specific effects.

(iii) A Pooled OLS Estimator: This exploits both 'between' and 'within' dimensions of the data but does not do so efficiently. The consistency (or unbiasedness) of the estimator requires that the explanatory variables are uncorrelated with country specific effects.

(iv) A Random Effects Estimator: This exploits both 'between' and 'within' dimensions of the data but, in contrast to OLS in (iii), does so efficiently by using a GLS estimator. It can be determined as a weighted average of the 'between' and 'within' estimators. The weight itself depends on the relative variances of the two estimators. The more accurate (or more efficient) estimator gets the higher weight. The Hausman test compares this random effects estimator to the 'within' estimator. If the null is rejected, this favours the 'within' estimator's treatment of the omitted effects (i.e., it favours the fixed effects model but only relative to the random effects model).

1.2 The author commences with a simple growth convergence model designed to provide a benchmark for subsequent analysis (see table 1). This could be summarised as follows:

$$y_{it} = \alpha + \beta x_{it} + u_{it} \qquad [1]$$

where i = 1,……..N countries and t =1,2,3 decades (i.e., 1960s, 1970s and 1980s). The data structure is clearly panel in nature and the sample size is $N \times 3$, where N is the number of countries.

The y_{it} variable denotes the decade average growth rate (measured in terms of GDP per capita) for the ith country in the tth decade. The decade average growth rates are used to smooth out any cyclical fluctuations in economic activity and obtain a proxy measure for trend (or long-run growth). The x_{it} variable captures, for instance, the realisation of daily energy supply (DES) for the ith country in the tth decade. It is not, however, clear from the study whether this (or its counterpart measure PFI) also represents a decade average measure. The u_{it} is a random variable assumed to be independently and normally distributed. In other words, random shocks in the 1960s are not correlated with those for 1970s or beyond, and, within particular decades, random shocks are uncorrelated across countries.

This model [1] is estimated by Ordinary Least Squares (OLS) (see (iii) above) and the only concession made to heterogeneity across time or space is the inclusion of dummy variables for two decades (the omitted category being the 1980s) and dummy variables for two geographical areas (Africa and Latin America). The purpose of estimating this is to provide some baseline estimates for the effects of the PFI and DES variables on economic growth. However, the estimation procedure treats the T observations on N countries as if they were N×T separate observations.

1.3 The next model estimated by the author (see table 2) is a Solow growth model augmented by the set of PFI or DES variables. The data used, however, is not panel in nature but purely cross-sectional and the estimating equation could be expressed as:

$$y_i = \alpha + \beta x_i + u_i \qquad [2]$$

where i = 1,……..N countries.

The y_i denotes the thirty–year average growth rate (measured in terms of GDP per capita) for the ith country. Again, it is not obvious to what time period the PFI or DES measures relate. The estimation procedure used is again OLS (see (iii) above) but, given the cross-sectional nature of the data, no concessions are made to potential heterogeneity across either time or space.

1.4 The third set of estimates reported (see table 3) are based on a model that controls for country-specific heterogeneity. The exclusion of relevant variables from any econometric model biases the coefficients of the included variables (in this case the primary interest is in terms of the estimated effects for DES and PFI). If country-specific effects are relevant they should be included in some way, otherwise the estimated effects are subject to potential bias. In simple terms, the model could be expressed as:

$$y_{it} = \alpha_i + \beta x_{it} + u_{it} \qquad [3]$$

where i = 1,……..N countries and t =1,2,3 decades (i.e., 1960s, 1970s and 1980s).

In this case the model is a variant of [1] but has the additional feature that each country is allowed its own specific intercept term. This explicitly assumes that any omitted country-specific factors can be captured by a country-specific fixed effect or dummy variable. This is one way of dealing with heterogeneity across countries and generates N additional intercept estimates but explicitly assumes that country-specific heterogeneity is constant across time. This may not be the case. The estimation procedure implicitly assumes that the fixed effects are correlated with the included explanatory variables (i.e., x_{it}).

The implications of the estimation procedure can be discerned from table 3. There are no estimates reported for the overall intercept term or for the Africa or Latin America dummies. This is because each country now has its own specific intercept and so these other terms are now redundant.

A useful trick is used in estimating the parameters of this fixed effects model. This involves sweeping out the country–specific fixed effects through taking the deviations from the time series means for each individual country. The estimation procedure is explaining to what extent y_{it} differs from $\bar{y}_i$ – the mean for country i over the time period of the panel. This is why the estimator is known as a 'within-group' estimator because only variation <u>within</u> each country is used in formulating the estimator. In the context of this application, this estimator captures the effects of inter-decade movements in DES and PFI on economic growth having controlled for country-specific heterogeneity. The author argues that this provides insights into 'medium-run' rather than 'long-run' determinants of the underlying relationship. The 'within-group' model is also referred to as a Fixed Effects model or a Least Squares Dummy Variable (LSDV) model or (more rarely) a covariance estimator. The estimator is the 'within' estimator noted in (i) above. It is important to

note that this estimator does not explain variation across country (i.e., why $\bar{y}_i$ is different from $\bar{y}_j$ where i and j are two different countries). In other words, it is not a 'between-group' estimator. This type of estimator is subsequently used by the author and is discussed below.

Finally, note at the bottom of table 3 the Hausman test. The use of the test in this case is to discriminate between a model where the omitted country heterogeneity is treated as fixed and correlated with the explanatory variables, and a model where the omitted country heterogeneity is treated as random and independent of the explanatory variables. In the case of the latter regression model, α_i is now distributed as a random variable rather than as a set of N country-specific dummy variables. This is called a Random Effects model. The estimation of this type of model requires implementing a more complicated Generalised Least Squares (GLS) procedure than the simpler OLS procedure. The GLS estimator is actually a weighted average of a 'within-group' and a 'between-group' estimator (see above). The Hausman test result reported in the bottom of table 3, however, suggests that the fixed effects treatment for the omitted heterogeneity is appropriate for the specifications estimated by the author.

1.5 The fourth set of estimates (see table 4) is based on a model that attempts to address a potential measurement error problem in the two explanatory variables of interest (DES and PFI). The approach adopted involves eliminating country-specific factors through 'first differencing' and then using instrumental variable estimation to purge the two explanatory variables of their stochastic component thus rendering them independent of the equation's composite error term. The author treats the explanatory nutritional status variables as being measured with error for reasons cited in the paper. The 'true' explanatory variable is defined as x_{it} but the empirical measure available to the investigator contains a country-specific error component (π_i) and a random error that varies across both time and country (ε_{it}). The author assumes that the empirical measure is expressed as: $x_{it} + \pi_i + \varepsilon_{it}$

In terms of equation [3]:

$$y_{it} = \alpha_i + \beta x_{it} + u_{it} \qquad [3]$$

this mis-measured variable could be introduced as:

$$y_{it} = \alpha_i + \beta[x_{it} + \pi_i + \varepsilon_{it}] + u_{it} \qquad [4]$$

The primary problem here is that ε_{it} could be correlated with u_{it} and the estimate of β will thus be biased. A second-order problem relates to the role played by π_i. Its effect could be captured by the country-specific effect α_i. The author deals with both these problems by first lagging [4] by one period:

$$y_{it-1} = \alpha_i + \beta[x_{it-1} + \pi_i + \varepsilon_{it-1}] + u_{it-1} \qquad [5]$$

and subtracting [5] from [4] yielding:

$$y_{it} - y_{it-1} = [\alpha_i - \alpha_i] + \beta[x_{it} - x_{it-1}] + \beta[\pi_i - \pi_i] + \beta[\varepsilon_{it} - \varepsilon_{it-1}] + [u_{it} - u_{it-1}] \qquad [6]$$

This could be re-expressed as:

$$\Delta y_{it} = \beta\Delta x_{it} + \beta[\Delta\varepsilon_{it}] + \Delta u_{it}$$

since $[\alpha_i - \alpha_i] = 0$, $\beta[\pi_i - \pi_i] = 0$, and where Δ is the first difference operator. Both error terms are treated as moving averages with a unit root. In this case, both sets of country-specific fixed effects [α_i and π_i] have been eliminated and the only remaining problem now relates to the potential correlation between the moving average errors $\Delta\varepsilon_{it}$ and Δu_{it}. This is resolved by instrumental variable (IV) estimation where either the differenced DES or PFI variables are predicted on the

basis of an instrument set that includes all the levels variables from the first decade (1960s – see p.21). This has the effect of purging the differenced DES or PFI variables of their stochastic components thus providing consistent and unbiased estimates for the effect of the nutritional status variables on economic growth. A good instrument (or set of instruments) must be independent of the errors ($\Delta\varepsilon_{it} + \Delta u_{it}$) but highly correlated with the variables being instrumented (i.e., DES or PFI). For instance, if we define t=1,2,3 for the 1960, 1970, and 1980 decades, we could re-write [6] as:

$$y_{i3} - y_{i2} = \beta[x_{i3} - x_{i2}] + \beta[\varepsilon_{i3} - \varepsilon_{i2}] + [u_{i3} - u_{i2}] \qquad [6']$$

where $y_{i3} - y_{i2}$ is the difference in average growth rates between the 1980s and 1970s, and $[x_{i3} - x_{i2}]$ is the difference in DES or PFI between the two decades. The instrument set used by the author contains, *inter alia*, x_{i1} and y_{i1}, which are correlated with (or determine) x_{i3} and x_{i2} but independent of u_{i3} and u_{i2}. This is clearly a desirable property. The levels (rather than differences) are used as instruments because the IV estimator with levels as instruments has smaller variances and this approach, as used by the author, is generally recommended in the literature.

The actual IV estimation procedure used in this case adopts the Generalised Methods of Moments (GMM) technique. This approach is popular in panel estimation and exploits what are called moment conditions. The number of moment conditions (and hence the number of valid instruments) increases with the number of time periods in the panel. It is known that imposing more moment conditions increases the efficiency of the estimator.

Three additional points are worth referencing here. Firstly, the use of differencing in conjunction with the use of an instrument set based on data from the first decade reduces the sample size by about two-thirds. Secondly, the instrument set used is only valid as long as the u_{it} are not serially correlated. This is an empirical issue and the author (p.21) remains somewhat silent on this particular matter. Thirdly, the author ignores the additional bias introduced by the fixed effects estimator through the correlation in the (DES or PSI) variables over time. The implications of these latter two issues are discussed further below in section 3.

1.6 The fifth set of estimates (see table 5) is reported by the author to provide a sensitivity check for the 'within-group' estimator. The estimates reported in this table are based on the 'between-group' estimator that exploits the between dimension of the data (i.e., differences between countries). The 'between-group' estimates are obtained by an OLS regression of country mean growth rates (averaged over time) on a constant term and country averages of the explanatory variables (averaged over time). Thus, we return to a simple cross-sectional model that's primarily concerned with explaining variation in growth rates across (or 'between') countries and not 'within' countries. Given the extended time period over which the averaging occurs, the author refers to this estimator as providing insights into the 'long-run' relationship. I would argue that this is a misnomer as the within-estimator also provides insights into the long-run relationship given the construction of the data.

A second sensitivity check (see table 6), using the alternative Sachs and Warner (1997) dataset, is also undertaken. This data set contains an array of institutional, climatic and economic variables that are assumed to determine long-run economic growth. The sample size (or number of countries) is lower than for the Heston-Summers data set used for the earlier analysis but this does not seem to be of too much significance. I assume that this is also a 'between-group' estimator but the author is not clear. The DES per capita variable is now less well determined but the PFI is extremely well determined. However, in contrast to its treatment in earlier equations, it is now introduced into the averaged growth equation using a logistic transformation. I discuss this further below.

1.7 The next phase of the author's analysis is to explore the indirect mechanisms through which nutritional status can affect economic growth. The four econometric procedures listed above ((i) to (iv)) are all used to explore the role of nutritional status on economic growth controlling exogenously for life expectancy at birth (see table 7) and schooling (see table 8). The author

recognises the potentially endogenous nature of life expectancy and schooling in regard to the economic growth process and addresses this issue in section five of the paper.

1.8 The econometric analysis used to model the 'malnutrition trap' (see section 4 and tables 9 and 10) is relatively distinct from the panel procedures discussed earlier and I'll review this method separately below. Given the issue of exogeneity, the next logical issue is the impact exerted by the nutritional variables given an endogenous treatment of life expectancy and schooling. This is done through the separate estimation of a two-equation structural model of economic growth and life expectancy, and a three-equation model of economic growth, life expectancy and schooling.

One key issue in the estimation of any structural model is the issue of identification. A simple way of explaining this issue is by reference to basic demand/supply models. The parameters of a demand equation are identified by reference to variables that shift the supply equation and vice-versa. This is sometimes known as the 'paradox of identification'. Thus, in general, in order to identify the parameters of one equation, variables excluded from that equation but relevant to other equations in the system are necessary as identifying instruments. Determining and choosing the appropriate instruments is always difficult, regardless of the application. Investigators can sometimes be *ad hoc* in their choice of instruments and it behoves investigators to demonstrate the robustness of their results to variations in the instrument set used. The identification restrictions used by the author in the structural models here are not free of criticism, as we'll see below in section 6. The IV estimation technique used by the author is based on the GMM procedure discussed earlier.

2. Part 1: Basic Empirical Regularities

This section focuses on determining the stylised facts inherent in the data. It is reasonably well done and thorough. The analysis raised some points, which I now turn to.

2.1. Sections 1.3 to 1.4 of the paper outline how simple neo-classical models of economic growth are amenable to the introduction of nutritional status variables. The direct mechanism by which nutrition exerts influence on economic growth is through its effect on labour productivity, which is relatively uncontroversial. However, increased levels of a country's physical capital stock and technological innovations also affect labour productivity. In terms of the empirics offered by the author, there is nothing convincing to suggest that the measures for nutritional status are not simply capturing omitted technological effects, capital stock effects, and/or the efficiency with which capital is employed. The nearest the author gets to tackling this issue is in table 2 when a proxy for a changing capital stock (i.e., the log of investment share) is introduced. It is noteworthy that the estimated capital share is attenuated significantly when the nutritional measures are added (see models (2) and (3)). It could be informative to undertake some basic 'old-fashioned' growth accounting with the estimated models (1), (2) and (3). In particular, it would be useful to determine how total factor productivity changes when the nutritional measures are introduced. One would expect change given nutritional status enhances human capital but if the change was implausibly large, then this suggests that the nutritional measure are capturing factors other than the assumed enhancement to human capital.

2.2. Section 1.5 of the paper examines the existence of non-linearities in the relationship between nutritional status and economic growth. Figures 3 and 4 do indicate evidence of a non-linear relationship between PFI and DES respectively. A quadratic approach is used to model the relationship between DES and economic growth. Although it seems to fit the data reasonably well (in a statistical sense) it can allow for too slow an effect on the upward part of the quadratic and too sharp a decline in the downward part. In terms of figure 4, there is some evidence that there are sharper returns to changes in DES over the early realisations of the variable than suggested by the quadratic. An alternative, and perhaps more appropriate, characterisation of the relationship would be to introduce piece-wise linear splines to capture the structure of the relationship more accurately.

In contrast to the DES measure, the third order polynomial for (100 – PFI) performs less well. I'm surprised throughout why the author does not follow the lesson learned in table 6 and use a logistic transformation to model this nutritional measure. This would provide a more parsimonious way of capturing the saturation effects inherent in this measure.

2.3. Section 1.5 also explores the existence of threshold effects in the relationship between economic growth and DES/PFI. This is done to ascertain if the effects of nutritional status interact with different levels of initial GDP in their effect on economic growth. The author finds this to be the case. However, the estimator used is the pooled OLS estimator (see table 3). I am not sure why the 'within' (or fixed effect) estimator is not used here as it is with the other models reported in this table. The author does not explain. I disagree with the author's conclusion in regard to the effect of the (100 – PFI) variable given the log of initial GDP. The overall marginal effect of (100 – PFI) on economic growth in model (6) is –0.002 + 0.001×[log of initial GDP]. This is likely to be positive for almost all values of initial GDP in the sample. The overall positive effect makes sense since a percentage point increase in (100 – PFI) translates as a percentage point reduction in the prevalence of food insecurity. Given the modelling assumptions of the author, we would expect this to have a positive effect on economic growth.

2.4. Why are non-linearities examined in isolation of the threshold effects? The thrust of the empirical analysis is that these effects can be explored in isolation. There is no reason why this should be the case. It is likely that non-linear effect also interact with initial levels of GDP. Why was this not explored?

2.5. An issue that does not appear to have burdened the author throughout the econometric analysis is the role of outliers. Figures 3 and 4 provide some potential insight into this issue. The units of measurement on the vertical axis appear different so it's difficult to get a feel for the dependent variable in regard to the standard error of the regression model. Nevertheless, there are a number of cases where (100 – PFI) is modest but growth is relatively high (see figure 3). Similarly, in using the DES measure, there are a number of cases where it is low but where the corresponding country's economic growth rate is high. What are the implications of these cases for the estimated residuals but, more pertinently, what are the implications of these potential outliers for the coefficient estimates on DES and (100 – PFI) and variants of these variables? They may or may not exert a substantive influence on the estimates. Use of a median rather than a mean regression might provide some insights into the role of outliers on the estimates.

3. Part 2: Robustness

This section focuses on evaluating the robustness of the estimation procedures used. In particular, an emphasis is placed on examining the impact of measurement error in the DES and PFI variables on economic growth. We noted earlier the motivation for the procedure but flagged a number of potential problems attached to using the procedure. Attention now turns to these issues.

3.1. The fixed effects estimation procedure used by the author attempts to solve the omitted variables and measurement error problems by first differencing and thus eliminating some of the variance that contaminates the OLS estimate. As noted in Jonhston and DiNardo, *Econometric Methods* (1997), there are certain circumstances where the 'cure' used by the investigator (i.e., the first differencing procedure) might be worse than the 'disease' (i.e., the measurement error in the DES/PFI variable) it is attempting to eradicate. The best way of looking at this problem is to simplify things a little relative to the earlier discussion, and specify the following equation:

$$y_{it} = \alpha_i + \beta x_{it} + u_{it}$$

and re-express $x_{it} = x^*_{it} + \pi_i$ where x^*_{it} is the true measure and π_i is the measurement error. In this case we could assume, again for purposes of simplicity $x^*_{it} \sim N(0, \sigma^2_x)$, $\pi_i \sim N(0, \sigma^2_\pi)$, and cov($x^*_{it}$, u_{it}) = 0. It can be shown in the case of a bivariate regression model (i.e., a model with one

regressor measured with error) that the OLS estimator is downward-biased (or attenuated) by the following factor:

$$\lambda = \frac{\sigma_\pi^2}{\sigma_\pi^2 + \sigma_x^2}$$

If the variance in the measurement error is zero (i.e., $\sigma_\pi^2 = 0$ and thus no measurement error in the regressor, then $\lambda = 0$), the OLS estimate is unbiased. If $\sigma_\pi^2 \neq 0$, the bias in the estimate is determined by the proportion of the total variance in x that is explained by the measurement error variance. The greater the measurement error, the greater is the bias. However, fixed effects estimation can exacerbate the bias. It can be shown that the downward bias in the first differenced version of the model is:

$\lambda = \dfrac{\sigma_\pi^2}{[1-\rho][\sigma_\pi^2 + \sigma_x^2]}$ where ρ is the correlation coefficient between the true x_{it} and its lagged value x_{it-1}.

In the type of model estimated by the author there is likely to be very little movement in DES/PFI across the decades but considerable variation across the countries. As a consequence, this would give rise to a relatively large positive ρ value.

The author makes no reference to this issue but one reason why the estimated effects for the DES/PFI increase by a factor of four or five (see p. 21) once the 'errors-in-variables' problem is addressed (*sic.*) as per table 4 could be more attributable to the higher correlation in the explanatory variables across time than any measurement error in the explanatory variables within a given time period. In that sense the larger estimates obtained could not be exclusively attributable to the measurement error in DES/PFI.

The effect of the measurement error on the first difference estimator can be evaluated by comparing first differenced estimates to estimates obtained with a longer difference filters (i.e., four or five time periods). In the absence of measurement errors, the first differenced operator should produce comparable results to the fifth difference operator, for instance. In the presence of measurement errors, the attenuation bias with the longer differenced filter should be less because the correlation coefficient between x_{it} and x_{it-5} should be smaller. Given the data used, this option is not available to the author.

3.2. Assume for the moment that measurement error is a legitimate issue here and that DES/PFI are contaminated by it to some extent. Then another aspect of the potential measurement error in DES/PFI overlooked by the author relates to the effect on other regressors included in the model (i.e., the variables in the Z_{it} vector described on p.20). The log of initial GDP is one such important measure and it does appear to feature in table 4 (albeit implicitly through the estimated annual rate of convergence reported). We could assume that log of initial GDP is measured without error. In such circumstances, if DES/PFI are measured with error then the estimate for this variable is also biased. In the simple two-variable case (e.g., DES and log of initial GDP), the magnitude of the coefficient bias on log initial GDP is given by:

– [correlation coefficient between DES and log of initial GDP]×
[bias in DES coefficient due to measurement error].

Thus, if DES and log initial GDP are positively correlated, as we might expect, and we know DES is downward biased, the estimated coefficient on log initial GDP is upward biased. The coefficient estimates reported, therefore, would be upward biased and suggest slower convergence than is actually the case.

The author could use this type of reasoning to inform on the presence of measurement error. For instance, what happens to the estimated coefficient on log initial GDP when DES or PFI is included in the standard regressions? In most cases, the changes in estimated coefficients on log of initial GDP are not consistent with introducing nutritional variables containing a large measurement error (see tables 2, 3 or 6). In most cases reported there is no material change in the estimated effects on the log of initial GDP.

It is difficult not to conclude that measurement error in DES/PFI is nothing more than a 'red-herring' and that the enhanced effects for the coefficients reported by the author using the GMM procedure are largely attributable to the strong correlations in the nutritional variables across the decades. In that sense they could not be construed as primarily reflecting the effects of mis-measurement.

3.3. The GMM IV estimation procedure used in table 4 assumes serially independent errors. The author offers no insights into whether this is a potential problem or not. The validity of the procedure rests on the assumption of independence so some insights here would have been useful.

3.4. The author notes that the third order polynomial in (100 – PFI) is not as robust when the 'between' estimator is used compared to the 'within' estimator (see table 5). One wonders whether this would be the case if the logistic transformation of PFI were used. In the context of such comparisons, I think it would be instructive to report the estimates for the 'random effects' model, as this is a weighted average of the 'between' and 'within' estimators. The author does not report this anywhere. The closer the estimator is the to 'random effects' estimator, the more accurate and efficient that estimator is. In regard to DES/PFI, this may provide some insights into the relative importance of the inter-decade ('within') and inter-country ('between') effects in regard to DES/PFI.

3.5. The author suggests that changing the dataset provides an alternative robustness check. This approach would be more persuasive if the nutritional status variables entered the growth models in a manner comparable to the earlier specifications. The DES enters linearly without any interactions to capture threshold effects and PFI enters within a logistic transformation. Under such circumstances, it is difficult to compare tables 1,2,3 and 5 with table 6. What happens if the quadratic in DES is introduced to the model in table 6?

4. Part 3: Transmission Mechanism

The estimates reported in tables 7 and 8 compare the effects of PFI and DES on economic growth controlling for life expectancy and schooling as exogenous regressors. The motivation for the inclusion of life expectancy and schooling (albeit separately) is to try and unpack the direct and indirect effects of nutritional status on economic growth. The results bring into sharp relief some differences between DES and PFI across the 'within' and 'between' estimators.

4.1. The author offers little explanation for the differences and provides little comment on the fact that life expectancy is statistically insignificant in all cases where the 'within estimator' is used. The obvious explanation is that, for the vast majority of countries in the sample, there is little variation in this measure across the three decades and hence the poorly determined coefficient. In contrast, there would be ample variation across or <u>between</u> countries – hence the well determined 'between' estimate in all cases. It would be informative to have some simple correlation coefficients for life expectancy and the nutritional status variables for the different transformations. These inter-correlations might be higher for the 'within' than 'between' and, for interpretational reasons, it would be useful to know the strength of these effects.

4.2. The author makes some claims about the insignificance of DES, which I don't think are supportable. The author finds that a linear treatment of DES is well determined using the 'within estimator' (model (6)) and insignificant using the 'between estimator' (model (5)). In contrast, the author finds that the quadratic treatment of DES is well determined using the 'between estimator' (model (8)) and insignificant using the 'within estimator' (model (9)). I don't think there is any

mystery in terms of this result and it is relatively intuitive. In modelling variation in economic growth rates within countries (over the three decades), one would actually anticipate DES to be linearly related to growth. However, in modelling variation in economic growth rates across or between countries, then one would expect some non-linearities in terms of the relationship. Thus, I don't think the author can make the type of inferences offered on the top of p.30.

4.3. Despite the rejection of the random effects model using the Hausman test (see bottom of table 7), it would again have been useful to see the estimates for this model to get a sense of the relative efficiency of the 'within' and 'between' estimators in this case.

4.4. I think the author is making a relatively spurious distinction between 'long run' and the 'medium run'. I think it inappropriate to classify the 'within' estimator as informing on the long-run and the 'between' estimator as informing on some medium run. What exactly does the author mean by the medium run? What is being held fixed in this medium-run, which is then relaxed in the long-run? The theory sketched by the author is all predicated within a long-run setting and the construction of the data is all based on the concept of the long-run. For instance, the decade averaging is primarily used to obtain long-run empirical measures. The distinction made by the author is not tenable. Given the data construction, both the 'within' and 'between' estimators are providing long-run estimates. They are just long-run estimates constructed from different types of variation within the data.

4.5. The bigger mystery to me in this section relates to why the PFI measure is more robust across the different estimators than the DES measure. The author offers no insight into this finding.

4.6. Table 8 reports the same type of analysis but this time with a schooling measure included. The quadratic effect in DES is better determined for the 'within' estimator in this case but still less well determined than the 'between' estimator. Again, it would be informative to have some simple correlation coefficients for schooling and the nutritional status variables for the different transformations. The intuition is that they would be considerably lower than for life expectancy and this would support the author's conclusion that the role of schooling in this analysis is a relatively minor one.

5. Part 4: Conditional Response

This section explores the case where nutritional growth traps can endogenously emerge. There is some merit in this analysis. In the type of empirical model estimated, the response of growth to nutritional status is conditioned on the nutritional status of the country's population. The underlying reasoning is that at low levels of consumption (as measured by high PFI or low DES), country's can get stuck in an 'under-nourishment growth trap'. The absence of a convergence effect is used to identify countries confined to such a trap. In addition, for countries outside the 'trap', the effect of nutritional status on economic growth should be zero.

The author proposes a three-equation model where there is one growth equation for each sector (i.e., the under-nourishment growth trap sector and the no under-nourishment growth trap sector). A switching or sorting equation is also specified that sorts the countries into one or other of the two sectors. The sorting is not done *a priori* and is based on whether the random variable in the sorting equation is above a certain index value threshold. The three-equation model is expressed as:

$y_{1i} = \beta_1 x_{1i} + u_{1i}$ [under-nourishment growth trap sector – high PFI]

$y_{2i} = \beta_2 x_{2i} + u_{2i}$ [no under-nourishment growth trap sector – low PFI]

$y_{3i} = \gamma z_i + v_i$ [sorting or switching equation]

In this model, y_{1i} and y_{2i} are the economic growth rates where the sub-scripts denote the sector. x_{1i} and x_{2i} are the observable explanatory variables that determine growth. There can be over-lapping

variables in the two sets. However, if the author's hypothesis is correct DES should feature in x_{1i} and not in x_{2i}. And, the log of initial GDP per capita, the convergence control, should feature in x_{2i} and not x_{1i}. y_{3i} is a latent (or unobservable) variable. However, we do observe z_i for all i countries and this includes the log of initial GDP per capita and PFI – the only two variables taken to determine sectoral attachment in this case. u_{1i}, u_{2i} and v_i are random error terms where $u_{1i} \sim N(0, \sigma_1^2)$, $u_{2i} \sim N(0, \sigma_2^2)$, $v_i \sim N(0,1)$.

The author assumes that a country belongs to the high PFI sector [under-nourishment growth trap sector] if:

$$\gamma z_i + v_i < 0$$

and to the low PFI sector [no under-nourishment growth trap sector] if:

$$\gamma z_i + v_i \geq 0$$

The author further assumes that $I_i = 1$ if the ith country belongs to the low PFI sector and $I_i=0$ if the ith country belongs to the high-PFI sector. Since the author assumes a probit structure for the attachment model, this can be expressed as:

$$\text{Prob}[I_i = 1] = \text{prob}[\gamma z_i + v_i \geq 0] = \text{prob}[v_i \geq -\gamma z_i] = \text{prob}[v_i \leq \gamma z_i] = \Phi(\gamma z_i)$$

where Φ is the cumulative distribution function for the standard normal. After some standard identification restrictions are imposed, the log-likelihood function for the model can be compactly expressed as:

$$\sum_{i=1}^{N} \text{Log}[[1 - \Phi(\gamma z_i)]\frac{1}{\sigma_1}\phi\left(\frac{y_{1i} - \beta_1 x_{1i}}{\sigma_1}\right) + \Phi(\gamma z_i)\frac{1}{\sigma_2}\phi\left(\frac{y_{2i} - \beta_2 x_{2i}}{\sigma_2}\right)]$$

where N is the number of observations, ϕ and Φ are the density and cumulative distribution functions for the standard normal, and σ_1 and σ_2 are the standard deviations for the two growth equations. $\Phi(\gamma z_i)$ captures sectoral attachment to the low PFI regime. The log-likelihood function essentially weights the density function for each sector's growth equation by the probability that a country will be attached to that sector.

The parameters of the model [β_1, β_2, γ, σ_1 and σ_2] are estimated by maximum likelihood techniques. In other words, the procedure determines the values of [β_1, β_2, γ, σ_1 and σ_2] that are most likely to have generated the data observed. Thus, we don't need to assign any observation (i.e., country) to a particular regime as the estimation procedure does this for us. We are then in a position to examine *ex-post* to which sector the empirical model has allocated the countries. I assume the author uses the empirical values $\Phi(\gamma z_i) \geq 0.5$ for attachment to low PFI and $\Phi(\gamma z_i) < 0.5$ for attachment to the high PFI sector.

Table 9 reports the estimates for the switching model. The switching equation's coefficients are not commented on but appear to make intuitive sense. For instance, a 10% increase in initial GDP per capita raises the probability of attachment to the low PFI sector by about one percentage point. A one-percentage point increase in PFI reduces the probability of attachment to the low PFI sector by about nine percentage points (on my assumption that PFI is expressed as percentage points and not as a proportion by the author).

The use of such an empirical model is innovative in this context and has some intuitive appeal. I think how it has been implemented raises some questions though. A major theme of the author's approach in the first three sections of the paper was a concern for the robustness of the relationships estimated. In this section, this concern has conveniently been laid to one side. The following comments raise some questions about the robustness of the switching model estimated.

5.1. Part two of this paper focused on the issue of measurement error in the PFI variable. The author is now silent on the implications of this problem for a model where separation is predicted on the basis of a variable assumed prone to some measurement error.

5.2. The robustness of the relationship between growth and nutritional status was explored in part two by reference to comparisons of 'within' and 'between' estimators. The model estimated here is most closely related to the pooled OLS case in terms of data structure. What happens to the estimated relationships if the data are transformed to eliminate country specific effects (i.e., use deviations from country means to exploit 'within' variation? What happens to the estimated relationships if country averages are used (i.e., analogous to the transformations used for the 'between' estimator)? One would have additional confidence in the results if they proved robust to these different transformations.

5.3. Why is PFI used to separate the sectors? What happens to the analysis if separation is on the basis of low and high DES? In other words, what happens if DES enters the switching model rather than PFI?

5.4. What criterion was used to determine variables included in the switching equation? This is unclear from the econometric analysis and one concern would be the sensitivity of estimates to variables included in the separation model. Is there any role for life expectancy in the analysis?

5.5. A problem I have always encountered with this type of switching model is that in many applications it predicts approximately about one-half to either sector. This is close to what is predicted in this paper. According to table 9 approximately 46% of observations are predicted to belong to the low PFI sector and 54% to the high PFI sector. This makes me feel uneasy and is many ways linked to the structure of the switching model. The estimates might be enhanced if more structure was actually imposed on the data. This would involve using priors to allocate some countries unambiguously to one of the two sectors. For instance, it is quite clear that over the time period considered a number of countries are unambiguously in the low PFI sector since they have a zero value for PFI. It makes sense to use this information to allocate these countries to the low PFI sector *a priori*.

If there are N countries in total, 1,…….,M are allocated by the model's separation equation and the remaining M+1 to N are allocated on the basis of common sense priors. This would sharpen up the procedure and the log-likelihood function would be re-expressed as:

$$\sum_{i=1}^{M} \text{Log}[[1 - \Phi(\gamma z_i)]\frac{1}{\sigma_1}\phi\left(\frac{y_{1i} - \beta_1 x_{1i}}{\sigma_1}\right) + \Phi(\gamma z_i)\frac{1}{\sigma_2}\phi\left(\frac{y_{2i} - \beta_2 x_{2i}}{\sigma_2}\right)] +$$

$$\sum_{i=M+1}^{N} \log[\frac{1}{\sigma_2}\phi\left(\frac{y_{2i} - \beta_2 x_{2i}}{\sigma_2}\right)]$$

On p.39 the author reports the success and failures in moving from high PFI and low PFI. The result in regard to Panama and Chile is not explained and appears counter-intuitive. One possible reason why Panama and Chile are re-classified as belonging to the high PFI regime in the 1980s could be because there is inadequate structure being imposed on the likelihood function.

6. Part Five: Endogeneity and Structural Estimation

In this section the author outlines two structural models in which a growth equation is combined with equations that provide an additional conduit through which nutritional status affects growth. The transmission mechanism equations are life expectancy and schooling. The first structural model couples an economic growth and life expectancy equation and the second structural model augments this through the inclusion of a separate schooling equation. The GMM method is used

and the estimates for the two-equation structural model are reported in table 11 and those for the three-equation model in table 12. The data used are simply pooled data and no attempt is made to test the robustness of the relationships across different estimators as was done with the earlier models. This represents a return to the basic pooled OLS models from table 1. Thus, no concession is made to country-specific heterogeneity aside from the inclusion of controls for Africa and Latin America. This represents one significant limitation of the structural modelling exercise. I now turn to some specific points about this part of the paper.

6.1. The main concern I have in regard to the structural modelling is the assumption that DES is exogenous. In the introductory section of the paper, the author notes the tradition in the early literature of exploring the effects of growth on nutritional status. The author's claim to innovation is to 'flip over' the equation and explore the effect of nutritional status on economic growth. The author makes no effort to defend the empirical treatment of DES (or PFI) as exogenous. Though it is worth noting that's not how PFI was treated in part four of the paper when under-nourishment growth traps were explored. It was treated endogenously. I have little problem with the notion that nutritional status affects economic growth. Equally, I have no difficulty with the notion that economic growth impacts on nutritional status. One is left feeling that a potential simultaneity bias problem is being brushed under the carpet. I would go so far as to say that unless this particular dimension of the problem is tackled in a robust and rigorous manner, the econometric estimates of the effects of nutritional status on economic growth reported are unconvincing.

6.2. The author provides no justification for the use of DES over PFI in the structural. Why the preference for DES now?

6.3. In terms of system (2) in table 11, the parameters of the growth equation are identified by the inclusion of a variable capturing the number of physicians per 10,000 of the population in the life expectancy equation. This is assumed to be independent of the growth rate but is an important determinant of life expectancy. This is relatively intuitive and the author notes its insignificance in the economic growth equation (p.45). However, the parameters of the life expectancy equation are exactly identified by reference to the quadratic term in DES. There are two concerns with this. The first concern is that from table 7 we know that the quadratic treatment of DES performs poorly in the economic growth equation once we control for country-specific effects (i.e., using the 'within estimator'). This suggests that the identification restriction for the life expectancy transmission mechanism equation is unlikely to be robust. The second concern is the extent to which it's plausible to treat any transformation of DES as an exogenous instrument given that we know it is determined by economic growth.

6.4. In terms of system (3), the parameters of the growth equation are now over-identified since, in addition to physicians per 10,000, the Africa dummy now shifts life expectancy but does not feature in the growth equation. However, the identification of the life expectancy equation still relies on the quadratic term in DES. The estimates remain relatively suspect for reasons outlined above.

6.5. I suppose I find it somewhat implausible that the GMM life expectancy coefficient in the economic growth equations in table 11 is totally insensitive to the introduction of a very potent instrument (i.e., the Africa dummy). The estimated coefficient is identical in system (2) and system (3) and its estimated variance reduces.

7. Part 6: The Efficiency Cost of Hunger: A Quantitative Assessment

In this section the author reports a summary of the quantitative effects of nutritional status on economic growth drawn from the variety of estimation procedures used. Tables 14 and 15 provide the summarised results from the PFI measure and the DES measure respectively. For the full sample the estimates for the former range by a factor of 8.6 and for the latter the range is by a factor of 20.7. If we exclude the GMM estimates designed to correct for measurement error, the factors are located in a more modest range of between 4 and 5. These are still relatively high. However, after all the econometric efforts reported, the author decides to use simple pooling

results and simple country-specific fixed effects estimates to summarise the quantitative analysis. This summary is reported in table 16.

7.1. I'm not sure I understand the rationale for reducing the quantitative effects by 20% in table 14 simply because PFI is over-estimated by 20%. Firstly, if PFI is over-estimated by 20%, it should be reduced by something less than 20% to return to a more accurate level. Secondly, if every country's measure of PFI is over-estimated by a fixed factor, this will show up as a scaling difference in the estimated coefficient on PFI. For instance, if I reduce all values of the explanatory variable on x by 16% the estimated coefficient on x will rise by approximately 20%. It's not clear to me why the author has attenuated the effects of PFI on economic growth by 20% because PFI itself is under-estimated. This requires some clarification.

7.2. The type of counterfactual exercise undertaken in this section always worries me. In some sense table 16 is trying to demonstrate what would happen to economic growth if DES increased to 2,777 kcal for the high PFI countries. The estimates suggest that this would translate into annual growth rate increases of just over one percent. In the type of analysis offered by the author here, and throughout the paper, DES is being treated as an exogenous variable. The underlying issue is that deficiencies in DES represent the outcome of the interactions of a number of complex processes. The deficiencies in nutritional status largely reflect the incidence of poverty in a particular country. Policies that reduce the incidence of poverty should also exert a positive effect on DES. These polices can have many dimensions. For instance they may include nutritional policies designed to expand food supplies through increasing production and reducing the capacity for food losses, – and if carefully designed may increase and stabilise rural household incomes – educational policies that influence household preferences for more nutritional food types, and that inform on health and hygiene issues, health policies designed to increase access to clean water and sanitation, economic polices that help develop markets and provide greater access to food markets and other markets for individuals and households. This does not mean that there is not a role for direct intervention through nutritional projects but it is perfectly conceivable that such direct interventions, though enhancing nutritional status, may exert little impact on economic growth. The above highlights just how complex the process may be and emphasises the obvious endogeneity of nutritional status. Economic growth represents one channel through which effects on nutritional status are mediated. Any econometric treatment of the process that fails to recognise this particular endogeneity can't expect to make a worthwhile or durable contribution to the literature.
