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Controversies in Nutrition and their Implications for the Economics of Food

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I Introduction

Economists have had a long-standing interest in matters of nutrition. Many of their concepts and policy deliberations are based on ideas borrowed from nutrition science. For example, the concept of absolute poverty has, at least since the turn of this century, been explicitly defined as the failure to satisfy a minimum standard of nutritional requirement (Rowntree, 1901). To the extent that such failure constrains the physiological and behavioural performance of human beings, it also has a bearing on the welfare-theoretic notions of 'functioning', 'capabilities' and 'quality of life' (Sen, 1985). Moreover, practical application of these concepts often requires the use of anthropometric indicators of nutritional status and the comparison of observed status with some desirable 'standard'. Finally, both 'requirement standards' and 'nutritional status indicators' are also of relevance in formulating food and nutrition policies in so far as they provide a tool for assessing the incidence of food deficits and nutritional deprivation among population groups (FAO, 1985; World Bank, 1986).[1]

In all these matters the economists of course rely on what they perceive to be the nutrition profession's best judgement on how to set up 'requirement norms' and 'status indicators'. In recent years, however, it has become increasingly apparent that the 'best judgement' depends

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1. Some formulations of the positive theory of labour market also have nutritional foundations. Dasgupta and Ray (1986) have recently taken a fresh look at this issue.

very much on which authority an economist happens to turn to. The differences do not merely consist in the magnitude of this or that 'requirement' or 'standard'. More basic disagreements on the very foundations of concepts and theories are also involved. Naturally, economists cannot help looking ruefully across the border as they feel the loss of bearing for some of their own concepts and theories. This paper is an attempt by a concerned economist to regain his own bearings in an alien territory which is apparently in a state of flux.

Our main objective however is not to resolve the basic disagreements; even to attempt such a task would be impertinent for an outsider. Our interest really is a rather parochial one: we wish to assess whether the current controversies in nutrition literature warrant any serious rethinking in two specific areas of interest to the economists, viz. (i) the methodology for assessing poverty and capability and (ii) the orientation of food policy in the developing world. It has been claimed by most protagonists in the 'Great Nutrition Debate' that much is at stake in both these areas depending on how one perceives the concepts of requirements and standards. This is then where our enquiry will begin: differing perceptions of these basic nutritional concepts and the presumed scientific basis of these perceptions.

In Section II, we take up the concept of 'energy requirement' of an adult and the controversies surrounding its definition and measurement.[2] Issues relating to the

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2. The focus on energy, to the exclusion of all other nutrients, is consistent with the prevailing view that energy deficiency is the most pervasive form of nutritional deficiency in the developing world. It is also appropriate in a discussion from the perspective of poverty because it is arguable that poverty has a more direct impact on the intake of energy than of any other nutrient (Osmani, 1982, p. 112).

'anthropometric standards of nutritional status' and the related question of energy requirement for children are discussed in Section III. Next, in Section IV, we look at the relationship between food intake, non-food environment and physical activity, and their implications for assessing nutritional status. The insights developed from these sections are then brought together in Section V to shed light on the question of our primary interest: what changes, if any, are warranted in our approaches to poverty, capability and food policy? Finally, Section VI offers a brief summary and a few concluding remarks.

II Energy Requirement of a 'Reference' Adult

The Concept of a 'Standard'

The human body requires energy for performing both internal functions (chemical as well as mechanical works within the body) and external work on the environment. The required energy is supplied mostly by the food consumed; but, if necessary, additional supplies can also be obtained by burning up reserve stores of energy inside the body. Conversely when the ingested food yields more energy than is expended by the body, the excess energy is stored inside. Any imbalance between intake and expenditure thus causes an equivalent variation in energy stores. This is reflected mainly in a corresponding change of bodyweight, but partly also in the composition of the body i.e., in the proportion of different forms of energy stores such as fat (adipose tissue) and lean bodymass.

When a person is in a state of 'energy balance' i.e., intake equals expenditure, his bodyweight and composition remain unchanged; as such he can be said to be in a state of equilibrium. Most people in the world are actually found to be in a state of equilibrium in the sense that very few are perennially losing or gaining weight. However, different persons are found at different levels of equilibrium and the same person can move from one equilibrium to another over time.

Now one way of approaching the concept of 'energy requirement' may be to ask: how much energy is required to maintain an equilibrium? The answer obviously is, the intake associated with the lowest possible equilibrium level. Clearly, if 'requirement' is defined in this way, then most people would be found to be meeting their requirement; only the miniscule proportion who are perennially losing weight would be considered to be failing to do so. But is the maintenance of equilibrium an adequate criterion for defining the requirement 'standard'? The answer depends on what meaning we wish to vest in the notion of a 'standard'. If it is merely intended to mean a level of adequacy that would ensure the survival of an organism, then certainly an intake that can sustain the lowest equilibrium would qualify to be a standard. But when nutritionists talk about 'standards', they usually aim at levels of adequacy that would ensure something more than survival. And quite rightly so. After all, the objective of being nourished is not merely to survive, but to achieve satisfactory levels of various functional capabilities (e.g. the ability to avoid disease or to perform physical activities) which depend upon the level of nutrition. If a 'standard' is supposed to ensure all these capabilities, then obviously mere maintenance of equilibrium will not do. One will have to ask 'which equilibrium', because different levels of equilibrium may be associated with different levels of functional capabilities. Thus the choice of a 'requirement standard' amounts to the choice of a particular equilibrium at which all the functional capabilities can be maintained at desirable levels.

Once it is known which equilibrium bodyweight (and composition) is consistent with full functional capabilities, it will then be possible to calculate what level of intake is required for maintaining that body-wieght. However, the specification of bodyweight is not enough, for the same bodyweight can be maintained at different levels of intake by adjusting energy expenditure through appropriate variation of physical activity. Therefore, the specification of 'required intake' also depends on the specification of a level of physical activity. Accordingly, the latest expert committee on nutritional standards offers the following definition. "The energy requirement of an individual is that level of energy intake from food which will balance energy expenditure when the individual has a bodysize and composition and level of physical activity, consistent with long-term good health and which will allow for the maintenance of economically necessary and socially desirable physical activity" (FAO/WHO/UNU, 1985, p. 12).

Several comments on this definiton are in order.

First, it espouses what may be called the <u>functional</u> view of requirement: the 'standard' is supposed to indicate the level of intake at which the set of nutrition-dependent functions achieve a satisfactory level. The contrast is with what may be called the <u>normative</u> view of requirement. For example, a society may decide, in view of alternative claims on scarce resources and its that judegement about the worth of alternative claims, that no more than a certain amount of resources can be devoted to improving the level of nutrition within the plan period. This will lead to a target level of intake, which may justifiably be viewed as a requirement norm in the planning context. Alternatively, a society may wish to achieve, perhaps in the long run, a certain level of intake that other, and presumably wealthier, societies have already been able to achieve. That target may again be taken as the requirement norm. As opposed to these normative views, it is the functionalist view that the nutritionists generally subscribe to.[3] It is not suggested that the other concepts are in some sense inappropriate, but merely that the search for a 'functional' standard is a legitimate scientific concern for the nutritionists. Accordingly, undernutrition, which is defined as the failure to meet the standard, is also viewed as a functional concept. A person is said to be undernourished if and only if he fails to achieve a satisfactory level of all the nutrition-related functions. Those who fail to achive 'normative' targets, not explicitly based on functional achievements, may be underfulfilled in some sense but not necessarily 'undernourished' in the sense of nutrition science. By the same token, those who fulfil a normative target are not necessarily well-nourished.

Secondly, while we have drawn a contrast between the 'functional' and 'normative' concepts of requirement, it should be pointed out that the 'functional' standard is not necessarily independent of normative judgements. For example, one group of nutritionists believes that it is futile to look for a level of intake at which <u>all</u> the functions will simultaneously reach a satisfactory level in some absolute sense. According to them, there is always a

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3. The matter is actually a little more complicated than that. A group of nutritionists allege that the traditional establishment view takes a normative approach by basing requirement norms on Western standards. On the other hand, the traditionalists argue that theirs is a truly functionalist view, while the heretics are alleged to aim for an uncacceptably low level of functional achievement. We shall come back to this debate (the so-called adaptationist debate) time and again. But suffice it to note at this stage that each camp, in its own judgement, is adhering to the functionalist view.

trade-off whereby some functions are enhanced at the expense of others.[4] Consequently, the specification of a standard perforce requires a normative judgement about the acceptable trade-offs (Pacey and Payne, 1984). In contrast, the establishement view by and large takes the position that it is possible to identify a standard at which all functions are simultaneously satisfactory in an absolute sense. However, they too recognise the role of normative judgements, specially with regard to the specification of a "socially desirable" level of activity. This is especially true in respect of 'discretionary' or non-economic activities on which different societies may have different norms and values as to what is a desirable minimum.[5] Thus one way or the other, normative judgements inevitably appear in the specification of a 'functional' standard. However, what still distinguishes it from the 'normative' standards, as defined earlier, is the fact that it is explicitly linked to the achievement of optimum levels of functions, whereas the latter may have some other goals in mind ('catching up with the West', for instance).

The contrast between functional and normative standards does however tend to get blurred when one takes the "planning goal" view of a requirement norm. We have

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4. For instance, it is argued that with high levels of intake, children may achieve a higher level of functions that accrue from a larger physical stature, but only at the risk of greater obesity which may have fatal consequences. There is thus supposed to be a trade-off between physical stature and longevity. We shall come back to this issue in the context of what is known as the 'Small but Healthy' hypothesis.

5. There is also a related issue of whether it is at all sensible (or possible) to allow for any positive level of activity in defining the requirement standard. Since this issue is related to the 'adaptationist' debate referred to earlier but not yet discussed, we postpone its consideration until a later section (Section IV).

described it as normative; but it may also have the functionalist property that it may represent an 'optimal' level of functions given the resource constraint as well as society's value judgements on the trade-offs between alternative claims on resources. Is it then justified to distinguish between normative and functionalist approaches? Some nutritionists (notably Philip Payne and his colleagues) argue that it is not. While being avowedly functionalist), they also insist that a nutritional standard cannot but be normative in the planning sense. They link it up with their view that trade-off between functions is unavoidable, and argue that the optimal trade-off can only be determined in the light of resource constraints and value judgements.

According to this view, there can be no unique functional standard even for a given society with unchanged value judgements - it will depend on the level of resources. As more resources become available, the standard will be raised. Thus the whole notion of undernutrition becomes resource-dependent (in addition to being dependent on value judgements, which, as we have argued, it must be).

We shall argue however that a functional standard does not have to be normative in the planning sense. More precisely, while value judgements are indeed unavoidable, one does not have to make the standard contingent on resources as well, even if one believes _à la_ Payne that trade-off between functions is inevitable. Given the nature of trade-offs and society's value judgements about them, one can define an _unconstrained_ optimum at which the functions would achieve the most satisfactory level from a particular society's point of view. This is the level of nutrition the society would aim for, if its objective was to optimise the level of functions. It is this unconstrained optimum that in an obvious sense qualifies to be the truly functional standard.

Of course, if the resources are too meagre to achieve the unconstrained optimum, it will certainly be legitimate to aim for a constrained optimum (one that is contingent on both resources and value judgements) as a target for the planning horizon. But it does not seem sensible to use the latter as the defining criterion of undernutrition. If the criterion were actually made resource-dependent, it will lead to the perverse result that more undernutrition may be observed at higher levels of resources, even if the functions of every individual happen to improve. Conversely, the simplest way to eliminate undernutrition will be to declare that no resources were available to improve the level of nutrition![6]

The particular feature of a resource-dependent criterion that seems to appeal to its proponents is the fact that it helps to isolate the most deserving cases (Pacey and Payne, 1984; Dowler et al., 1982). If $\hat{I}$ stands for the constrained optimum, then it follows from definition that the scarce resources can be best utilised by bringing those below $\hat{I}$ upto the level of $\hat{I}$, leaving aside for the moment all those whose functional levels lie above $\hat{I}$ but below the unconstrained optimum, I^*. The latter group, which is relatively better off, can be dealt with later, when the resource position improves. This is of course a perfectly sensible policy. But for this policy to operate, it is not necessary to call the latter group well-nourished and recognise only the former as undernourished. We can call all of them undernourished, grade them according to their degree of undernutrition (as measured by the distance

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6. Sen (1983) used the same argument to dispose of similar attempts to define poverty in relation to a resource-dependent concept of poverty line.

from I*) and pick up only the severest cases (those below $\hat{I}$) for remedial action during the plan period. In other words, while $\hat{I}$ is a perfectly legitimate selection criterion for immediate action, it need not at the same time be a defining criterion of undernutrition.

In sum, a functional standard is not independent of normative judgement, but unlike a 'planning goal' it should not be made contingent on resources as well. In the jargon of economics, the functional standard represents an unconstrained optimum, while the planning goal is a constrained optimum. The latter is a valid target for the plan period as well as an attractive selection criterion for immediate remedial action; but only the former is a sensible criterion of undernutrition.

The final comment regarding the definition relates to its domain of application - in particular, whether it is applicable to individuals or population groups. In principle, it should be applicable to every single individual, provided one can stipulate an appropriate bodyweight and activity level for the person and also find the minimum level of intake that could sustain the stipulated levels of weight and activity. In practice, however, it is extremely difficult to achieve this objective. There are a number of reasons for this.

First, even if all the relevant characteristics (age, sex etc.) of a person are fully specified, one cannot pin down a single bodyweight which is ideal for him. The nutritionists can only tell that the healthy individuals of a given type usually maintain bodyweight within a certain range. The average of this (fairly narrow) range is a useful basis for computing the average requirement of a group of individuals of a given type, but there is no basis for computing the specific requirement of a single individual.

Secondly, there is a similar problem with the level of activity. In principle, it should be possible to specify the desirable level of activity for any single individual. But when one is dealing with a large population, it may be practically infeasible to elicit the required information for every single person. The best one can do is to classify the population into certain major groups (in terms of age , sex and occupation) and specify an average level of activity for each group. Once again, one can only compute the average requirement of individuals of a given type. Thirdly, even when a particular bodyweight and level of activity are specified, one is not able to find a particular level of intake that is most appropriate. The nutritionists only know that a sample of healthy individuals of a given bodyweight and activity will be found to have intakes within a certain range, but no single level of intake can be declared as ideal. Once again, one can only say, provided the range is fairly narrow, that the average intake of the sample is a useful measure of the average requirement of the group.

Therefore, in practice, the standard is applicable only as an average requirement of groups of individuals of particular types. By considering as detailed a typology as possible one can reduce the error of approximation that is inevitably entailed by the use of an average. But one cannot pronounce (except in probabilistic terms) on the specific requirement and thus the nutritional status of a particular individual picked up at random.

The Schism: Variable Efficiency of Energy Utilisation

In actual practice, the 'requirement standard' is first postulated for a hypothetical 'reference person' who is defined as an adult male of a given age living in a given environment. The requirements of all the real people (or more precisely, types of people) are then obtained by applying appropriate conversion factors. According to the definition given earlier, the requirement of a reference adult will of course depend on what we specify to be a desirable bodyweight and his level of activity.[7] But once these are specified, do we know how to go about estimatin his requirement? This is the point where a major controversy begins to emerge, for there does not appear to exist a commonly accepted theoretical framework for estimating the requirement of a reference adult. There is of course a dominant 'establishment' view, but there is also a strong 'heretic' view which differs fundamentally from the prevailing orthodoxy.

The establishment view can be described as the 'fixed requirement' model, which postulates that once bodyweight and the level of activity have been specified, the energy requirement of a reference adult can be uniquely determined. It is of course granted that different individuals of the reference type may have different requirements (for reasons to be explained below), but it is believed that for any given individuals, there is only one level of intake which can maintain equilibrium at a given bodyweight and activity level.

The heretics the other hand argue that requirement is

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7. In practice, the desirable bodyweight is obtained by first taking the average height of adult males in a given community and then applying some norm of weight-for-height ratio. Note that in doing so, requirement gets related to desirable weight but actual height. This does not however mean that the nutritionists regard height to be a matter of indifference from the point of view of functional capability (as we shall see in Section III). It merely reflects the recognition that height cannot be changed in adult life so that there is little point in basing requirement on a norm of desirable height.

not fixed for any given person, but can vary intra-individually. That is to say, there is supposed to be a range of intakes within which a person can maintain his bodyweight and activity.

The crucial issue at stake is whether or not energy expenditure can vary when bodyweight and activity remain fixed. If expenditure also remains fixed, then obviously there is only one level of intake which can maintain equilibrium, and hence requirement can be said to be fixed. On the other hand, if expenditure can vary, then so can equilibrium intake, so that requirement can be said to be variable intra-individually. But how can expenditure vary?

Note that total expenditure of energy is made up of three components, viz. (1) the energy expended in doing essential internal works; it is called the basal metabolic rate (BMR), (2) energy expended in the performance of external physical activity and (3) a small component called the 'thermic effect of food' which accounts for the energy expended in absorbing the ingested food. Now the amount of internal work depends partly on age and sex. But it depends mainly on the amount of bodymass, because more work is obviously involved in a body of greater mass. It also depends to some extent on body composition since the metabolic activities of different types of tissues are not the same. The amount of external work on the other hand depends on the nature of physical activity, but partly it also depends on bodymass, specially in activities involving movement of the body, for the simple reason that more energy has to be expended in moving a greater mass. Thus the total amount of work done by the body would seem to depend on age, sex, bodyweight, body composition and the level of physical activity. But all these parameters are specified at given levels while defining a reference person, who therefore must be assumed to be doing the same amount of work all the time.

How then does the question of variable expenditure arise?

It arises simply from the proposition that the amount of energy expended per unit of work need not remain fixed, so that total expenditure of energy can vary even if the amount of work does not.[8] Underlying this proposition, and the controversies surrounding it, is a deep theoretical dispute on the process of "energy metabolism" i.e. the biochemical process of converting food into various forms of work done by the body. To clarify where the difference of opinion lies, it is perhaps best to begin with the common core that is not in dispute.

It is generally agreed that in order to obtain energy in usable form, the body has to convert food through a series of transformation, from one kind of 'energy store' to another. To give a simplified picture, when food is ingested, there first occurs what is known as intermediate metabolism. At this stage, energy is stored temporarily in several alternative forms such as glycogen, lipid (fat) and protein. When the body needs energy for work, these intermediate stores are further metabolised into high-energy bonds, like ATP (adenosine tri-phosphate), which are finally converted into work.[9] In each of these stages of

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8. Strictly speaking, even the amount of work need not remain constant for a reference person. This is partly due to the fact that even when physical activity is specified in terms of given tasks, the amount of 'work' (in the thermodynamic sense) may still vary due to subtle differences in the manner of performing a task. This possibility gives rise to the notion of variable 'ergonomic' efficiency. In internal work too, there is room for variation, in mechanical work such as sodium pumping and chemical work such as protein turnover, although to what extent such variation does in fact occur is strictly in the realm of speculation (Blaxter, 1985).

9. A brief exposition of the process can be found in Flatt (1978).

transformation (food → intermediate stores → ATP → work) some energy is inevitably lost. Therefore, it is necessary to make a distinction between the energy involved in a work and the energy expended in doing the work. In an overall accounting, total energy expended (which includes the energy lost in the process of transformation) is always greater than the energy actually involved in a work. The proportion of energy that actually gets utilised is called the 'efficiency of energy metabolism'.[10]

All this is common ground between opposing theories. Where the difference lies is on the issue of whether, and to what extent, the efficiency of energy utilisation can vary for a given individual. According to the establishment view, efficiency remains more or less fixed when bodyweight is held constant, as is the case with a reference person. As mentioned before, the amount of work done by a reference person may also be assumed to be fixed. Now if a fixed amount of work is done with a constant efficiency, then obviously total expenditure of energy must remain constant. Hence the notion of a fixed requirement.

The heretics accept that the amount of work may remain constant for a reference person, but they believe that efficiency can vary, so that total expenditure of energy can vary while equilibrium is maintained.[11] Hence the notion of variable requirement.

This difference in theoretical perspective leads naturally to different empirical estimates of energy

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10. As it happens, this efficiency is rather surprisingly low, no more than 33 per cent for physical work and even less for internal work (Blaxter, 1985).

11. Hegstead (1974) and Sims (1976) describe a number of possible ways in which efficiency can vary.

requirement. Historically, the requirement of a reference adult has been estimated in the establishment tradition by two alternative empirical procedures.[12] The most common procedure, which may be called the 'intake-based approach', is to observe the intakes of a group of healthy individuals of the reference type over a period of time and to take their average intake as the standard of requirement. The underlying assumption here is that since these people are apparently healthy, their intakes must be matching their requirements. In the alternative procedure, called the 'factorial approach', total requirement is estimated by adding up different components of energy expenditure (viz. BMR, physical activity and the thermic effect of food). In this approach too, the 'average' value of each component is estimated for a group of reference-type individuals.

The idea of taking 'average' is partly to smooth out random errors of measurement. But partly it also follows from the recognition that while efficiency, and hence requirement, is fixed for any particular individual, it may vary inter-individually among different individuals of the reference-type. Such variation is deemed to arise from inherent genetic differences in the efficiency of energy utilisation.

The heretics on the other hand argue that the observed variation in requirement among individuals is due not so much to <u>genotypic</u> variation in efficiency, as to <u>phenotypic</u> variation within the same individual over time. In other words, it is suggested that if different people of the reference-type are seen to be maintaining equilibrium at different levels of intake and expenditure, it is

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12. A brief historical sketch of the evolution of nutritional standards can be found in Harper (1985).

mainly because each of them is occupying a different point within the range of intra-individual variation of requirement.

From this perspective, Sukhatme has gone on to argue that if one is interested in setting a cut-off point for identifying inadequate intake, it should be set, not at the average value, but at the lower end of what he calls the 'range of homeostatis' (which means the range of variation in the requirement of the same individual). He postulates this lower limit to be 2σ below the 'average' (μ), where σ stands for the standard deviation of intra-individual variation in requirement.[13] Using this cut-off point, he was able to show that the incidence of nutritional poverty in India was less than half of what Dandekar and Rath (1971) had estimated earlier by using the 'average' norm. By sheer reconceptualisation of the requirement norm, the magnitude of poverty was cut down at a stroke from an enormous 46 per cent to a more innocuous-looking 15-20 per cent. Hence the furore over the concept and measurement of requirement.

In order to form a proper judgement on this matter, it is necessary to look very closely at the manner in which Sukhatme reached his remarkable conclusions. We shall try to do this in two steps. First we examine the foundations of Sukhatme's hypothesis that requirement varies intra-individually due to variable efficiency of energy utilisation. Next, we explore the logic of $\mu - 2\sigma$, given the

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13. Sukhatme has developed his argument through a series of papers. See Sukhatme (1977, 1978, 1981, 1982, 1982a), Sukhatme and Margen (1982) and Sukhatme and Narain (1982), among others.

hypothesis of variable requirement.[14]

The Theory of Autoregulatory Homeostatis

It will be helpful to bear in mind at the outset that the theory from which Sukhatme draws his conclusions is not primarily a theory of requirement at all. It is a theory of how long-term energy balance is maintained by free-living healthy individuals; it is called the theory of autoregulatory homeostatis.

One of the interesting facts of life which has intrigued applied nutritionists over the years is the way so many people manage to keep bodyweight within a fairly narrow range even in the developed world where energy intake is practically unconstrained by the limitations of purchasing power (Garrow, 1967). Various explanations have been offered to account for this remarkable phenomenon of weight homeostatis. Some nutritionists consider physiological mechanisms of appetite control to be the principal regulatory system (James, 1985). Many others believe that the answer lies in conscious control of energy expenditure (Beaton, 1984). Alternatively, Payne and Dugdale (1977) have postulated an internal feedback mechanism whereby random fluctuations in intake and expenditure lead to transient and self-correcting changes in bodyweight. But the most relevant in our present context is the old idea of 'luxusconsumption' which has recently been revived by Miller et al. (1967), Sims (1976) and others, by the name of 'dietary-induced

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14. Perhaps we should mention that we have found Sukhatme's style of reasoning eminently obscure and his arguments often difficult to follow. What we present below is a strictly personal interpretation of what we believe to be the logical core that lies beneath his writings.

thermogenesis' (DIT). According to this hypothesis, bodyweight is maintained by burning off excess calories i.e., by producing more heat (thermogenesis) than usual. Presumably, the opposite occurs when intake is in deficit. Such variation in thermogenesis is simply the mirror image of efficiency of variable energy utilisation.

Sukhatme and Margen (1982) endorse the 'thermogenesis' view that weight homeostatis is maintained through variation of efficiency, but in doing so they go one step farther. They postulate a model to describe the manner in which efficiency varies over time. The essence of the model can be stated in the form of the following proposition:

Efficiency of energy utilisation varies over time in an autocorrelated manner. If a healthy person is economically or otherwise unconstrained in his intake of calories and if he remains engaged at a fixed level of activity while maintaining his bodyweight, then his intake will also be seen to vary over time in an autocorrelated manner.

Formally, let I'_t denote the intake on any day t comsumed by an unconstrained healthy person maintaining bodyweight and engaged in fixed activity. Then, according to this theory, the temporal pattern of intakes can be shown by the following equation:[15]

$$I'_t = \rho I'_{t-1} + e_t \qquad (1)$$

Where ρ is the co-efficient of first-order autocorrelation and e_t is a random error.

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15. There seems to exist a good deal of confusion as to how Sukhatme actually deduced this equation. We give a brief account in the Appendix of what we believe to be the basis of this deduction.

Recall that the intake of a healthy person (maintaining bodyweight and activity) is by definition equal to his requirement. Therefore, equation (1) can also be interpreted as stating that the energy requirement of a person is not fixed, but varies from day to day in an autocorrelated manner.

Now the phenomenon of autocorrelation has three very important implications:

(i) An autocorrelated variable is generated by a stochastic stationary distribution i.e., a distribution with constant mean and variance. This implies that we cannot exactly predict what the value of I'_t will be on any particular day, but we know that the distribution from which it comes will remain unchanged, and in particular will have a constant mean, over time. Because of the constancy of mean, it can be expected that if a large number of observations are taken over time, the observed average of I'_t will approach the theoretical mean μ of the distribution. But what does μ stand for? Since variation in I'_t arises solely from variation in efficiency (with bodyweight and activity being fixed, and no constraint operating on the intake of calories), μ must stand for the level of intake and expenditure based on average efficiency. One can therefore say that the observed average intake of a healthy individual maintaining bodyweight and engaged in a fixed level of activity will be equal to the theoretical average expenditure (based on average efficiency). This is the theory of autoregulatory homeostatis. The idea of autoregulation consists in the fact that although efficiency varies from day to day, it does so in an autoregulated manner so as to keep average intake equal to average expenditure. We can thus see how autocorrelation - a statistical concept - implies autoregulation - a physiological concept, an inference that has met with considerable skepticism (e.g. Dandekar, 1982, p. 210; Waterlow, 1985, p. 3).[16]

(ii) Autocorrelation also implies that the variance of the mean of intakes I'_t does not decline rapidly to zero as the period over which the mean is taken is increased. Therefore, if mean is taken over a relatively short period, say a week, then the mean intake will have a significantly non-zero variance. In other words, the weekly mean intake will not as a rule be equal to μ, the 'true' mean. From Edholm´s (1970) data, Sukhatme has estimated that the coefficient of variation of weekly mean is in the order of 12-15 per cent. Recalling that intake equals requirement for the reference-type people one could therefore say that requirement varies intra-individually with a coefficient of variation of about 12-15 per cent (when the reference period is a short span of time, such as a week).

(iii) The variation of intake in Edholm´s data is due apparently to both inter- and intra-individual variation. But when the data was subjected to hierachical analysis of variance, taking note of autocorrelation in time series, it was found that most of the observed variation was actually due to intra-variation. This is the basis of Sukhatme´s contention that intra-individual (or phenotypic) variation overwhelms any truly inter-individual (or genotypic)

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16. Misunderstanding about the nature of the argument is very often the source of skepticism. For example, Dandekar (1982) has argued that if intakes are autocorrelated due to extraneous constraints, it would not indicate autoregulation. This is of course true, but beside the point. It has not been suggested that any autocorrelation implies autoregulation. Only that autocorrelation is relevant which is observed in the intakes of reference-type people who have an unconstrained access to food intake.

variation. In physiological terms it means that if requirements are found to vary in a population of reference individuals, it is not because each has a fixed but mutually different level of metabolic efficiency (as assumed in the establishment view) but because each happens to occupy a different point in the range of intra-individual variation at the time of enquiry.

Of the three implications of autocorrelation mentioned above, the first one, deducing the theory of autoregulatory homeostatis, constitutes the specific contribution of Sukhatme and Margen in the theory of weight regulation by healthy unconstrained individuals. Their theory can also be described as the model of stochastic regulation to denote the idea that bodyweight is regulated over time through stochastic variation of efficiency.

The last two implications are by-products of this theory, which Sukhatme utilises in the process of justifying his cut-off point of $\mu - 2\sigma$ in the debate over assessment of undernutrition. We should however point out at this stage that there are really two Sukhatmes when it comes to justifying $\mu - 2\sigma$. We shall refer to them as Mark I and Mark II versions. The Mark I version follows straight from the model of stochastic regulation just described. But the Mark II version virtually abandons this model and embraces what may be called the model of adaptive regulation. This mutation is defended on the ground that the latter model presumably follows from the former, but as we shall see this is not quite so.[17]

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17. Mark I is of chronologically earlier vintage, being most visible in the publications of 1977 and 1978. Mark II is more a product of the Eighties, developed largely in response to the criticisms of Mark I.

Mark I Rationale of $\mu - 2\sigma$: Stochastic Regulation

In the conventional approach, a person is identified as undernourished if his mean intake (m) over a sample period (say, a week) falls short of some norm of average requirement (μ) i.e., if $m < \mu$. But this inequality can be shown to be a poor criterion of undernutrition if the theory of stochastic regulation is accepted. Recall that μ is obtained in practice from inter-individual average of requirements over a sample period. But according to the third implication of autocorrelation mentioned above, these inter-individual differences are really the reflection of underlying intra-individual variation. Therefore μ can be taken to be an estimate of intra-individual average requirement over the long-term.[18] Accordingly, the inequality $m < \mu$ can be interpreted as a person's weekly intake falling below his long-term requirement. Now we know from the second implication of autocorrelation that the weekly intake of a healthy unconstrained person may easily deviate from his long-term requirement. Therefore it will be wrong to treat everyone with m less than μ as undernourished.

One really has to distinguish between two separate cases that may generate the inequality $m < \mu$: (i) the case of unconstrained healthy people eating less in the sample period simply because their requirement has fallen below μ in the normal course of intra-individual variation, and (ii) the less fortunate case where the inequality reflects some constraint on intake. The first case is evidently not one of undernutrition, for although the observed intake is less than μ, the 'expected' intake over the long-term will be

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18. Assuming of course that the distribution of requirements is the same for everyone.

equal to μ (by the first implication of autocorrelation); in the words of Sukhatme, the variation in intake is in 'statistical control'. However, in the second case, where the intake is constrained, there is a possibility of failure in 'statistical control' i.e., 'expected' intake may turn out to be less than μ. Only such cases should be treated as undernutrition.[19] We therefore need a criterion for determining who are and who are not in 'statistical control' of intake variation over time. But how do we find such a criterion?

There seems to be some confusion as to how Sukhatme actually approached this problem. Dandekar (1981), writing in a critical vein, and Srinivasan (1981), taking a sympathetic view, have both interpreted Sukhatme to be posing the problem in the framework of a statistical test of hypothesis. The problem is conceived as follows: Given that a person's weekly intake (m) varies over time, how can we judge whether or not his long-term average intake $\bar{m}$ is equal to his requirement μ? The usual statistical procedure is to assume that $\bar{m}$ is in fact equal to μ, and to reject this hypothesis <u>only if</u> the probability of its being correct is very low. Now the probability of its being correct will be as low as 5 per cent or less if m happens to be below 'μ minus two standard deviations' (assuming that intake is normally distributed). Therefore, those with intake below this cricital limit could be treated as undernourished. It is thus that the cut-off point of $\mu - 2\sigma$ is supposed to have been derived.

But this does not seem to be the correct interpretation of Sukhatme. In this interpretation, the critical limit of

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19. "Clearly, undernutrition must be defined as the failure of the process to be in statistical control" (Sukhatme, 1978, p. 1383).

'μ minus two standard deviations' must be derived by using the standard deviation of <u>intake distribution</u>. In contrast, the entity σ in Sukhatme's criterion of $\mu - 2\sigma$ refers to the standard deviation of <u>requirement distribution</u>. It is true of course that σ will also give the standard deviation of intake for the healthy unconstrained people, but not for everyone. For those whose intake is somehow constrained, σ will in general be different from the standard deviation of intake. Thus the criterion $\mu - 2\sigma$ does not emerge from the test of hypothesis framework.[20]

Also note that the problem of variable requirement, which is so central to Sukhatme's concern, is not relevant to this framework at all. Everyone could have the same requirement Θ, constant over time, and yet if intake is based on a short sample period it will be necessary to ask whether $\bar{m}$ can be expected to equal Θ, knowing only the sample observation m. In short, the test of hypothesis framework is essentially concerned with the <u>general</u> problem that intake may vary over time for a variety of reasons so that one week's data may not reveal the average intake of <u>anyone</u>; whereas Sukhatme's concern lies in the <u>specific</u> case of <u>physiological</u> variation in efficiency which will create a divergence between weekly intake and long-term requirement of <u>healthy unconstrained individuals</u>. His problem was to ensure that such people do not get counted as undernourished.

Faced with this problem, Sukhatme's strategy was to look for a characteristic feature of the intakes of healthy

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20. Srinivasan (1981, p. 7) recognises that σ will not be appropriate for everyone, but does not apparently see this as rendering the 'test of hypothesis' framework an incorrect interpretation of Sukhatme.

unconstrained people. This feature was then to be used as the criterion for distinguishing the well-nourished people from the undernourished ones. The particular feature that he used for this purpose was the "range of homeostatis" i.e., the range of variation within which the intake of a healthy person can be expected to lie. As it happens, 95 per cent of the healthy people can be expected to have intake within the range $\mu \pm 2\sigma$, assuming that efficiency varies in a normal distribution. If intake falls outside this range, one can be reasonably confident that it is not the intake of a healthy person. Thus argues Sukhatme (1978, p. 1383), "Clearly, most individuals in health in the framework of this model will have an intake between $\mu \pm 2\sigma$. It follows that the proportion of individuals below the lower critical limit may be taken to represent the estimate of the incidence of undernutrition". This is the Mark I rationale for using μ - 2 as the criterion of undernutrition.

There is however a serious flaw in this logic. In order to claim that undernutrition occurs <u>only</u> when intake falls short of $\mu - 2\sigma$, it must be established that <u>everyone</u> with intake in the range of homeostatis is well-nourished. But Sukhatme´s premise (the first proposition in the quotation above) does not ensure that. It only ensures that if a person is healthy his intake will lie between $\mu \pm 2\sigma$. From this it does not follow that anyone who has intake within this range is necessarily healthy. For example, if some extraneous constraint on intake rather than variable efficiency brings down someone´s intake in the sample period below μ (albeit above $\mu - 2\sigma$), and if the nature of the constraint is such that it is going to persist over time, then clearly average intake will not equal μ even in the long run. Such people must be considered undernourished by Sukhatme´s own criterion viz. the failure of 'statistical control'. Consequently, the cut-off point of $\mu - 2\sigma$ will in general lead to an underestimate of total undernutrition.

The mistake basically lies in the failure to see that to have one's intake within the range of homeostatis is only a necessary condition for being well-nourished. It is by no means sufficient. Yet it is sufficiency that is needed to justify the claim that everyone with intake above $\mu - 2\sigma$ should be considered well-nourished. The failure of 'Mark I justification' is thus seen to lie in an elementary confusion between 'necessity' and 'sufficiency'.

Elementary as it is, this flaw in logic has seldom been noticed, perhaps because the argument has been misinterpreted by friends and foes alike.[21] Instead, Sukhatme's advocacy of $\mu - 2\sigma$ has often been indicted for the wrong reasons.

For example, it has sometimes been suggested that the choice of $\mu - 2\sigma$ reflects a particularly unappealing kind of value judgement. This type of argument originates from the habit of looking at the problem in the test of hypothesis framework. As we have seen, the procedure in this framework is to start with the premise that there is no undernutrition (i.e., $\bar{m} = \mu$), and to reject it only if the probability of its being true is extremely small. It "... means that we shall not accept the existence of undernutrition unless the evidence is overwhelming" (Dandekar, 1981, p. 1248). This would indeed appear to be a rather odd kind of value judgement, specially when one notes the deep social concern with the problem of undernutrition that motivated the search for cut-off points in the first place.

The value-judgement-based critique has also at times arisen from the mistaken belief that the cut-off point of $\mu - 2\sigma$ was intended to take care of the problem of

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21. One exception is Mehta (1982, p. 1335).

inter-individual variation in requirement.[22] The argument briefly is as follows.

When requirement varies from person to person, the choice of any single cut-off point will give rise to two opposite kinds of errors. Some people whose intake is less than the cut-off point but greater than their respective requirement will be wrongly classified as undernourished (Type I error). On the other hand, some people whose intake is greater than the chosen point but less than their respective requirement will be wrongly classified as well-nourished (Type II error). Clearly, there is a trade-off between the two types of error; a cutt-off point that reduces one type of error inevitably raises the other. Therefore, the choice of a cut-off point must depend on the relative importance one wishes to attach to avoiding the two types of error. If one's value judgement was such as to attach a high premium on capturing the truly undernourished people, one would use a high cut-off point even though it meant wrongly counting some well-nourished people as undernourished. On the other hand the choice of a low cut-off point like $\mu - 2\sigma$ implies a strong desire to avoid calling a person undernourished when he is not so, even though it raises the probability that we shall fail to call a person undernourished when he is not so. The value

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22. Dandekar (1981, p. 1248), for one, seems to commit this error. In all fairness, however, it ought to be pointed out that Sukhatme himself sowed the seeds of confusion when he initially described σ to be composed of both inter- and intra-variation (Sukhatme, 1978, p. 1383). But later, in response to the criticisms of Dandekar (1981), Krishnaji (1981) and others, he tried to salvage the matter by invoking the (third) implication of autocorrelation that the apparent inter-variation is due almost entirely to intra-variation, so that all of σ could be taken to reflect intra-variation (Sukhatme, 1981, p. 1321 or Sukhatme, 1981a, p. 1035).

judgement implicit in this choice does seem to betray a lack of social concern with the problem of undernutrition. This is indeed a valid point, but not one that applies to Sukhatme, because his choice is based on the logic of intra- not inter-individual variation in requirement.[23]

The problem with his choice lies in faulty logic, not in value judgement. As we have noted, his error consisted in wrongly deducing from the theory of stochastic regulation that <u>everyone</u> with intake above $\mu - 2\sigma$ can be considered well-nourished.

At this point enters Sukhatme Mark II. He brings in the notion of 'adaptation' to justify the proposition that <u>everyone</u> with intake above $\mu - 2\sigma$ should indeed be considered well-nourished.

<u>Mark II Rationale of $\mu - 2\sigma$: Adaptive Regulation</u>

Before coming to the structure of the argument, it is necessary to make a few remarks about the concept of adaptation. It is one of the most elusive concepts in the whole of nutrition science, as different people seem to use

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23. However it does apply to an earlier incarnation of Sukhatme when σ actually referred to <u>inter</u>-individual variation among reference individuals (Sukhatme, 1961). Curiously, he now refers to this earlier work too as <u>implicitly</u> using the notion of <u>intra</u>-variability (Sukhatme, 1982a, pp 14-16). This is strange because the value of σ was derived there entirely from the difference in requirements due to activity differences. It had nothing to do with <u>intra</u>-individual variation, nor even with genotypic <u>inter</u>-individual variation due to different efficiencies of energy utilisation. Why nevertheless Sukhatme likes to hark back to bygone days to conjure up a thematic continuity that does not exist remains a source of abiding mystery.

it to mean quite different things.[24] It is therefore essential to be completely clear about the sense in which the word 'adaptation' is used in the present context.

In the broadest possible sense, adaptation can be described simply as an organism's strategy to survive in response to an adverse environment, for instance, a reduction in food intake. In this general sense, adaptation may take various forms covering time spans of varying lengths. It may, for example, involve long-term genetic adaptation through the Darwinian process of natural selection in which only the genotypes less demanding in their energy requirement come to survive. But individuals can also adapt in their own life span (phenotypic adaptation). This may again involve a relatively long-term somatic adaptation in which the rate of growth of body is reduced in response to lower food intake (such adaptation is discussed in Section III), or it may involve fairly short-term responses. These short-term responses may again be of at least three kinds, involving adjustment of either physical activity or bodyweight (and composition) or efficiency of energy utilisation.

While all these adaptive mechanisms help an organism to survive in a stressful situation, not all of them are altogether costless in terms of various functional capabilities. There is in fact a great deal of controversy over which of these mechanisms, if any, enables an organism to adapt in a functionally costless manner, and if so,

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24. A whole conference devoted to clarifying the concept and mechanisms of Nutritional Adaptations in Man has failed to produce a generally agreed definition. Compare the introduction and post-script written by Waterlow (1985, 1985a).

within what limits. Sukhatme proposes that variation in efficiency offers an avenue for costless adaptation to low food intake (upto a point). On this he builds the Mark II rationale for $\mu - 2\sigma$. We shall see in a moment how this was done; but it is first necessary to clear up one confusion that has needlessly obscured the main issue.

It has not always been appreciated that Sukhatme's argument is based exclusively on pure adaptation in efficiency, unaccompanied by any other adaptive mechanism. Thus Gopalan (1983) bases his critique on the wrong premise that a person permanently subsisting at the lower end of Sukhatme´s range of adaptation would have exhausted all adaptive possibilities. In particular, he believes that bodyweight will be reduced to such a low level that, unlike a healthy person, an 'adapted' person will no longer be able to take further advantage of the important regulatory mechanism of adjustment in bodyweight when faced with temporary shortfall in intake. He further argues that such adaptation "may help the victim to avoid the catastrophe of death, but ... will not help him to live a normal life of activity and productivity" (p. 598). Not surprisingly, the idea of calling such people wellnourished appears to him to be an unacceptable 'policy of brinkmanship'. However, the fact is that the idea of adaptation through reduction of bodyweight does not play any role in Sukhatme´s advocacy of $\mu - 2\sigma$. It could scarcely have been otherwise in view of his sole concern with the requirement of a reference person maintaining bodyweight and activity.[25]

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25. This confusion is blown up into absurd proportions by Zurbigg (1983) who launches a rather spiteful ideological critique from the premise that Sukhatme´s theory was all about reducing requirement because reduction in bodyweight did not matter.

Perhaps the confusion arises from Sukhatme´s show of solidarity with Seckler´s 'small but healthy' hypothesis (which we discuss in Section III). But it should be noted in the first place that even Seckler´s hypothesis is not concerned with adaptive changes in bodyweight from a given level; it has to do with adaptation in the rate of growth among children. Secondly, while it is true that Sukhatme combines Seckler´s hypothesis with his own to present a grand vision of what he calls 'the process view of nutrition', his own methodology for the measurement of undernutrition does not allow for Seckler´s hypothesis. Thus the 'conversion factors' between adults and children were estimated on the basis of the same requirement norms for children which everybody else used for India, instead of scaling them down to take note of adaptation in physical growth. And the logic of setting the cut-off point 2σ below μ was derived solely from adaptation in efficiency, using the following kind of argument.

If efficiency increases adaptively as food intake is decreased, then the body can do the same amount of 'work' as before without drawing upon energy stores. Thus adaptation in efficiency can enable a 'reference' person to maintain bodyweight and activity, within a range of energy intake. Accordingly, no single intake can serve as the requirement of a reference person. In fact, he could be said to be meeting his requirement at any intake within the 'range of adaptation'. Only when the intake falls below the range of adaptation that a person can be said to be undernourished. As Sukhatme (1982a, p. 38) explains, "However a point is reached in the intake of food below which BMR gets depressed and the body is forced to part with its fat in favour of the vital need to maintain body heat. This is the point of minimum physiological requirement or clinical undernutrition". Then he suggests that the range of adaptation is given by $\mu \pm 2\sigma$, which he had earlier described as the range of autoregulation, and hence argues that $\mu - 2\sigma$ is the appropriate cut-off point for measuring undernutrition.

Now the description of $\mu \pm 2\sigma$ as the range of adaptation, and not merely as the range of autoregulation, is a particularly significant departure. We have seen that in the autoregulation framework one could only say that the intakes of healthy people will mostly fall within the range of $\mu \pm 2\sigma$. One could not assert the reverse, and that created a logical problem in accepting $\mu - 2\sigma$ as the cut-off point. But it's different if $\mu \pm 2\sigma$ is viewed as the range of adaptation. Give a man any intake within this range; his efficiency will adapt accordingly to bring expenditure into equality with his intake, without any effect on bodyweight and activity. If that is so, then <u>everyone</u> with intake (m) above $\mu - 2\sigma$ must be considered adequately nourished.[26] In other words, the inequality $m > \mu - 2\sigma$ is now both <u>necessary</u> and <u>sufficient</u> for a person to be well-nourished. The logical flaw in the Mark I rationale thus seems to be neatly avoided in the Mark II version.

But this achievement is more apparent than real. It is true of course that if $\mu - 2\sigma$ could be establised as the limit of adaptation, it would indeed be the correct cut-off point. But as we have noted, $\mu \pm 2\sigma$ was originally described as the 'range of autoregulation' i.e., the range within which the intake of a healthy <u>unconstrained</u> person can be expected to lie. In contrast, the 'range of adaptation' refers to a range within which a person can costlessly adapt

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26. "It follows that a person <u>must</u> be considered in energy balance <u>whenever</u> his intake falls within homeostatic limits of balance" (Sukhatme, 1982a, p. 39; emphasis added).

even if his intake is constrained to deviate from the average. They are apparently not the same thing. Yet Sukhatme assumes that the two 'ranges' are exactly the same; and in doing so, he takes recourse to the argument that autoregulation implies adaptation.[27] But we shall argue that autoregulation does not imply adaptation and that a fortiori the range of autoregulation cannot be identified with the range of adaptation.

Sukhatme makes the transition from autoregulation to adaptation by hinging on the fact that both phenomena are characterised by a common mechanism - namely, variation in the efficiency of energy utilisation. But this transition is not logically valid because the qualitative nature of 'efficiency variation' is not the same in the two cases. In particular, the causal relationship between intake and efficiency are exactly the opposite. In autoregulation, "... a healthy individual varies his or her intake, increasing it when wastage is larger and decreasing it when it is lower" (Sukhatme and Margen, 1982a, p. (109). But in adaptation the line of causation is completely reversed, "When total energy is less, the body wastes less, thus using the intake with greater efficiency. As intake increases, wastage also increases and the energy is used with decreased efficiency" (Sukhatme, 1982a, p. 38). Indeed, adaptation is by definition a phenomenon where variation in efficiency is induced by prior variation in intake, whereas in autoregulation efficiency varies in a spontaneous manner, and intake merely follows suit.

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27. "It is apparently the autoregressive mechanism in daily expenditure in maintaining bodyweight which enables a man to adapt his requirement to intakes without affecting the net energy needed for maintenance and physical activity" (Sukhatme, 1982a, p. 39).

Recall that in the autoregulation model efficiency is supposed to vary 'as a matter of course' within a stochastic stationary distribution. This leads to corresponding variation in requirement, so that in the case of an unconstrained person "... intake is regulated in autoregressive manner to meet his needs" (Sukhatme 1981, p. 1318; emphasis added). The causation thus runs from variation of efficiency to change of requirement and then finally to intake ('to meet his needs'). It all starts with prior spontaneous variation in efficiency as a stochastic process.

The idea of spontaneous variation comes out most clearly when one considers the analogy drawn by Sukhatme and Margen (1978, 1982) between their models for energy and protein. They found autocorrelation in both models and claimed that these are similar biological phenomena. It is however significant to note that intake was kept fixed in the protein model, and yet expenditure varied in an autocorrelated manner due presumably to similar variation in the efficiency of protein absorption. There was thus no scope for efficiency to vary in response to intake. If the analogy is still to be retained, one must then admit that efficiency of energy utilisation varies in a spontaneous manner; intake merely follows suit when it is free to vary.[28]

Indeed, unless one assumes that spontaneous variation in efficiency is the driving force, it becomes impossible to explain why intakes must be autocorrelated in the free-living healthy individuals. If efficiency merely responded to intake, a healthy person could vary his intake

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28. See Mehta (1982, p. 1335).

in any way he liked (within limits of course) and still maintain weight homeostatis. Therefore, instead of observing that "... low intake is followed by a low intake and high intake followed by a high intake" (Sukhatme 1981, p. 1319), one could as well observe low intake being followed by high intake, and vice versa. It is only because efficiency is supposed to vary spontaneously within a stochastic stationary distribution and intake is supposed to follow suit, that intake too must vary in a similar manner and exhibit autocorrelation in its time series.

It is thus clear that autoregulation and adaptation envisage two entirely opposite lines of causation between intake and efficiency. Sukhatme however tends to ignore this fact. He keeps on changing direction in the mid-course and moves back and forth along the opposite lines of causation, while giving the impression that he was talking about the same thing.[29] But this impression is completely misleading because 'induced' and 'spontaneous' variation in efficiency cannot be the same biological phenomenon.

Autoregulation, therefore, cannot imply adaptation. However, it is also true that adaptation may exist regardless of autoregulation. In other words, autoregulation is neither necessary nor sufficient for the existence of

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29. Here is an interesting example. In explaining the autoregressive model, he writes, "(1) On current evidence this means that intake in man will vary as a matter of course ... in a manner that is self-regulated ... (2) It implies that when intake is low the body uses energy more efficiently ... (3) In other words the body adapts requirement to intake by varying the efficiency of utilisation" (Sukhatme 1981, p. 1319; numbering added). Note the mode of transition. The first part is clearly about autoregulation. In the second part causation gets blurred and gives way to 'association'. Then hinging on this association, causation is reversed in the final part.

adaptation. But if adaptation does exist, then of course it is the limit of adaptation rather than the average value that should be used as the cut-off point for the assessment of undernutrition. However, if it is the idea of adaptation that has to be invoked in the final analysis to justify a lower cut-off point, the theory of autoregulation becomes an unnecessary detour. One might as well jettison this theory altogether and go back to the notion of 'dietary-induced thermogenesis' which had envisaged adaptive variation in efficiency long before Sukhatme came into picture.[30] He had in fact seized upon this idea and 'inverted' the causal process to postulate his model of autoregulation. On realising that autoregulation by itself leaves a gaping hole in the logic of $\mu-2\sigma$, he then 'reinverted' the process and went back to the origin. We might as well do the same.

There is of course nothing wrong in doing this, except that it must be realised that the limit of adaptation cannot be deduced from the value of $\mu - 2\sigma$ derived from an autoregressive model. It has to be based on an independent assessment of the evidence on adaptation.

Is There Adaptation in Energy Efficiency?

The idea that efficiency of utilisation can adapt to

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30. Indeed the idea of autoregulation may have to be abandoned in any case in the light of recent scientific evidence. For example, the findings of Rand et al. (1985) cast serious doubt on the hypothesis of autoregressive variation in both protein and energy requirement. Also, Garby and his colleagues have found that the energy expenditure of subjects at fixed levels of intake and activity remains remarkably constant over time, which contradicts the hypothesis that efficiency varies spontaneously within a stochastic distribution (Garby et al., 1984; Garby and Lammert, 1984).

different levels of intake is not a novel one, nor is it claimed to exist for energy intake alone. Several nutrients such as protein and iron are known to be subject to this phenomenon. In fact, the established practice of calculating protein requirement is based on this notion. It is accepted that there exists a 'safe range' of protein intake within which the effective absorption of protein is maintained at a constant level by varying the rate of utilisation. Accordingly, the lower limit of the range is defined as the minimum protein requirement; and it is this 'minimum' rather than 'average' requirement that is used to judge whether a person consumes adequate protein or not [31] (Scrimshaw and Young 1978, Munro 1985; FAO/WHO/UNU, 1985).

The idea of 'adaptation' to a range of energy intake is an exactly analogous one, and so is the plea to use the 'minimum' rather than the 'average' requirement as the cut-off point. The problem however is that the biological evidence on adaptation is not as clear-cut in the case of energy as in the case of nutrients such as protein or iron. As a result, the paradigm of 'fixed' energy requirement still holds sway over much of nutrition profession, although the weight of discordant notes cannot be ignored.

There is a vast literature on the subject and we cannot

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31. It is however recognised that the 'minimum' requirement may vary from person to person. Accordingly, "recommended" protein allowance for population groups is set at two standard deviations above the average of 'minimum' requirements, with a view to providing adequate protein for as many people as possible even if overproviding for some of them in the process.

attempt to review it here.[32] However, several salient features of the accumulating evidence may be pointed out.

The adaptationists draw evidence from both cross-sectional and longitudinal studies. There is by now a considerable body of cross-sectional evidence to show that people at different levels of habitual intakes can maintain similar bodyweight while remaining engaged in apparently similar activities. In fact, as high as two-fold range in the variation of energy intake is quite common (Rose and Williams, 1962; Widdowson 1962, Edmundson 1977, 1979). This could be interpreted as an <u>a priori</u> evidence that people can adapt to low food intake by using energy more efficiently.

But there are at least three problems with this interpretation: (a) 'apparently similar' activities may conceal important differences in energy expenditure, specially in the so-called 'discretionary' (leisure-time) activities; (b) the reported 'low intakes' may not really be low: for example, when some of the 'reportedly' low-intake subjects were experimentally fed their 'reported' diet, most of them were found to be losing weight (Ashworth, 1968; Durnin, 1979); (c) most importantly, the observed inter-individual differences may simply reflect <u>genotypic</u> differences in the efficiency of energy utilisation between individuals rather than <u>phenotypic</u> adaptation within the same individual.

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32. Several useful reviews have come out in the recent years, some more dismissive of the adaptationist view than others. The comprehensive reviews of James and Shetty (1982) and Norgan (1983) belong to the dismissive category, while that of Apfelbaum (1978) is much more sympathetic. Somewhat less comprehensive reviews can be found in Grande (1964), Durnin (1979), Edmundson (1980), Garrow (1978) and Miller (1975). See also Dasgupta and Ray (1986) for an outsiders' view.

The last argument is sometimes countered by invoking Sukhtame´s finding that 'apparent' inter-individual variation can be ascribed almost entirely to intra-individual variation. But this is just a misapplication of an argument in the wrong context. Sukhatme´s argument referred to short-run variations in intake and the implication of autocorrelation in such variations around the mean level. It does not obviously apply to the case of long-term 'habitual' intakes with which these cross-sectional studies are concerned. Edmundson, whose own studies are relatively free from the first two problems mentioned above, is keenly aware of the third, and readily admits that one cannot be sure how much of intake variation is due to genotypic differences and how much, if any, to genuine phenotypic adaptation (Edmundson, 1980).

In fact, this difficulty of disentangling the phenotypic from the genotypic variation is inherent in all cross-sectional studies (Prentice, 1984). What one really needs is longitudinal evidence on the same person experiencing deviation from habitual intakes. A striking evidence of this kind was provided long ago by Neumann (1902) who found over a period of two years that he could increase his intake by as much as 800 calories per day without significant change of bodyweight. He described this phenomenon as 'luxusconsumption', and provided probably the first scientific evidence that human efficiency of energy utilisation can adapt to a wide range of intake.

But subsequent laboratory experiments, under more controlled conditions and covering a larger sample of subjects, have failed to provide unambiguously corroborative evidence. In this respect, one can draw upon both underfeeding and overfeeding experiments, the principal findings of which may be summarised as follows:[33] (a) under severe caloric restriction, BMR per kg of bodymass decreases, thus showing an increase in the efficiency of energy utilisation; but it is necessarily accompanied by a reduction in bodymass as well; (b) the findings of overfeeding experiments are more controversial; not all experiments show that weight gain can be contained by adaptive decrease in efficiency (i.e., through 'dietary-induced thermogenesis'); even when thermogenesis occurs, it seems to be triggered only after some weight gain has taken place.

There is thus some evidence to show that phenotypic adaptation of a kind may take place in the efficiency of energy utilisation, but it seems almost always to be accompanied by alteration of bodyweight. It is therefore not the kind of adaptation that is relevant for determining the intra-individual range of requirement which is based on a given level of bodyweight. What is relevant is 'pure' adaptation in efficiency, without any change of bodyweight. But the existing evidence does not indicate that such adaptation is possible.

It should however be borne in mind that in all these experiments, calorie intake was either severely reduced or grossly blown up. They do not therefore rule out the possibility that at moderate levels of deviation, 'pure'

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33. Some of the best-known underfeeding experiments are reported in Benedict _et al._ (1919), Keys _et al._ (1950), Grande _et al._ (1958) and Apfelbaum _et al._ (1971). Among the overfeeding experiments, one may mention _inter alia_ Miller _et al._ (1967), Apfelbaum _et al._ (1971), Sims (1976) and Norgan and Durnin (1980).

adaptation in efficiency may still occur.[34] Future experiments may throw light on this possibility. But at the present moment there is no scientific basis for arguing that a significant scope for 'pure adaptation' exists, far less to make a quantitative assessment of the limits of adaptation.

We are thus left with the judgement that the hypothesis of intra-individual adaptation in requirement is not yet substantiated by scientific evidence, although the possibility cannot be ruled out; on the other hand, there is ample evidence of inter-individual variation in requirement. What all this implies for some of the concerns in economics is discussed in Section V.

III Physical Growth and Anthropometric Status: The 'Small but Healthy' Hypothesis

'Genetic Potential' vs. Homeostatic Theory of Growth

We have so far been concerned with the energy requirement of a 'reference' adult. But much of the concern with malnutrition in the developing world actually relates to the plight of children who are the most vulnerable and the most visible victims of nutritional stress. In this section, we look at some of the problems related specifically to the case of children.

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34. Rand et al. (1985) have produced evidence which shows that even at moderate levels of deviation, adaptive thermogenesis does not seem to operate without accompanying change in bodyweight. But as the authors point out, there are problems of interpretation. One difficulty is that the levels of physical activity were not monitored, which makes it difficult to ascertain if the failure to maintain weight was due to failure of thermogenesis or to alteration in activity.

The energy requirement of children differs from that of adults in one important respect: they need energy to maintain a satisfactory rate of physical growth, in addition to the usual needs for BMR and physical activity. What constitutes a satisfactory rate of growth is therefore a matter of crucial concern in the estimation of requirement and hence in the dietary measurement of undernutrition and poverty. It is also relevant to the assessment of nutritional capabilities with the help of anthropometric measurement. Anthropometry provides a technique for assessing nutritional status by comparing actual achievements of physical growth with some desirable standards. Once again, it becomes necessary to decide what constitutes a desirable standard of growth.

But the specification of a desirable standard is by no means a simple matter. In fact it has turned out to be one of the most controversial issues in contemporary nutrition literature. As in the case of the requirement of a reference adult, here too there is a deep division between a widely held 'establishment' view and a small but influential group expounding a 'heretic' view. The former may be described as the 'genetic potential' theory, and the latter as the 'homeostatic' theory of growth.

The difference may be best clarified by referring to the actual practice of how 'desirable standards' are determined in the traditional approach. The dietary needs of children are usually equated with the actual intakes of well-nourished children growing normally in the developed world. The 'growth curves' along which such children are growing are also used to determine the rates of desirable growth as well as optimal anthropometric standards, for application in the rest of the world. The underlying principle is the following: since the children in the reference group are unhindered by nutritional deprivation and enjoying the maximal growth permitted by their genetic potential, they constitute an ideal standard against which to judge the nutritional adequacy of all other groups. Two assumptions are implicitly made in this procedure, viz. (i) there is no difference in the genetic potential of different races in the world, and (ii) anything less than the achievement of full genetic potential must be deemed to constitute 'inadequate' nutrition.

The first assumption is based on fairly strong empirical grounds, as can be seen from the recent reviews by Martorell (1984) and Roberts (1985). Numerous studies have shown, for example, that the children in poor developing countries can in general grow at the western rates if adequate intake and environmental hygiene can be ensured. The assumption of the same genetic potential is therefore a fairly safe one.[35]

But the second assumption is deeply controversial. It is the essence of 'genetic potential' theory and the prime target of attack by 'homeostatic' theory. The problem centres on why should nutrition be deemed inadequate when genetic potential is not achieved. It is of course quite clear that the concept of genetic potential has an obvious appeal as the normative target of growth which every community may aspire to achieve. In this normative sense, the failure to achieve genetic potential is indeed a case of 'inadequate' nutrition. But the scientific concept of 'undernutrition' does not refer to the failure to meet some normative target, but to the failure to maintain the functional capabilities that depend on the level of nutrition. Therefore, in the context of assessment of

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35. Some exceptions are noted, for example, in respect of a few tribal communities and the people of the Far East (Roberts, 1985).

undernutrition, the second assumption must imply that any deviation from the genetic potential necessarily entails some functional impairment. But this is precisely where the problem lies. Historically, the studies of functions were confined to severe cases of nutritional stress; and there is no doubt that functional impairment does occur in such cases. However, the evidence on what happens to the moderate cases of chronic deprivation is only recently beginning to emerge. Yet, the genetic potential theory has always remained the dominant paradigm in nutrition literature.

The homeostatic theory challenges this paradigm and argues that, within limits, deviation from genetic potential does not entail any functional impairment i.e., people can be 'small but healthy' to use a popular phrase coined by Seckler. The origin of this hypothesis can be traced to a group of eminent biologists who have been much concerned with the processes of human growth. For instance, J.M. Tanner, who is one of the leading authorities on human growth and whose influence Seckler acknowledges, explicitly warns against assuming that being small is necessarily bad. In fact he coins the phrase 'bigger not better' and argues that , "Though rate of growth remains one of the most useful of all indices of public health and economic well-being in developing and heterogenously developed countries, it must not be thought that bigger, or faster, is necessarily better. From an ecological point of view smallness has advantages" (Tanner, 1978, p. 4). The advantage consists in the fact that a small body enables a person to survive and to sustain his level of activity in a world of nutritional constraint, because a smaller body requires less energy both for maintaining itself and for performing external work. Seckler extends this point further and argues that while maintaining itself and the level of activity, the small body, unless it is too small (in a sense to be defined below), can also avoid all kinds of functional disabilities

(Seckler, 1980, 1982, 1984, 1984a).[36] Accordingly, he suggests that the appropriate reference standard for the assessment of undernutrition is the lowest growth path within the homeostatic range rather than the maximal growth path permitted by genetic potential.

Before assessing the merit of the hypothesis, it should be noted that Seckler's initial formulation of the hypothesis suffered from a lack of rigour which has given rise to a good deal of confusion. He defined as 'small but healthy' (SBH, for short) all those who are suffering from mild to moderate malnutrition (MMM) according to conventional standards (Seckler, 1980, 1982). But this is imprecise, because the set of MMM may be different for different types of anthropometric measurements, of which there are quite a few. The most widely used measures are weight-for-age (Gomez classification), height-for-age and weight-for-height (Waterlow classification).[37] Weight-for-height is a measure of 'wasting' and height-for-age is a measure of 'stunting' whereas weight-for-age may reflect both wasting and stunting. It is clear from Seckler's writings that the domain of SBH was meant to be confined to the case of pure 'stunting' unaccompanied by 'wasting'. Thus, for instance, he claimed that "... about 90% of all

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36. More precisely, the hypothesis can be stated as follows: If two persons of different bodysizes (one having achieved his genetic potential and the other falling short by a margin to be specified below) are both living in the same environment and receiving an intake that is commensurate with their respective bodysize and (desirable) activity, then the smaller person will enjoy the same level of functional capability as the bigger person, despite having failed to achieve his genetic potential; smallness <u>per se</u> (within a range) does not matter.

37. An excellent account of alternative measures and their relative usefulness can be found in Waterlow (1972).

the malnutrition found in these countries involved people with low height for age but with the proper weight for height ratio" (Seckler, 1980, p. 223, emphasis original), and wondered "... if there is anything wrong with these small people other than their smallness" (p. 223). Yet he often talked of mild-to-moderate malnutrition as if it were an undifferentiated category. The imprecision has since been rectified, and Seckler (1984b) has recently defined SBH explicitly as referring to the mild-to-moderate degrees of pure stunting (i.e., between 80 and 100 per cent of the 'Harvard median standard' for height-for-age with normal weight-for-height ratio). However, the confusion in the meantime has taken its toll, and much of the critique of SBH has simply gone astray by bringing in evidence that was not quite relevant.

The distinction between 'wasting' and 'stunting' is in fact quite a crucial one. Both are indicative of retarded growth in a general sense, but of two quite different kinds. 'Wasting' represents depletion of body tissue, whereas 'stunting' indicates slower rate of new tissue deposition. They thus represent two distinct biochemical processes whose functional consequences need not be the same. It is generally recognised that 'wasting' is much more harmful than 'stunting'. But Seckler goes a step further and suggests that moderate stunting is not harmful at all.

This proposition is based on the following kind of reasoning. If the level of nutrition is not consistent with normal bodyweight at the genetically permissible maximal height and if yet such height is to be attained, then equilibrium will have to be achieved by depleting body tissue (i.e., by 'wasting') in order to supply additional energy. This will admittedly have adverse functional consequences. But 'stunting' offers an adaptive mechanism to avoid these consequences by reducing height and thereby reducing nutrient demand in keeping with supply. At this low-level equilibrium, existing tissues need not be depleted to supply additional energy, and normal bodyweight for height can be maintained. Therefore, it is argued, if stunting is within a moderate range, "There are no impairments because this range represents an adaptive response of bodysize to adverse conditions in order to avoid these impairments" (Seckler, 1980, p. 224, emphasis original).

But this argument is not entirely convincing. Just because adaptation is designed to avoid some adverse consequences, it does not follow that it has no adverse effects of its own. It is at least conceivable that more severe consequences are averted at the cost of less severe ones. There is thus no *a priori* reason to believe that an adapted state, such as 'stunting', is necessarily costless.

Thus on the purely conceptual ground, the plausibility of homeostatic theory remains wide open. It is now necessary to evaluate the existing empirical evidence on the relationship between stunting and functional impairment.

We shall presently review some of the evidence in this regard. But prior to that, we wish to point out some of the unfair criticisms that have been made from both sides of the debate based on misunderstanding of each other's position.

Firstly, the precise content of Seckler's hypothesis has not always been correctly appreciated. He does not propose that good health consists in being small, but that being small (upto a point) may be just as healthy as being big. The operative phrase is therefore 'small *but* healthy' and not 'small *is* healthy' as has sometimes been misconstrued. This confusion seems to permeate much of the critique by Gopalan (1983a), who even goes as far as using evocative phrases like 'small is beautiful' in presenting a caricature of Seckler's views, and takes him to task for "pleading the virtues of smallness". The confusion seems to arise from a failure to distinguish between two quite different issues: one relates to the relative desirability of different bodysizes in a given situation of nutritional constraint, and the other relates to looking at bodysize as an indicator of health. It is in the first context that Seckler pleads the virtues of smallness, but then nobody questions that smallness has advantages in a situation of constraint. On the other hand, when it comes to indicating health, Seckler shows no particular preference for smallness, because in his view the same level of health is maintained within a range of bodysize; that is after all precisely the idea of homeostatis.

On the other side of the fence, the 'genetic potential' theory too has faced a couple of unfair criticisms. One of them has been raised by Payne and his colleagues and the other by Seckler himself.

Payne and his colleagues interpret the genetic potential theory in quite a novel way. In the usual interpretation, all functions are assumed to be maximised when the full genetic potential is achieved. In their novel interpretation, however, no diet maximises all aspects of health at the same time; and one particular aspect that is not maximised by diets permitting realisation of genetic potential is longevity.[38] The argument is that such diets usually give rise to obesity, and the cardio-vascular diseases associated with obesity tend to reduce longevity.

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38. See Payne (1985, p. 87), Payne and Cutler (1986, p. 1490) and Pacey and Payne (1984, p. 71). See also Martorell et al. (1978, p. 142) for a similar view.

The state of 'full genetic potential' is thus seen to represent a particular trade-off between longevity and other functions, so that a belief in the virtue of 'genetic potential' would imply an acceptance of this particular trade-off. In other words, the genetic potential theorist is supposed to view the human body as a self-optimising mechanism which, if faced with no constraint, will choose an optimum configuration using "... a set of built-in relative value weightings or trade-offs between different kinds of function ..." (Payne and Cutler, 1984, p. 1486). He is thus supposed to have an axiomatic faith in the self-optimising capacity of the human body, in much the same way as a neoclassical economist assumes optimising behaviour on the part of an individual consumer.[39] But this faith is argued to be misplaced, because while "It is reasonable to believe that evolution through natural selection has given rise to some built in system of priorities, ... it is quite another thing to believe that these priorities are those we might choose" (Payne and Cutler, 1984, p. 1490). In other words, what the body chooses for itself is not necessarily what we as scientists should accept as the best for our body.

As a matter of historical fact, however, the 'genetic potential' theorists have not in general viewed the 'state of full potential' as one involving trade-off between conflicting functions, simply because they do not admit of any such conflict. Obesity, and its effect on longevity (or any other function such as work capacity) is not seen as a necessary concomitant of achieving genetic potential. It is true that ad libitum feeding, while permitting the

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39. Payne and his colleagues make a lot of play with this analogy, quite wrongly in our view, when it comes to drawing policy implications from alternative theories. See our discussion in Section V.

expression of full genetic potential, will also tend to increase obesity. But the argument for allowing full genetic potential is not an argument for ad libitum feeding. Nobody suggests that we should achieve the full genetic potential of accumulating fat. The plea is for achieving the potential for height with a body composition that does not veer towards obesity. It is believed that such balance is possible to achieve, and it is this balanced state that is implied by the achievement of full genetic potential. In reality, of course, what we often observe in the western societies is an unbalanced state where obesity prevails due to overeating. This calls for a careful selection of samples when deriving nutritional standards from the western world. If the prevailing 'standards' have failed to take such care, as it is sometimes alleged, it is a problem with the practice, not with the principle.

Seckler (1984) launches his own critique from a more philosophical plane. He invokes the Popperian criterion of falsifiability to judge whether a theory is scientific or not; and argues that while his own homeostatic theory yields falsifiable predictions and is hence scientific, the genetic potential theory is in principle non-falsifiable and hence non-scientific. In making this claim, he interprets the 'genetic potential theory' as one which defines 'health' without any regard to functional implication, as if definitionally equating good health with the achievement of genetic potential. Thus a person below the potential would by definition be called 'unhealthy' "... even if he or she has no observable signs or symptoms of 'unhealth'" (Seckler, 1984, p. 1886). Such a definitional theory would indeed be non-scientific, as it would be even in principle non-falsifiable.

However, this does not seem to be a correct interpretation of the establishment view. Leading modern advocates of the genetic potential theory such as Beaton (1983), Gopalan (1978), Thomson (1980) and others have always emphasised the functional concept of health, and the latest testament of the establishment view embodied in the form of FAO/WHO/UNU (1985) enshrines this concept in unequivocal terms. Their hypothesis is that all functions are simultaneously maximised when and only when full genetic potential is achieved. It is certainly falsifiable in principle, the falsifiable prediction being that any deviation from potential would impair at least some function. It is therefore as much scientific by the Popperian criterion as Seckler's own theory is.

The trouble however is that the hypothesis has been accepted and put into practice without sufficient demonstration of its validity, which has encouraged the emergence of the 'small but healthy' hypothesis in the first place. But the latter hypothesis too has so far been based on inadequate empirical evidence, drawing mainly upon studies on only one kind of function viz. immunocompetence among the children. But there are also other functions that need to be studied. Moreover, the studies of children alone will not suffice. Since stunted children very often grow into small adults, it must be asked what adverse effects the small adult size might have in later life. We consider below four functions which are usually singled out for this purpose.[40] Two of them viz. immunocompetence and cognitive development relate principally to the children. The other two viz. physical work capacity and reproductive efficiency become more relevant in adult life.

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40. We should point out that this is not intended to be a comprehensive review, which is clearly beyond our competence. However, we do draw upon a number of authoritative reviews done by experts in their respective areas, though not always agreeing with them fully.

Evidence on the Relationship between Moderate Stunting and Nutritional Functions

(i) Immunocompetence:

The effect on immunocompetence can be studied at least at two levels: (a) epidemiological studies on mortality and morbidity resulting from reduced immunocompetence, and (b) clinical studies of impairments in the immunological system of the human body.

Studies of both kind abound in the literature. But they suffer from at least two limitations for our present purpose. Firstly, most studies relate to severe cases of malnutrition, while our interest here is with the mild-to-moderate category. Secondly, the most frequently used nutritional indicator is weight-for-age which does not serve our purpose, unable as it is to distinguish between wasting and stunting; moreover even when height-for-age is used it is not always clear that weight-for-height is normal for the sample, but that is the kind of sample we need.[41] However, some tentative conclusions can still be reached.

In a celebrated study among Bangladeshi children, Chen et al. (1980, 1981) have found that the rates of morbidity and mortality among children with mild-to-moderate deficit in terms of height-for-age were no different from those of normal children. A more recent study by Heywood (1983) has also shown that when weight-for-height was normal, stunting by itself made no difference to the incidence of mortality among the children of Papua New Guinea.

The general nature of these findings is confirmed by

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41. These two limitations apply equally to the evaluation of other functions as well.

Martorell and Ho (1984) who have recently reviewed the literature on the relationship between anthoropometric indicators and the risk of infection. They have in fact found no evidence that poor nutritional status, as measured by anthropometric indicators, is associated with greater incidence of infection ; as Martorell (1985, p. 22) concludes, "nutrition seems to have little to do with who gets sick". On the other hand, there does appear to exist an association between nutritional status and severity of infection; but it is the weight-for-height index (wasting) rather than height-for-age (stunting) that seems generally to predict well the severity of infection.

It may be argued that in some sense a direct study of the immunological system would be a better way of identifying functional impairment. In fact, clinical studies have shown that all the major components of the immunological system, viz. the cell-mediated immune system, immunoglobulines (antibodies), phagocytes and complements, may be damaged in the event of severe undernutrition, the first component being the most visibly damaged (Chandra, 1980, 1982; Suskind, 1977). But no such conclusion can be drawn about the moderate levels of nutritional stress. For example, Reddy et al. (1976) found that the cell-mediated immune system as well as phagocytic function were damaged only when weight-for-age fell below 70 % of the standard. It is true that the index used here was weight-for-age rather than height-for-age which is more relevant for our purpose. But note that the set above 70 % of the standard weight-for-age fully subsumes the SBH-relevant set (i.e., those with height-for-age above 80 % of standard and with normal weight-for-height). Thus the findings of Reddy et al. would seem to indicate that no part of the immune system is visibly impaired in the case of pure moderate stunting. A more direct evidence is provided by Bhaskaram et al. (1980) who actually used the height-for-age indicator. They studied the immunocompetence of adolescents who had experienced growth retardation in their early childhood. No impairment was found in any component when stunting was within moderate limits, and just one component (cell-mediated immunity) was found damaged in the case of severe stunting.

Thus both epidemiological and clinical studies would seem to suggest that pure moderate stunting does not entail any visible loss of immunocompetence.

(ii) Reproductive efficiency

It has been suggested that shorter women produce more vulnerable babies. The suggestion is based on the following kind of reasoning. First, it is well-known that shorter women generally bear smaller babies (DDH/INCAP, 1975). Secondly, it has been repeatedly shown that low-birthweight babies have a higher risk of mortality (Chase 1969, Martorell, 1979). By combining these two facts, Martorell et. al. (1978) argue that maternal height can be said to be related "logically to infant mortality as well" (p. 150).[42]

However, it may not be entirely logical to combine the two facts mentioned above. There are two reasons for this.

First, it should be noted that the critical concept here is that of a low-birthweight (LBW) baby which is defined as a normal-term newborn weighing less than 2.5 kg. It is among such babies that higher mortality has been observed as compared with babies weighing more than 2.5 kg. In the context of SBH hypothesis, it should therefore be shown that

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42. See also Thomson (1980) for a similar mode of reasoning.

moderately stunted women tend to bear a higher proportion of LBW babies; a simple association between maternal height and birthweight will not do. While the oft-quoted DDH/INCAP (1975) study has indeed shown that shorter women bear a larger percentage of LBW babies than taller women, the difference becomes striking only when the extreme ranges of height are compared. Between the tallest two groups, spanning a range of 147-165 cm the difference is negligible. Thus the effect of moderate stunting is not yet established.

Secondly, it is not at all clear at the theoretical level why stunting in a mother's own childhood should retard the growth of her foetus in adult life. In a comprehensive review of the etiology of low-birthweight infants, Battaglia and Simmons (1979) do not mention a single factor which can be unequivocally attributed to stunting <u>per se</u> on the basis of existing biological knowledge.[43]

Yet researchers are often persuaded by the existence of empirical association alone. A case in point is the pioneering study by Thomson (1966) which gave currency to the view that short mothers produce more vulnerable babies. Based on a study of women belonging to the more developed part of the world (Scotland), he concluded that low maternal stature is associated with higher risk of infant mortality. His own explanation was in terms of physical and physiological malfunctions presumably caused by maternal short stature. But as Calloway (1982, p. 739) has pointed out, Thomson reflects a 'bias' here in <u>assuming</u> that "... height and birthweight are related through inherent

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43. It has been pointed out by Thomson (1980) that if stunting is caused by 'ricket', a woman may suffer from a kind of pelvic distortion which may seriously damage the foetus. But here we are concerned with the more general case of stunting caused by calorie deficiency.

physical/physiological processes", for he does not spell what these processes could conceivably be or what the theoretical basis of these processes are. A more logical explanation can be found in some other information supplied by Thomson himself. The shorter women came generally from a lower socio-economic status and had on the average a lower health status which may have been due to lower socio-economic status itself. Ill-health, possibly combined with lower level of nutrition, may have meant early damage to fetal life and hence the subsequent risk of mortality of their babies compared to those of taller women who mostly came from a higher socio-economic group. Stunting as such may not have had anything to do with this.

The same argument applies a fortiori to the stunted women in the developing world. The deprivation experienced by them in their childhood have made them stunted. When they grow into adults, the same deprivation at the time of pregnancy may retard the growth of their foetus. But here it is the lack of nutrition during pregnancy, rather than stunting per se, that is responsible for retardation in fetal growth. Much of the association between short maternal stature and low-birthweight babies in the poor societies may perhaps be explained in this manner.

In any case, when small mothers are seen to produce small babies, for whatever reason, that in itself need not be a cause for worry. It may even be an advantage in a situation of nutritional constraint. Smaller babies have a lower maintenance requirement than the larger ones and hence carry a better chance of survival in an adverse condition (unless of course they happen to be low-birthweight babies in the technical sense). This was noted by Thomson (1966, 9. 211) himself who found that when only the low socio-economic group was considered, it was the taller women whose infants were subject to a higher incidence of mortality. A subsequent study by Frisancho _et al._ (1973) confirmed this observation among a poor population group in Peru; the shorter women were found to have a higher 'offspring survival ratio'. Against this however there is a counter-example recently provided by Martorell _et al._ (1981), who found among a group of poor Mayan women that the taller women had a lower rate of infant mortality and higher rate of surviving offspring. Also, unlike in Thomson's study, the poor performance of shorter women cannot be explained by worse socio-economic condition, as the samples came from a particularly homogeneous group. However, as the authors point out, the group as a whole was very short, 'among the shortest in the world'. The comparison was thus between short and very short mothers in an exceptionally short population. The question of moderate stunting therefore still remains wide open.

(iii) Work Capacity and Productivity

Does small stature in men lead to reduced work capacity and productivity? Gopalan (1983a), Spurr (1983, 1984) and several others have argued that it does. To understand the theory behind this claim it is necessary to be familiar with an important physiological concept called Physical Work Capacity (PWC).

Physical activity is performed through the mediation of skeletal muscles which generate energy with the help of oxygen and produce work. The maximal volume of oxygen uptake (VO_2 max) is therefore an indicator of the maximum amount of energy that can be liberated by skeletal muscles and hence the maximal amount of work that can be done. This maximal work potential is called Physical Work Capacity, also known as the maximal aerobic capacity.

The amount of VO_2 max depends in the first place on the total amount of muscle cell mass (MCM), sometimes proxied by lean body mass (LBM) or simply bodyweight. Partly it also depends on the health of the circulatory system as measured by the level of blood haemoglobin and cardiac efficiency. It is also known to have a positive association with sustained training (as in the case of athletes) as well as the level of habitual physical activity. But bodysize is the most important determinant, accounting for over 80 % of the difference in VO_2 max between subjects suffering from varying degrees of protein-calorie deficiency (Spurr, 1983, p. 7).

Viteri (1971) has noted that cell function does not seem to be impaired in mild to moderate cases of nutritional deficiency i.e., aerobic capacity per unit of cell mass remains intact. It is the difference in the amount of cell mass that makes the difference in absolute VO_2 max. One can thus argue that since the stunted adults have less bodymass than adults of normal height, they would also have a proportionately lower VO_2 max and hence a lower level of Physical Work Capacity.

But does a lower level of Physical Work Capacity matter? After all, it is well-known that in actual work conditions, people do not exert at more than 35-40% of VO_2 max even when engaged in heavy work for a 8-hour day (Åstrand and Rodahl, 1977). So what is wrong if stunting leads to a lowering of a ceiling that is never reached in real life anyway? Spurr (1983, 1984) has argued that stunting will nevertheless lead to lower productivity, specially in occupations involving heavy physical work. He postulates a theoretical model to explain the logic and also cites evidence in support of his contention. We shall argue that neither the theory nor the evidence is entirely convincing.

The logic of Spurr's theory is deceptively simple. In heavy physical work everyone may be assumed to be expending energy at the maximum sustainable rate (say, 40% of VO_2 max). Since a shorter man has lower VO_2 max than a taller man, he will be spending a lower absolute amount of energy, while working at this rate. Assuming that energy cost per unit of task is the same for everyone, it must then follow that the shorter man will produce less.[44]

The trouble with this argument lies in the assumption of the same (per unit) energy cost for everyone. Note that the energy cost per unit of task (as measured by oxygen consumed while doing the work) has two components: the energy cost of basal metabolism that goes on during the period of work and the energy required for the work itself. The first component, as we have noted (section II), depends directly on bodymass. The same will be true also for the second component specially in typical heavy work involving body movement, because the energy involved in a work is directly proportional to the mass that is moved. Thus the gross energy cost per unit of (heavy) task can be assumed to be roughly proportional to bodymass. It means that while lower VO_2 max (due to lower bodymass) will constrain a shorter man to spend lower absolute amount of total energy, his energy

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44. The unit of 'task' is given by a set of physical dimensions (e.g. moving an object of a given mass to a certain distance along a given direction). Note that we speak here of 'task' rather than 'work'. This is intended to avoid terminological confusion: in thermodynamics, the concept of 'work' is measured in terms of energy so that it does not make sense to speak of energy cost per unit of 'work'.

cost per unit of task will also be proportionately lower.[45] As a result, his productivity need not suffer.[46]

The preceding argument relates to productivity per unit of time. Total productivity will obviously depend also on stamina i.e., for what length of time a person can sustain a heavy work. But then there is no evidence that smaller men have less stamina. When Spurr and his colleagues conducted endurance tests on subjects with different bodyweights (but none severely malnourished) they found no difference in the time for which a subject could sustain a workload at 80% of his VO_2 max (Barac-Nieto et al., 1978).

There is thus no a priori reason to assume that the stunted men would be less productive, even in doing heavy physical work. Yet empirical evidence is often cited to prove the disadvantage of being small. Two kinds of evidence are marshalled in this respect - one indirect and the other direct.

The indirect evidence is drawn from a number of studies which have shown that productivity is postitively related to VO_2 max. Coupled with the observation that smaller men must have lower VO_2 max, this evidence is then taken to imply

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45. In technical jargon, a shorter man will have a higher 'gross mechanical efficiency'. Note that this conclusion does not depend on the assumption of higher metabolic or ergonomic efficiency on the part of the smaller men. Spurr based his assumption of the same energy cost on the premise that smaller men are not known to be more efficient users of energy. In respect of metabolic or ergonomic efficiency, he may well be right. But surely the 'gross mechanical efficiency' must still be lower for the smaller men simply by virtue of lower bodymass, and that alone would give them a lower energy cost per unit of task.

46. See Ferro-Luzzi (1985, p. 65) for a similar argument.

that smaller men would be less productive (Spurr, 1984). But this inference is not necessarily valid. Note that the difference in VO_2 max can arise due to difference in residual factors such as haemoglobin, habitual activity etc. When these residual factors, rather than lower bodymass, bring down VO_2 max there is no reason to expect a corresponding reduction in energy expenditure per unit of work. Therefore if lower VO_2 max constrains total energy expenditure, as in heavy physical work, there is every reason to suspect that productivity will suffer. One can thus accept the finding that lower VO_2 max leads to lower productivity without accepting that smaller men produce less. For the latter to follow, it must be shown that the difference in VO_2 max occurred in the first place due to difference in bodysize.[47]

This means that nothing short of direct evidence will do. In this respect, two studies are often quoted, namely Spurr et al. (1977) and Satyanarayana et al. (1977). Both demonstrate that productivity is positively related to height, but care is needed in interpreting the results.

Spurr et al. (1977) have shown through a multiple regression analysis that VO_2 max, height and body fat are all significantly related to productivity. In other words, even when the effect of VO_2 max is eliminated, height still remains positively associated with productivity. Now, that seems somewhat surprising. According to theory, height is supposed to affect productivity by limiting VO_2 max. So, if VO_2 max is controlled, what physiological mechanism remains through which height is supposed to affect productivity? One

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47. As it happens, in several of the studies cited by Spurr, the "high productivity" group did not differ from the rest in respect of height and weight. See, Hansson (1965) and Davies (1973).

cannot help suspect that height happens to be a proxy here for some unknown non-physiological variable. Therefore, the physiological theory that stunting leads to lower productivity remains open to question.

The study by Satyanarayana et al. (1977) is free from this particular problem. When weight was controlled, height did not seem to have any independent effect, as should be the case if height is to limit productivity through bodymass and VO_2 max. This study is therefore more relevant to the issue at stake. However, a couple of points ought to be noted. First, the correlation between height and productivity is pretty weak at the individual level, and non-existent at the group level. Secondly, and most significantly for our present purpose, almost all the subjects had a below normal weight-to-height ratio, and hence do not constitute a SBH-relevant set.

On the whole then there is as yet no compelling reason to suspect that 'pure' stunting hampers productivity in adult life.

(iv) Cognitive development

Cognitive ability is a catch-all phrase that denotes a number of separate functions (such as motor development, intersensory co-ordination, intelligence, learning skill etc.) which depend on the health of the neorological system. Scientific research on the effect of nutrition on these functions, though at a very nascent stage, has already identified a number of possible pathways through which severe protein-calorie deficiency may damage cognitive ability (Chase, 1976; Balazs et al., 1979). However, it is not yet established whether the biochemical processes involved in moderate stunting can produce similar results.

Nevertheless, empirical studies have almost always found a positive association between height and mental development. In a survey of studies on mild-to-moderate undernutrition, Pollitt and Thompson (1977) have come to the generalisation that among populations where malnutrition is endemic, indicators of human growth (stunting) as well as socio-economic variables correlate significantly with measurements of mental development. Some of the studies are not exactly relevant for our purpose since they compared only severe stunting with normal height. But the same cannot be said about a study by Klein et al. (1971) who looked for an association across the spectrum and found a correlation coefficient that was small (0.23) but statistically significant.[48]

These studies have given currency to the view that 'big is smart'. Seen merely as an association, this view is probably right. But to interpret it causally would be rather premature. One complication arises from the fact that the development of brain and the central nervous system as a whole depends on both nutrition from within and stimulus from outside. Furthermore, in actual socio-economic conditions in which malnourished children live, both nutrition and stimulus are often simultaneously deficient. It is therefore difficult, and often impossible, to determine to what extent, if at all, nutrition per se is responsible for slow mental development.[49]

In spite of this difficulty, it has been possible to

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48. For some recent evidence, see Jamison (1986) and Moock and Leslie (1986).

49. This is a general conclusion reached by several reviewers of the state of the art. See, for example, Lloyd-Still (1976), Martorell et al. (1978), Cravioto and Delicardie (1979) and Brozek (1982).

show fairly conclusively that episodes of <u>acute</u> undernutrition in early life do retard mental development. This has been shown for example by comparing children with a history of acute undernutrition with siblings (from the same family) without such a history but who presumably have been exposed to similar environmental stimulus. This convenient strategy cannot however be pursued in the chronic case (the case of moderate stunting) because marked difference in chronic undernutrition (as opposed to acute episodes) can seldom be found within the same family. Comparisons are therefore necessarily made across different environmental conditions, thus creating the problem of identification.

When this observation is combined with our lack of knowledge of how the biochemical processes involved in moderate stunting might affect cognitive development, it may seem tempting to defend the 'small but healthy' hypothesis by arguing that the observed mental retardation cannot be ascribed to nutritional deprivation. In other words, one might argue that the small may still be called healthy from the nutritional point of view (Pacey and Payne, 1984, p. 113).

There is however a difficulty in this argument. One may accept that the nutrition-induced biochemical processes that are involved in stunting do not cause impairment of cognition and that stimulus is the culprit; but that does not eliminate the role of nutrition. It is important to realise that nutrition and stimulus are not two independent factors. Stimulus depends a great deal on a child´s exploratory activities, of which play is a very important medium; and activity is very much a function of nutrition. It means that unlike other nutritional functions, cognitive development need not be affected by the pathways of biochemical processes alone. An alternative pathway exists in the form of: nutrition → activity → stimulus → cognitive development. In this way, the very same phenomenon of nutritional constraint that leads to stunting through the pathway of biochemical processes may also retard mental development through the pathway of activity. In that event, the mental retardation of a stunted child cannot but be viewed as a loss of nutritional capability.

Summing up

In view of the preceding discussion, we find it necessary to make a distinction between two possible interpretations of the 'small but healthy' hypothesis: (A) moderate stunting does not impair any nutritional capability, and (B) a moderately stunted child does not suffer from any impairment of nutritional capabilities.

The first is a causative statement; and we feel inclined to treat it sympathetically in view of our discussion of all the functions mentioned above. The second is an associative statement; and here our discussion on cognition points to the need for caution. We should not simply assume that a moderately stunted child is nutritionally sound, even if we accept the statement (A).

The causative statement is relevant for the dietary approach to the assessment of nutritional status, while the associative statement is relevant for the anthropometric approach. The validity of the causative statement would imply that there is room for intra-individual variation in requirement through costless adaptation in the rate of physical growth. Accordingly, the minimum dietary needs of children ought to be based on a growth rate which lies at the lower end of the homeostatic range rather than at the top of genetic potential, provided the diet allows for an adequate level of activity commensurate with proper cognitive development.

On the other hand, the doubt over the associative statement leaves one in quite a quandary. While one cannot accept that a stunted child is necessarily healthy, neither can one go back to embrace the genetic potential theory because the falsity of the associative statement does not imply the truth of the converse. In other words, one cannot assume that a stunted child has necessarily suffered from nutrition-constrained cognitive retardation. That would depend on whether the child had actually reduced his activity at the same time that it became stunted. For all we know, it may not have done so and may have actually absorbed the entire nutritional constraint through adaptive stunting. By looking at anthropometry alone, one cannot be sure.

In fact the relationship between nutrition and activity creates quite a general problem for anthropometry, not just in the context of 'small but healthy' hypothesis. We pursue this general problem further in Section IV and discuss some of its implications in Section V.

IV Food, Environmental Hygiene and Physical Activity[50]

The Problem of Incomplete Measurement

Assume for the moment that all the debates on adaptation in both efficiency and physical growth have been satisfactorily resolved. So we know how to set up minimum

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50. Parts of this section and the next draw heavily from the author's previous unpublished work which was used as background material for preparing the Technical Appendix in FAO (1985).

dietary standards and how to define anthropometric norms below which function of some kind is expected to be impaired. We may simplify further and assume away any genotypic variation in the energy requirement among individuals of the same age, sex, and physical parameters. Can we now measure the extent of undernutrition by using either dietary standards or anthropometric norms?

The answer unfortunately is still 'no'. In fact, we shall presently demonstrate that the following three propositions are generally true:

(i) Neither the dietary approach nor the anthropometric approach <u>fully</u> captures the set of undernourished population.

(ii) Each approach captures a <u>different</u> subset of the undernourished people, although there may be a <u>partial</u> overlap between the two.

(iii) Even a combination of the two approaches cannot capture the complete set of undernourished people.

Let us take up the anthropometric approach first. Once the appropriate anthropometric standards have been identified, any observed deficiency relative to those standards would certainly be an indication of undernutrition. But the problem consists in the fact that the converse is not necessarily true i.e., adequate anthropometric status does not ensure that a person is well-nourished. Recall that the body needs energy for <u>both</u> maintaining its internal functions and to undertake physical activity in the external environment. If the available energy is less than minimum requirement, then the body has two options: either to reduce bodysize below the minimum desirable level or to reduce physical activity. (Of course, some combination of the two is also possible.) If the first option is chosen, we shall observe the outcome as an anthropometric shortfall. But if only the second option is chosen, anthropometric status will be maintained at a satisfactory level. Yet we must say that the person is undernourished for the simple reason that the energy available to his body was not enough to meet the minimum requirements for maintaining both his body and his physical activity at the stipulated levels.

The anthropometric approach is thus intrinsically incapable of measuring the complete set of undernourished people.[51] What it ideally measures can be called physiological dysfunction.

The dietary approach too yields an incomplete measurement, but in a different sort of way. Recall that dietary standards are usually set up with reference to either western populations or laboratory subjects who happen to live in a near-ideal condition of environmental hygiene. Most people in the developing world however live in poor hygienic conditions and are much more susceptible to infections and disease, is now being recognised to have a most profound effect on nutritional status.[52] Some researchers even tend to accord a greater role to environmental hygiene compared to access to food in explaining the present state of undernutrition in the developing world (Mata, 1977).

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51. This has been one of the recurring themes of Beaton´s recent writings on nutritional measurement. See Beaton (1983, 1985).

52. For a description of the mechanisms through which infection may affect nutrition, see Beisel et al. (1967), Scrimshaw et al. (1968), Mata (1975), Beisel (1977), Scrimshaw (1977) and Chen and Scrimshaw (1983), among others.

In general, infection may precipitate undernutrition by affecting both intake and requirement of food. Anorexia (loss of appetite) is the principal mechanism through which infections and disease reduce the intake of food; some cultural practices also play a contributory part in reducing the food allocation for a child when it is taken ill. Mata (1978) has shown in a celebrated study of the children of Santa Maria Cauqué (in Guatemala) that it is the reduced food intake due to illness that explains most of the nutritional deficiency.

Requirement on the other hand is affected in a number of ways. Firstly, malabsorption of food, specially in diarrheal diseases, either through vomiting or increased fecal and urinary losses, reduces the effective utilisation of food. Secondly, parasitic elements thriving in the gastro-intestinal tract claim a share of the food and reduce the amount available to the body.[53] Thirdly, requirement is increased directly because of increased tissue catabolism and diversion of energy for the production of various host-protective factors.

Now insofar as infection affects actual food intake, the dietary approach can in principle take note of it, and the assumption of ideal environment does not make any difference. But not so in the case of requirement; here it does make a difference. The worse the environment (i.e., the greater the frequency and severity of infection), the

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53. In a sense, the effect of the first two factors can be seen either as reduced effective intake or as increased requirement. But since dietary intake is recorded at the level of ingestion rather than absorption, it is proper to view it as increased requirement specially in the context of dietary measurement of undernutrition.

greater is the need for energy.[54] Consequently, the usual dietary standards necessarily underestimate the energy requirement appropriate for the prevailing conditions in the developing world. People who are consuming adequate food according to these standards may still be undernourished, because their actual requirement may be higher due to poor environmental hygiene. The dietary approach is thus incapable of capturing the full set of undernourished people.

It should be noted, however, that unlike the limitation of anthropometric approach this limitation is not intrinsic to dietary measurement. For after all what stops us from devising requirement standards appropriate to the environmental condition of each region or country in the world? In principle of course this can be done, but there are very serious practical problems. The difficulty consists in the fact that people do not live in a similar environment even in a small locality, not to speak of country or region. This diversity cannot be glossed over, because even the inter-household differences in micro-environment can make enormous differences in susceptibility to infection. The need to take note of this diversity creates two kinds of informational problem.

First, we shall have to know a great deal about each household's or person's micro-environment. This is no simple task, especially because the relevant environment is not defined by a single factor but by a multitude of factors (such as water supply, housing, sanitation etc.) many of

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54. This does not however mean that supplying additional energy is necessarily the best strategy for dealing with poor environmental hygiene. The question of strategy will be discussed in the next section. Here we are concerned with the assessment of deficiency in a given environment.

which are not even easily quantifiable.

Secondly, we shall be required to quantify on the one hand the relationship between environmental quality and the risk of infection, and on the other the relationship between degrees of infection and nutritional requirement. The present state of knowledge is woefully inadequate to permit any of these quantifications with any reasonable degree of confidence.

Therefore for all practical purposes the dietary standards must assume, for at least some time to come, an ideal level of environmental quality, and hence must fail to measure undernutrition in its entirety.[55]

It is thus clear that both anthropometric and dietary approaches would fail to measure undernutrition completely even if all the adaptationist arguments were resolved. But it is important to note that each fails in a rather different way. In fact, the failure of one happens to be the strong point of the other. Thus, while anthropometry fails to capture undernutrition when it is manifested in reduced activity, dietary approach can capture it quite successfully. For, the latter is concerned only with the imbalance between need and intake; and how the imbalance is manifested does not make any difference to it. On the other hand, the failure of dietary approach to deal with a variety of micro-environments is no problem for anthropometry at all. Whatever the environment in which a person lives, if the food intake in the given environment leads to physical

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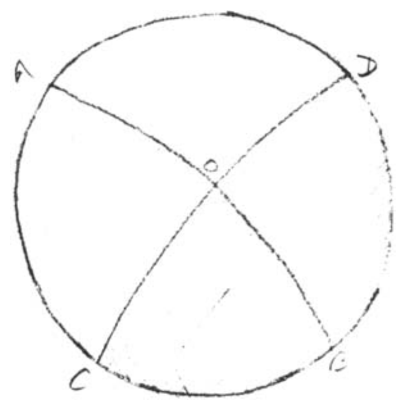
55. Some pioneering attempts are being made under the auspices of the United Nations University to develop dietary standards appropriate to the actual conditions of developing countries, but there is still a long way to go. See Turun et al. (1981) and Rand et al. (1984).

undernutrition, anthropometry can (ideally) capture it.

In a sense, therefore, dietary and anthropometric approaches are complementary to each other. But unfortunately it does not follow that when used in tandem, they will together provide the correct picture. For, firstly there is likely to be a partial overlap of unknown magnitude, and secondly there is one part of the undernutrition set which is out of reach for both approaches. These relationships can be shown schematically by the following diagram.

Diagram 1



The full set of undernutrition is shown by the area within the circle ACBD. Whatever be the micro-environment of a person, if his intake is less than the requirement for that environment, he belongs to this set, regardless of whether the actual outcome is suboptimal bodyweight or suboptimal activity. The dietary approach misses out those whose intake is enough for the chosen level of environmental quality, but not enough for their particular environments. Let such people belong to the subset AOBD, so that the subset actually captured by dietary approach can be represented by the area AOBC. On the other hand, the subset captured by anthropometry is shown by the area DOCB. These are the people whose energy deficiency relative to their respective environments has led to physical undernutrition, with or without accompanying reduction in activity. The overlapping area COB represents those whose intake is not enough even for the ideal environment and the resulting deficiency is manifest in physical undernutrition.

The out-of-reach area is shown by AOD. The situation here is the exact reverse of the overlapping area COB. Here the people have enough intake for an ideal environment, but not enough for their actual environment (hence out of reach of the dietary approach), and the resulting imbalance has been manifested entirely in reduced activity (hence out of reach of the anthropometric approach).

A matter of definition?

When we claim that neither dietary nor anthropometric approach can measure undernutrition fully, we are taking what we believe to be a widely accepted notion of undernutrition. This notion is characterised by its exclusive focus on the quantitative relationship between needs and intake. Neither the genesis nor the particular outcome of this relationship matters in defining a person as undernourished.[56] That is why, we call a person undernourished when infection creates deficiency by raising his requirement, even though he may be apparently eating well. Similarly, if the outcome of deficiency turns out to be reduced activity rather than reduced bodysize, we still call him undernourished.

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56. But of course it is extremely important to know about both genesis and outcome, if only because they do matter in policy formulation. More on this in Section V.

In contrast to this all-embracing notion of undernutrition, Payne and his colleagues have recently been arguing for a more restrictive concept. Although they present it as a matter of definition, we wish to get the matter straight, firstly because the subject matter is already confusing enough without the help of different meanings being attached to the same name, but especially because the proponents seem often to mix up definitional issues with matters of substance.

One of the recurring themes in Pacey and Payne (1984) is the advocacy of the idea that undernutrition occurs only when the outcome of deficiency is manifest in some physiological dysfunction; the level of activity has nothing to do with it. Thus they quote approvingly from Jelliffe (1966, p. 8) who defined malnutrition as a state of nutritional imbalance that is "... clinically manifested, and detected <u>only</u> by biochemical, anthropometric or physiological tests" (emphasis added).

It should be noted however that Jelliffe was giving a general definition of <u>malnutrition</u> arising from deficiency of any of the essential nutrients, not just of energy. Moreover he was writing in an era when nutrients other than energy were of primary concern.[57] In that context, his definition had some obvious merit. All the nutrients, other than energy, are needed by the body for functions <u>within</u> the body. Naturally, deficiency in these nutrients will be necessarily manifested in some internal disorder. But not so in the case of energy. As Beaton (1985) pointedly reminds

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57. The realisation that energy deficiency is the most serious aspect of the nutritional problem in the developing world is a relatively recent phenomenon. See, for example, McLaren (1974).

us, energy has the unique feature that, unlike other nutrients, it supports <u>both</u> internal and external work. Deficient energy can therefore cause either internal or external dysfunction. Accordingly, the same functional concept of undernutrition that focuses exclusively on physiological dysfunction in the context of other nutrients should take into account both internal and external function in the case of energy. It is interesting to note that Pacey and Payne fully condone the functional concept of undernutrition. Yet when they talk of function they only mean internal function. This is patently inconsistent with a truly functional concept of undernutrition.

Pacey and Payne seem to claim that their restricted definition follows from the adaptationist view of nutrition. This is where definition gets mixed up with matters of substance. Note first of all that they take a much broader view of adaptation than most other people who bring the idea of adaptation to bear upon the criterion of undernutrition. Sukhatme, for instance, is interested primarily in adaptation in energy efficiency, and Seckler in physical growth. But Pacey and Payne talk about a "total strategy of adaptive responses" which "comprises individual and social changes in behaviour as well as changes in bodyweight, body composition and metabolic regulation" (p. 63). A person is said to become undernourished only when all these adaptive mechanisms get exhausted.

Of particular interest here is the idea of adaptation through 'individual and social changes in behaviour' which essentially means reducing the level of physical activity. Now if this particular adaptive mechanism gets exhausted (i.e., comes down to zero) and energy deficiency still persists, then obviously there is no option but to suffer from physiological dysfunction. In this sense the choice of physiological dysfunction as the exclusive criterion of undernutrition would seem to follow from the notion of 'total adaptation'. In fact, Pacey and Payne make this logic quite explicit while explaining their choice of a dietary criterion of undernutrition. In general, they do not have much faith in the dietary criteria at all, but if a choice has to be made they would prefer the cut-off point of 1.2 BMR used by FAO (1977).[58] Now the interesting point about this cut-off point is that it does not allow for any physical activity. It is based on the notion of 'maintenance requirement' which is placed at 1.5 BMR and is deemed to be just adequate to maintain bodyweight when a person is doing nothing more than eating and sleeping. After allowing for adaptation in metabolic efficiency, the critical limit is then set at 1.2 BMR (i.e., two standard deviations below 1.5 BMR). Pacey and Payne call it the 'absolute limit of adaptation' and count only those as undernourished whose intake falls below this limit. Obviously, all such people would suffer from physiological dysfunction even after they have 'adapted' their activity down to zero.

But is it right that the 'adaptationist' view should lead to a definition of requirement which assumes a zero level of activity? Recall that what is relevant for defining a requirement standard is not just any form of adaptation but only those which do not entail any functional cost. Thus Sukhatme proposed to bring down requirement because adaptation of efficiency, in his view, allowed a person to reduce intake without incurring any functional cost. One can similarly argue that activity can be adapted down to a certain extent without any cost. But unless one makes the absurd suggestion that it can be driven down to zero without any cost, one must define requirement with reference to some

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58. Actually, Payne's advice was influential in persuading FAO to use this criterion in the first place.

positive level of activity. One will then have to recognise that deficient energy can lead to either internal disorder or reduced activity. Naturally, it will then be quite wrong to define undernutrition exclusively in terms of physiological dysfunction.

Elsewhere, Payne and Cutler (1984) seem to concede this point by using the term 'social cost malnutrition' to describe the situation where deficient energy leads to unacceptable reduction in activity. But they are still opposed to the use of a dietary criterion incorporating a positive level of activity. Their argument is that such a criterion will fail to distinguish those whose low activity is due to energy constraint from those whose activity as well as intake are constrained by external factors such as employment opportunity; and in their view it is only the former category that deserves to be called (social cost) malnourished.

There is indeed a problem here. But it has nothing to do with the principle of whether or not some positive level of activity ought to be chosen. It is rather a problem of how to choose an activity norm in a world of less than full employment. There are actually two possible options to choose from: either (i) to choose an activity level that is commensurate with the employment opportunity that is actually open to a person given the constraints on demand for labour, or (ii) to choose a socially desirable level of activity commensurate with the kind of gainful employment that we would ideally like him to have. There are those, such as Beaton (1985), who would prefer the second option. In doing so, they of course recognise that low energy is not necessarily the limiting constraint on activity for all those who are found to have both low intake and low activity. But they still consider such people to be inadequately nourished, while Payne and Cutler refuse to do so. We believe that there is room for genuine disagreement here. But this is not germane to the issue at hand. If Payne and Cutler insist on their particular notion of 'social cost malnutrition', all they have to do is to choose the first option of defining an activity norm. In no way does this justify the position that the criterion of undernutrition should altogether disregard the level of activity.

It is thus clear that undernutrition cannot be equated with physiological dysfuntion either by invoking the adaptationist view of nutrition or by pointing to the difficulty of choosing an activity norm. The two can be equated only by definition. However, as we have argued, such a definition will not be fully consistent with the functional view of nutrition which Payne and his colleagues happen to expound.

Of course, the concept of physiological dysfunction is an important one in its own right, and it is certainly worthwhile to try and identify the set of people who are afflicted with this syndrome. What we object to is the attempt to reduce the concept of undernutrition to the narrower concept of physiological dysfunction.

V Implications for economics: concepts and policies

In this section, we propose to piece together the preceding discussion on nutritional controversies and explore some of their implications for economics. Specifically, we shall try to assess the implications for the concept of poverty line (and the measurement of poverty), the assessment of capabilities and the formulation of food and nutrition policies.[59]

Poverty line and the measurement of poverty

As is well known, the absolute poverty line is usually defined in relation to a norm of nutritional requirement, frequently of calories alone. Accordingly, a person is said to be absolutely poor if his income or diet is inadequate to achieve the desired norm of calorie requirement. The measurement of poverty is thus closely linked to the dietary measurement of undernutrition. We shall presently see that they are not necessarily the same thing, but the linkage is undeniable. We shall therefore proceed by first noting the problems in the measurement of undernutrition as such, and then explore the implications for the measurement of poverty.

Our discussion in the preceding sections suggests that the use of an 'average' calorie norm for the dietary measurement of undernutrition has to contend with three major difficulties:

(i) The use of an average norm is unjustified if one allows for adaptive intra-individual variation in requirement. We have seen in section II that the particular adaptive mechanism of variable metabolic efficiency (â la Sukhatme Mark II) is not borne out by scientific evidence. But it is not quite so easy to dismiss the possibility of intra-individual variation in requirement arising from adaptation in

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59. The implications for yet another aspect of economics, viz. the positive theory of labour market, have recently been explored by Dasgupta and Ray (1986).

the rate of physical growth (à la Seckler).[60]

(ii) The use of 'average' cannot allow for inter-individual variation in requirement arising from genetic differences. Note that such variation will continue to exist even if we allow for intra-individual adaptation in defining the requirement norm. The allowance for adaptation simply means that the requirement norm is to be set at the lower end of the range of adaptation. But the range of adaptation can vary interpersonally due to genetic differences, thus leading to inter-individual variation in requirement.

(iii) The existing requirement standards are based on ideal environmental conditions, and are hence inappropriate for those whose requirement is increased by poor environmental hygiene (see section IV).

All three problems lead to a common consequence: misclassification of the undernourished. The first problem leads to the error of overestimation because if requirement adapts intra-individually, then the appropriate criterion of undernutrition is the minimum and the not average of the range of adaptation. The third problem on the other hand creates the opposite error of underestimation; many undernourished people living in a poor environment will not be classified as such because their intake, while being inadequate for their own environment, may be high enough in relation to an ideal environment. The second problem also

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60. This follows from the observation that the causal interpretation of 'small but healthy' hypothesis cannot be refuted in the light of existing evidence (section III).

creates a problem of misclassification, but the direction of bias is indeterminate in this case. If different people have different requirements, then some of those classified as undernourished by the criterion of average requirement will not actually be so, while some of the truly undernourished will not be classified as such.[61] Whether the net effect will be overestimation or underestimation, or whether the biases will cancel each other out cannot be predicted *a priori*.[62]

What does this problem of misclassification imply for the measurement of poverty when the same 'average norm' is used to determine the poverty line? Clearly it implies that the set of the poor, defined with reference to such a poverty line, will not in general be identical to the set of the undernourished.

Note that in the actual estimation of poverty, the

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61. Recall the discussion on Type I and Type II errors in section II. The various expert committees which calculated the recommended dietary allowances were always aware of these possible errors and explicitly warned against using the average norm as the criterion for assessing undernutrition. The use of average, in their view, lay in the assessment of aggregate food deficiency and in planning overall food supply. See Hegsted (1972).

62. While using the average norm, Reutlinger and Selowsky (1976) had expressed the hope that the biases will offset each other. But Srinivasan (1981, p. 10) and Lipton (1983, p. 27) later conjectured that overestimation is the more likely outcome. Kakwani (1986, p. 10) has recently provided a formal proof of this conjecture for the typical case where average requirement is higher than the mode of intake distribution. But the result is valid only for the special case of zero correlation between intake and requirement. Once the existence of correlation is acknowledged, the direction of bias can be shown to be indeterminate. See the results of simulation in Kakwani (1986, p. 57).

divergence between the poor and the undernourished can arise from yet another source. It has to do with the usual practice that poverty line is defined, not as the calorie norm itself, but as an income (or expenditure) level that would satisfy this norm. Now the problem arises from the fact that income and calorie intake are not monotonically related across individuals; what one actually uses in practice is an average relationship between income and intake. But the lack of monotonicity implies that having an income above the poverty line is no guarantee that calorie intake too will be above the required level. Similarly, having an income below the poverty line does not indicate that calorie intake is actually deficient. Thus, once again, there arises a divergence between the poor and the undernourished. This was in fact one of Rao´s (1977, 1981) criticisms of Dandekar and Rath´s (1971) estimation of poverty in India.[63]

But how damaging is this problem of divergence, from whichever source it may arise? Does this really warrant any revision in the methodology of constructing poverty line and consequently in the measurement of poverty? We shall argue that the answer depends, not surprisingly, on how one perceives the conceptual foundations of poverty.

Consider the general concept of living standard, of which poverty is merely one end of the spectrum. As Sen (1984) has recently argued, the concept of living standard

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63. Sukhatme (1978, p. 1383) also notes this problem, but he attributes it wrongly to the neglect of intra-individual variation in requirement. Actually, variation in requirement has nothing to do with this particular problem. Even if there were no inter- or intra-variation in requirement, misclassification would still arise from the use of an income standard simply because of the lack of monotonicity between income and calorie.

can be interpreted in at least three different ways: in terms of either utility, opulence or capability. Of the three, the last two interpretations are particularly relevant in the present context. In the opulence approach, the focus is on the commodity bundle a person happens to enjoy; he is said to be more opulent with a bundle x than with y if he prefers x to y.[64] In the capability approach on the other hand, the focus goes beyond the commodity bundle and looks at the way the possession of commodities enables a person to carry out various functions; if the bundle x gives him a greater capability to function than does y, his living standard is said to be higher with x than with y.

Poverty then can be interpreted as either the lack of opulence or the lack of capability, 'lack' being defined in either case as shortfall from a chosen standard. When the concern is with absolute poverty, as opposed to relative inequality, it is of course necessary to anchor the chosen standard to some objective phenomenon of deprivation; and since nutritional deprivation is considered to be the most depressing aspect of deprivation in the poor societies, it is customary to anchor this norm to the criterion of nutritional requirement. The linkage with nutrition is therefore common in both approaches to poverty. But there is a difference in the interpretation of deviation from the norm. In the capability approach, any shortfall from the norm should imply inadequate capacity to function owing to nutritional deficiency. In other words, poverty ought to be measured in the scale of nutritional status. On the other hand, in the opulence approach, poverty is to be measured in

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64. Despite an apparent similarity, this is not the same thing as evaluation of utility; at best it can be thought of as evaluation of <u>the commodity basis</u> of utility. For a lucid exposition of the subtle but fundamental difference between the two, see Sen (1984).

the scale of commodity status. Of course, the reference standard in this scale (i.e., the poverty line) will still be linked to the level of desirable nutritional status, but the shortfall of commodity status from the poverty line need not reflect a corresponding shortfall in nutritional status.

Clearly, if one takes an opulence view of poverty, one need not be disturbed by the fact that the traditional ways of measuring poverty fail to identify the truly undernourished people. As a matter of fact, arguments based implicitly on the opulence view have often been put forward to defend the traditional way of measuring poverty against criticisms arising out of the recent nutrition debate.

Consider for instance the problem created by the lack of monotonicity between income and calorie. Dandekar (1981) is aware that in assuming away this problem through the use of an average relationship between income and calorie, his poverty line income implicitly assumes an average standard of household management. Accordingly, he recognises that some of the poor may be able to satisfy their calorie needs by a better than average standard of household management, while some of the non-poor may fail to do so through poor management. But that does not worry him, because poverty for him is not the phenomenon of a household actually failing to meet calorie needs, bu of living "... on such levels of consumer expenditure that judged by average standards of household management, it could not provide for itself diet adequate even in terms of calories" (p. 1243, emphasis added). Here the concern is clearly with opulence or the lack of it, not with nutrition as such.

A similar argument has been made by Sen (1980) in response to the problem created by inter-individual variation in requirement. He argues that "Malnutrition can provide basis for a standard of poverty without poverty being identified as the extent of malnutrition. The level of income at which an average person will be able to meet his nutritional requirements has a claim to being considered as an appropriate poverty line even when it is explicitly recognised that nutritional requirements vary interpersonally around the mean". Clearly, poverty defined in this way will not reflect the extent of undernutrition, but it will enlighten us "on an income deprivation related to some average standard". Like Dandekar, Sen too is taking here the opulence view of poverty.

The remaining two problems i.e., those due to intra-individual adaptation in requirement and to variation in environmental hygiene, can also be dealt with in a similar manner. One only has to define the poverty line as a point in the scale of opulence which in an ideal condition of environmental hygiene will just satisfy average calorie requirement under average household management and without obliging a person to take recourse to his adaptive capacity. There is then nothing in principle wrong in accepting the usual figures of calorie requirement as the basis of a reference standard. Poverty is now simply defined as income deprivation in relation to this standard. Obviously, income deprivation in this sense may or may not reflect nutritional deprivation, and conversely nutritional deprivation may occur even when income deprivation does not. But that need not worry us when we take the opulence view of poverty.

One may however ask: is opulence or the lack of it the most appealing interpretation of poverty? Sen (1983) has in fact argued convincingly that capability rather than opulence can claim to be a more natural interpretation of poverty. It is after all the notion that poverty consists in the lack of some basic capabilities that gives it an absolutist core and makes it different from relative inequality. Therefore, if we are interested in measuring absolute poverty, it would seem natural to measure it on the scale of capabilities. From this perspective, we can no longer view the divergence between poverty and undernutrition with the same equanimity which we could afford in the opulence perspective. For, when one uses the yardstick of nutritional capability, the content of poverty is nothing other than undernutrition. Of course, one could wish to take a much broader view of poverty, covering more than nutritional capability. In that case, a person could be poor without being undernourished. But an undernourished person must be considered poor, as long as the lack of nutritional capability remains one of the defining criteria of poverty.[65] The problem of misclassifying the undernourished then becomes a matter of concern.

Naturally, the traditional way of measuring poverty on the basis of 'average norm' will no longer do. We shall have to face all the problems of measuring undernutrition that we mentioned earlier. Serious problems of empirical estimation will arise in the process. But, the principles of an appropriate methodology can be easily described.

Firstly, the problem of lack of monotonicity between income and calorie may be tackled by adandoning the income

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65. Strictly speaking, one can still draw a distinction between poverty and undernutrition in so far as there is a distinction between the capability to be nourished and being actually nourished. Sen (1984) makes the point starkly by giving the example of an ascetic who might choose to fast and become undernourished despite his being rich and having the means of being excellently nourished. In most cases, however, people will be undernourished not because they choose to do so, but because their capability is somehow constrained. For all practical purposes, therefore, the capability view of poverty (in the nutritional dimension) can be identified with the concept of undernutrition.

standard and comparing actual calorie intakes with the required level.

Secondly, intra-individual adaptation in requirement can be taken care of by setting the requirement norm at the lower end of the range of adaptation. The practical problem, however, is that the range of adaptation is not yet known with any reasonable degree of confidence.

The remaining problems of inter-personal variation in requirement due to genetic differences as well as different environmental conditions are much more difficult to handle. As we have noted earlier the use of average, or any other single figure of requirement, will fail to identify correctly who are actually unable to meet their respective requirements. Technically, the solution lies in evaluating a joint probability distribution of intake and requirement, which in simple terms means counting how many people have intakes below their respective requirements. This approach has sometimes been used for measuring the extent of nutritional deficiency (e.g. Lorstad, 1971; Reutlinger and Alderman, 1980). But it suffers from serious informational constraints. Basically, four types of information are needed for its application: the parameters of the intake distribution function, the parameters of the requirement distribution function, the correlation between intake and requirement, and finally the functional forms of intake and requirement distributions. Among these, information on intake distribution is perhaps the easiest to obtain, although that too can pose serious problems when it comes to individual as distinct from household distribution. Something is also known about the mean and variance of requirement distribution arising from genetic differences; but these relate primarily to ideal environmental condition. The nature and extent of variation due to different environmental hygiene is as yet in the realm of unknown. So too are the parameter of correlation between intake and requirement, and the functional form of requirement distribution. Useful insights can be gained by simulating with different values of parameters and different functional forms, as Kakwani (1986) has recently done with Indian data. But the main stumbling block remains the almost complete lack of knowledge about differences in environmental hygiene in which people actually live and the variation in requirement arising from that.

On the whole then, one may conclude that the measurement of poverty in the sense of capability is faced not so much with conceptual problems as with informational constraints, but with constraints that would appear to be well-nigh impossible to overcome in our existing state of knowledge. This is however not an argument for abandoning the capability concept of poverty, but for strengthening our efforts to elicit the required information.

Assessment of capability

The concept of capability is of course of interest in itself, not merely as a basis of poverty. In comparison of welfare between population groups, capability has a claim to be the essence and the measure of welfare.[66] From this perspective, the issues relating to the assessment of nutritional capability is of direct concern to the welfare economist.

We have already noted some of the problems of assessment

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66. Sen (1985) has argued forcefully for accepting capability as the conceptual basis of social welfare in preference to alternative bases such as utility or opulence.

in the context of measuring poverty from the capability perspective. Many of the problems in fact arose from the fact that the assessment was being done indirectly through a comparison between intake and requirement, and this was rendered difficult by the variation of requirement across individuals. But this particular problem does not arise when capability is assessed directly. Whatever the nutrient requirements may be, and however they may vary, the relationship between intake and requirement will be revealed in capabilities. If we can measure capabilities, we need not worry about requirements, nor for that matter about intakes.

However,the measurement of capability is not all that simple. As we have seen, nutritional capabilities can span several dimensions e.g. immunocompetence, physical work capacity, cognitive skill etc. Assessment of these functions demands a great deal of time, expertise and resources. When large population groups are involved, the exercise may become practically infeasible. But we have also seen that anthropometry is often used as a convenient short-cut, specially in the case of children, on the assumption that the achievement of physical growth is a reliable indicator of functional achievement. In his pioneering attempts to apply the concept of capability in actual comparisons of welfare, Sen too has sometimes used anthropometry as an indicator of nutritional capability, and has actually expressed a preference for anthropometry over the intake-requirement approach.[67] This preference is of course understandable in view of the problems with the latter approach discussed earlier. But our discussion in sections III and IV suggests that there are good reasons to be wary about the anthropometric approach too.

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67. As for instance, in exploring the issue of sex-bias in the allocation of household resources. See Sen (1984a) and Sen and Sengupta (1983).

In the first place, any attempt to use anthropometry as an indicator of welfare will have to contend with the homeostatic theory of growth, the so-called "small but healthy" hypothesis. What this theory does is to challenge the assumption of a monotonically increasing relationship between anthropometry and nutritional capabilities and to suggest instead that capabilities remain invariant within a range of anthropometry. We have seen (in section III) that there is a lot of force in this proposition when interpreted in a causal sense i.e., it seems quite plausible that moderate stunting within a certain range may not <u>cause</u> any impairment of functional capabilities. However, we have also seen that within this range, lower anthropometry may well be <u>associated</u> with lower level of one particular type of nutritional capability viz. cognitive skill. Consequently, while one cannot assume a monotonically increasing relationship, neither can one be sure that all capabilities will actually remain invariant within the specified range. One would not therefore be able to ascertain what really is the status of nutritional capabilities within the range of causal invariance. This does obviously create a problem in using anthropometry for the assessment of nutritional capability.

One way of getting around this problem would appear to lie in using anthropometry as an index of general capabilities, incorporating nutritional capability but going beyond it. It is generally agreed that whether or not a stunted child lacks any nutritional capability, he is certainly more deprived in a general sense compared to a normal child. If he were not in fact more deprived in terms of food and environmental status he would not have been stunted in the first place. That is why, even those who question the use of traditional anthropometric standards for the assessment of nutritional status, do not hesitate to add that anthropometry is nevertheless one of the most reliable indicators of general social deprivation (Goldstein and Tanner, 1980; Dowler et al., 1982). In terms of capabilities it means that whatever the status of nutritional capability, a stunted child is most likely to lack other capabilities associated with food and environmental quality.[68] Therefore, regardless of whether the 'small but healthy' hypothesis is valid or not (either in the causative or the associative sense), an economist would appear to be right in using anthropometry as long as he is aware that he is assessing a much broader range of capabilities than those associated with nutrition alone.[69] Except, that is, for one problem.

The idea of taking the route of general capabilities was to ensure that an overall monotonic relationship could be postulated between anthropometry and capability even though the same could not be done for the nutritional capabilities as such. This strategy will work as long as each type of capability is either monotonically increasing or invariant with anthropometry, and at least some capabilities are strictly monotonically increasing (as the non-nutritional capabilities are expected to be). But a problem will arise

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68. Food, for instance, can be used, in addition to yielding nutrition, "to give eating pleasure and to provide support for social meetings" (Sen 1985, p. 9).

69. The implication of this distinction between nutritional and general capabilities has not always been correctly understood. Thus for example, Gopalan (1983), Beaton (1985), Martorell (1985) and several others have used the argument of general deprivation to dispose off the 'small but healthy' hypothesis. As Beaton explains his position: "not because small is bad in itself, it is deprivation that is harmful" (p. 230). But surely being deprived in terms of general capabilities is not the same thing as being 'unhealthy'. The argument of general deprivation could be brought against a hypothesis like 'small but well-off' which no one proposes; but in order to question the 'small but healthy' hypothesis the battle must be fought on the plane of nutritional capabilities alone.

if some of the capabilities turn out to be inversely associated with anthropometric achievement. Paradoxical as it may seem, this is indeed possible in respect of some nutritional capabilities. This becomes clear as soon as one recognises the possibility of a trade-off between physical growth and physical activity (section IV).

Consider two children of the same genotype living in the same condition of adverse 'food and environmental status'. They are obviously facing the same nutritional stress, but each may respond differently by choosing a different combination of physical retardation and activity reduction. Assume that one of them has become physically retarded by maintaining a high level of activity, while the other has avoided physical retardation by reducing activity. A striking example of the latter possibility was provided by Rutishauser and Whitehead (1972) who described an Ugandan child population in which physical growth was maintained at reasonable levels in the face of an apparent low intake; and "further examination of the community suggested that one way in which this was achieved was by a very low level of activity, play" (Beaton, 1983, p. 11).

Clearly, anthropometry would be a misleading indicator of capability in this case. Considering nutritional capability first, the child with 'high anthropometry, low activity' is more likely to suffer from cognitive retardation. Moreover, quite apart from its effect on cognition, the 'capability to play' may be valued in itself; and in that regard too he would be suffering a greater loss of nutritional capability. Thus even though his physiological functions may happen to be superior by virtue of a better physical stature, nothing can be said about his overall nutritional capability without introducing relative value weightings for different functions. As for non-nutritional capabilities, there is no reason for the two children to fare differently since by assumption they belong to the same 'food and environmental status'.

It is thus clear that anthropometry by itself would be a poor indicator of capabilities. It will generally be necessary to obtain additional information on the activity patterns of the populations concerned.

Food and Nutrition Policy

Arguments over the presumed policy implications of differing views on nutrition have aroused a great deal of passion in the recent years. Much of it however has been sadly misplaced and many false battles have been fought, although, as we shall see, there are also some useful lessons to be learnt from all this.

Most of the controversies have centred around the implications of 'adaptationist' views, with both sides of the camp often hitting out against the wrong target. A more substantive debate has sprung from the realisation that the genesis of undernutrition is a much more complex phenomenon than was traditionally thought; but this too has got mixed up with the adaptationist debate. The issues covered in the process can be divided up into two broad categories, viz. (i) whether the adaptationist view warrants any diminution in the policy concern with poverty and (ii) what should be the content of policy for different groups of population. The first issue has generated an entirely false debate, while the second contains elements of both false and genuine claims.

One of the reasons why the debate over adaptation has generated so much heat is the fear that by showing the magnitude of malnutrition and poverty to be much less than what it is believed to be, the adaptationists might encourage a potentially dangerous complacence on the policy front. A typical expression of this fear can be found in Zurbigg's (1983) statement that 'Sukhatme's argument can lead to politically expedient redefinition of poverty' (p. 2083). Now, politicians do of course manipulate academic ideas if they find it expedient to do so. But ideas must be judged on their own merit; and in this particular case they do not logically warrant the kind of fears expressed.

The adaptationists have generally been careful in drawing a distinction between nutritional poverty (i.e., undernutrition caused by poverty) and general socio-economic poverty. A person who has adapted to a low level of intake may have avoided nutritional poverty; but in doing so he does not become non-poor in a general sense, for it is his general poverty that has forced him to adapt in the first place.[70] In other words, the necessity to adapt is itself an indication of poverty. By this criterion, Sukhatme reckons that most of the people in rural India could be counted as poor, although undernutrition due to poverty may be no more than 15-20 per cent (Sukhatme, 1982b, p. 248).[71] One may disagree with the second part of the statement (in view of our discussion in section II), but there is certainly

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70. In view of our discussion earlier in this section, this position would imply either the acceptance of the opulence view of poverty, or the adoption of a broad-based capability view incorporating a wider range of capabilities than those associated with nutrition alone.

71. By the same token, Sukhatme (1982b) also questions the usefulness of the traditional dietary approach for the measurement of poverty (quite apart from questioning its usefulness for the measurement of undernutrition) In doing so, he is implicitly adopting the 'broad-based' capability view of poverty.

nothing in it to encourage a complacent view of poverty.

Similar confusion has surrounded the views of Seckler. Because he considers the stunted children to be 'healthy', he has often been interpreted to imply that stunting is not a matter of policy concern. Thus Martorell (1985, p. 25) interprets him as arguing that "planners should concern themselves with wasting and not with stunting"; and Gopalan (1983a, p. 34) raises the alarm that to accept Seckler's views "is to acquiesce (however unwittingly) in the preservation of the status quo of poverty, ill-health, undernutrition and socio-economic deprivation". Once again, what the critics fail to notice is the distinction between undernutrition and general deprivation that the adaptationists are trying to highlight. Seckler (1984) in fact states quite categorically that whether or not we call the stunted children undernourished, the fact that they have been forced to become small indicates that they are generally deprived; and as such they are certainly a cause for policy concern.

Encouraging complacence on the policy front would therefore hardly seem to be a necessary consequence of the views espoused by the adaptationists. However, they do claim that the adaptationist perspective calls for certain reorientation in the content of policy. We shall argue that some of the suggested reorientations are indeed deserving of serious consideration, but their merit in no way depends on the scientific validity of the adaptationist perspective.

Three specific issues have figured prominently in this context: (i) prioritisation, (ii) target group for feeding programmes and (iii) relative emphasis on food versus non-food policies.

(i) Prioritisation: One of the recurring themes running through the writings of all the leading adaptationists, such as Sukhatme, Seckler and Payne, is the claim that the acceptance of their view will lead to a more equitable allocation of resources. The argument runs briefly as follows: those who have successfully adapted are indeed deprived, but those who have failed to adapt and become undernourished are even more so. Thus the people identified as 'truly undernourished' by their criterion would usually belong to the neediest section of the population; by identifying them, one is helping to channel scarce food and other resources to those who need them most. By implication, it is suggested that the non-adaptationist view does not make any distinction between degrees of need and is not concerned with priorities. This point is in fact made most explicitly by Payne and Cutler (1984) who claim that in the genetic potential theory, as in Paretian neoclassical economics, one is not concerned with whether the benefit goes to the severest or the least severe cases of deprivation as long as somebody benefits without worsening anyone else´s condition.[72]

This is clearly a rather curious argument. The desire to concentrate on the most deprived cases has nothing to do with the nutritional phenomenon of adaptation. It arises simply from the value judgement that the gain of the neediest should be valued most. A devout non-adaptationist can equally hold this value judgement and may decide to give

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72. Elsewhere, Pacey and Payne (1984) take the precaution of pointing out that this is not a necessary implication of the genetic potential model, but that it could be interpreted in this way by unscrupulous policy makers. This is of course sheer polemics. But if one wants to play the game, one should also face the retort that the unscrupulous policy makers can misinterpret the adaptationist model too to feel complacent about the 'adapted'.

top priority to those farthest from the genetical potential even though he may believe that anyone below the potential is undernourished to some extent. Obviously, the difference in nutritional perspective need not entail any difference in policy as far as ranking of priorities is concerned.

(ii) Target group for feeding programmes: It has also been suggested that 'adaptation' makes certain types of policies unsuitable for certain categories of people. The criticism has mainly centred on the nutrition intervention policies such as supplementary feeding programmes which have traditionally enjoyed a great deal of support from the nutritionists. It has been argued from the perspective of 'small but healthy' hypothesis that such feeding programmes are inappropriate for the purely stunted children. The argument can be summed up as follows. Since the moderately stunted children do not have any obvious nutritional disability, additional food will do them no good. In fact, if one catches them late in their childhood when the scope for catch-up growth is practically nil, supplementary feeding will only serve to make them obese.[73] If caught early, they may of course get out of the stunted growth path and get back to normal growth; but in order to keep them there, either the feeding programmes will have to continue until adulthood which they seldom do, or one will have to do something about the basic socio-economic deprivation which produced stunting in the first place. It is therefore suggested that feeding programmes should be targetted primarily towards the 'wasted' children (or the severely stunted ones showing clinical lesions), and the problem of

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73. A recent review by Beaton and Ghassemi (1982) shows that supplementary feeding of shcool-age children has seldom resulted in any objective signs of nutritional improvement.

moderate stunting should be tackled through broader socio-economic policies which can strike at the root of poverty.

We believe, there is a great deal of sense in this proposition. But one does not have to believe in the 'small but healthy' hypothesis in order to appreciate it. As we have noted earlier (section III), a child which has achieved equilibrium with his environment through stunting can be said to have successfully avoided the evils of wasting. If he is now 'unstunted' by temporary feeding and then thrown back to the original environment, he will achieve new equilibrium with his higher height only with a lower weight-for-height ratio i.e., through wasting. Such acts of temporary feeding will be considered inappropriate if the original equilibrium is preferred to the new. But in order to have this preference it is not necessary to believe that only wasting is bad and stunting carries no evil; all that is needed is the undisputed proposition that wasting is a greater evil than stunting. Therefore, the wisdom of targetting feeding programmes primarily towards the wasted children should be equally visible from all perspectives.[74] It is however true that the inappropriateness of bringing stunted children into the network of feeding programmes has seldom been appreciated in designing actual programmes. This is reflected in the widespread use of the Gomez classification by weight-for-age, which as a selection criterion fails singularly to distinguish between wasting and stunting. Much more discerning criterion ought to be used if the feeding programmes are to reach the genuine target group.

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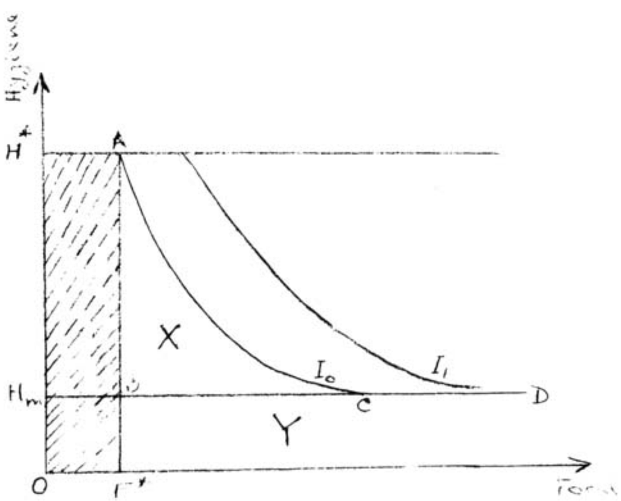
74. See, for instance, the judgement of a leading non-adaptationist, Scrimshaw (1982, p. 103).

(iii) Food versus non-food policies: Recent discussions on nutrition policy have frequently challenged the conventional wisdom that nutrition policy is basically about increasing access to food. In the emerging new perspective, non-food factors such as occupational pattern, environmental hygiene etc. may be of equal if not greater importance than access to food.

As Pacey and Payne (1984) rightly point out, the constraint imposed on mother's time by occupational patterns may be an important cause of child malnutrition in many poor societies. The nature of diet in poor societies is often such that the calorie content is very low in proportion to the bulk, so that a child has to be fed several times a day in order to ensure adequate calorie intake. Mothers may not be able to maintain the required frequency, if occupational demand keeps her away from home for long periods of time. In that case, the child will either go unfed or will have to make do with food prepared in bulk early in the day. In the latter case, the food is likely to become infested with germs given the poor condition of environmental hygiene; and given the fact that a child is most vulnerable to infection during the weaning stage, he is very likely to fall victim to infection-induced undernutrition. Giving the household more food in this condition will not help much to save the child. More fundamental measures are needed that will remove the occupational constraint on mother's time.

Similarly, emphasis has tended to shift away from access to food to access to better environmental quality (i.e., freedom from infection). We have seen in section IV that infection can widen the divergence between intake and requirement by both restricting intake and increasing requirement. The traditional emphasis on access to food seeks to bridge the gap by acting on the intake side of the inequality. In contrast, the new perspective emphasises the importance of tackling the problem from the requirement side (and also of relaxing the infection-constraint on food intake) through policies aimed at improving environmental hygiene. Clearly, the fight against infection is no less a nutrition policy than the struggle to ensure access to food. However, the relative importance of the two approaches has not always been judged on the basis of sound analysis. The adaptationists, for example, have been particularly forceful in advocating the case that nutrition policy should focus primarily on the environmental front, specially in countries like India where infection rather than food is in their view more of a limiting constraint. But it has not been appreciated that the choice of strategy does not simply follow from the nature of limiting constraint. The general nature of the problem can be illustrated with the help of the following diagram.

DIAGRAM 2



The combination of a person´s food intake and the environment in which he lives is represented by a point in this diagram. Food intake is measured along the horizontal axis and the factors denoting environmental hygiene (and somehow expressed as a composite index) are measured along the vertical axis. The curves I_0, I_1 etc. belong to a family of what may be called isonutrition contours. All points on a curve denote the same level of energy available to the body for its normal maintenance and external work, and hence the same nutritional status. The downward sloping segment of the curve signifies that as the non-food environment worsens more intake is required for providing the body with enough energy for normal maintenance and external work, after meeting the additional needs due to infections and disease. This captures the idea that energy requirement is increased when non-food environment worsens.

There are two critical points, H* and H_m, on the vertical axis. H* represents an ideal condition of environmental hygiene, so that the isonurition contours are shown truncated from above at this point. The other point H_m represents such a poor state of hygiene that additional food intake beyond a certain level gets wasted and therefore cannot contribute to the improvement of nutritional status.[75] In other words, when an isonutrition contour gets below the point H_m it becomes horizontal at some stage.[76] It also means that if I_0 happens to be the highest contour to become horizontal, all the contours above it will approach H_mD asymptotically.

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75. This may happen for instance in the case of acute diarrhea when extra food will literally go down the drain, or in the presence of parasites which eat up a large share of the food.

76. Presumably, the lower the contour, the lower will be the level of food intake at which it becomes horizontal.

Let I_0 also be the contour on which just enough energy is available to the body for maintaining bodysize and activity at the minimum desirable level. It can be described as the minimal desirable isonutrition contour which provides the basis for estimating food requirements at different levels of environmental hygiene.[77] Recall that the usual requirement standards refer to ideal hygiene i.e., to a point like H*. The corresponding food requirement is given by F*, and the population identified as undernourished by this criterion will belong to the area H*OF*A. Obviously, actual undernutrition will be much more than this because, by definition of the minimal desirable isonutrition contour, any point to the left of or below I_0 indicates undernutrition. The special characteristics of the subset H*OF*A is that, unlike the rest of the undernourished, these people face a limiting food constraint because their food intake is not enough even for an ideal environment. In fact, depending on the nature of the limiting constraint, the whole set of undernourished people can be split up into four subsets: (i) H^*H_mBA, (ii) H_mOF^*B, (iii) the area X which is below AC and bounded from below by the lines AB and BC, and (iv) the area Y which is below BD and bounded from the left by the line BF_m.

As we have just noted, for the people belonging to H^*H_mBA, more food is a binding necessity; their undernutrition cannot be removed by exclusive reliance on environmental measures. Those belonging to the area Y on the other hand have such a wretched environment that no amount of extra food will help on its own; for them, improvement

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77. The particular curve I_0 is chosen for this purpose purely for the sake of diagrammatic simplicity. It can be easily checked that none of the arguments below will be altered in essence if any other curve either above or below I_0 were chosen.

of health environment is an absolute necessity. For those belonging to H_mOF*B, more food and better environment are both binding necessities, neither will suffice by itself. Finally, for those in the area X, there is no binding constraint; their problem can be solved either by acting on food or environment alone or through a combination of the two.

Now if one accepts that only a small proportion of the undernourished face a limiting food constraint, it would mean that those belonging to the subsets $H*H_mBA$ and H_mOF*B (i.e., those identified as undernourished by the dietary criterion) are much fewer in number than those belonging to the other two subsets. But would that by itself indicate what strategy is optimal for the majority of the undernourished? Obviously not. Consider first the area X where there is clearly a trade-off between provision of food and improvement of hygiene. The economically optimal way of bringing a person onto the I_0 curve can only be determined by an analysis of the relative cost-effectiveness of alternative policies. For this one will have to know the exact location of the undernourished in relation to the I_0 curve, cost of food vis-a-vis environmental provision and total amount of resources available for the purpose. In fact, a similar economic analysis will be needed for each of the other three categories as well. Although one or the other (or both) of food and non-food factors can be viewed as limiting constraint in these cases, there is almost always a choice between alternative combinations of food and non-food endowments. Thus even when environmental improvement is an absolute necessity (as in area Y), one cannot maintain <u>a priori</u> that the choice of environmental measures alone is the optimal strategy; nor can one make even the milder statement that most of the resources should be devoted to environmental improvement. By the same token, neither can one claim that more resources should be necessarily committed on the food front even for the food-constrained population. Allocation of resources between alternative strategies is a distinct exercise from identification of the limiting constraint.

In any case, the relative emphasis to be placed on alternative strategies is quite independent of the notion of adaptation. If adaptation provides a buffer against nutritional stress, it does so irrespective of whether the stress arises from food constraint operating on the intake side or from the non-food factors operating on the requirement side. In terms of diagram 2, it merely has the effect of uniformly lowering the minimal desirable iso-nutrition contour. That by itself does not alter the relative importance of food and non-food factors. Conversely, there is nothing in the conceptual framework of the non-adaptionists that should prevent them from according a greater role to environmental policies. For example, Mitra (1978) clearly recognised the primacy of environmental policies from a non-adaptationist perspective. Gopalan (1983) has also noted how wrongly the adaptationists have been trying to usurp the case for environmental improvement as if its importance can only be perceived from their own perspective.

However, the fact remains that when it came to actual formulation of policy, the traditional nutritionists seemed almost always to accord the pride of place to the provision of food. Two of the most celebrated intervention programmes, viz. the INCAP project in Guatemala (Scrimshaw et al., 1969) and the Narangwal experiment in India (Kielman et al., 1980), added personal health care to the provision of food, but environmental hygiene was left out in both. The success achieved in these projects are now recognised to have been much too meagre compared to the resources deployed.[78] It is also recognised that the relative failure of these programmes is probably explained by the failure to address the limiting constraint of poor sanitation and water supply. It is thus clear that policies aimed at the improvement of environmental hygiene will have to be accorded a much greater role than has been done in the past. However, as Scrimshaw (1983, p. 222) rightly observes, environmental hygiene will be of little help if personal hygiene does not improve at the same time. Perhaps education can play a role here in promoting cleanliness (which will help both environmental and personal hygiene) as well as generally raising the nutritional consciousness of the people.

A broader view of the food problem

Finally, we would like to draw attention to an issue which in a sense is a matter of semantics, but it can have an important impact on one's perception of the problem. The recognition that nutritional stress can arise as much from poor hygiene as from inadequate access to food often leads to a propensity to distinguish between food problem and hygienic problem as two distinct aspects of the general problem of nutrition. As a consequence of making this distinction, the emerging emphasis on hygiene is sometimes accompanied by a corresponding de-emphasis of the food problem in the context of malnutrition in the developing world. We find it a somewhat misleading way of looking at the problem. There is, we believe, a strong case for looking at the whole of the nutrition problem (specially that of protein-calorie malnutrition) as a problem of inadequate

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78. See the reflections of a leading architect of the INCAP project, Scrimshaw (1983).

food. After all, the very notion of a food problem can only be conceived as the inequality between intake and requirement of food. Anything that contributes to this inequality can be said to constitute a food problem. Poor hygiene aggravates the inequality by raising requirement (or by restricting intake), thus making the food intake inadequate and hence causing a food problem. Therefore, instead of distinguishing between food problem and hygienic problem as two separate factors impinging on the problem of nutrition, it would seem more logical to distinguish between access to food and access to hygiene as two entitlements both of which have a bearing on the common problem of food and nutrition. Accordingly, even when we recognise that poor hygiene is the more important limiting factor or that more resources should be committed to environmental policies than to the provision of food, that by itself should not lead us to take a diminished view of the magnitude of food problem.

However, it does require us to take a much broader view of the concept of food problem than we have done in the past. It forces us to recognise that the food problem is not merely a problem of low intake, but also of high requirement due to poor environmental quality. Even when the focus is on intake, we should no longer perceive it to be solely a matter of economic access to food; for, as we have seen, intake may be constrained by other factors such as anorexia as in the case of the children of Santa Maria Cauqué (Mata, 1978) or the constraint on mother's time as in the example of Pacey and Payne (1984). Accordingly, the solution of the food problem should be seen to lie not solely in enhancing the absolute entitlement to food, but in a much broader spectrum of policies including health and environmental measures as well as educational measures which may be necessary to enhance the effectiveness of other measures. Pointing to this need for broadening the concept of food problem (and its solution) is perhaps the singlemost important contribution in the field of policy made by recent advances in our understanding of human nutrition.

VI Summary and Conclusions

We set out to understand the logical and scientific basis of some current controversies in the assessment of nutritional status. The objective was to explore in what way, if at all, these controversies affect some of the concerns of economics, specifically in the assessment of poverty and capability and in the formulation of food and nutrition policy.

Three specific nutritional issues were covered in the process, viz. (i) whether the phenomenon of variable efficiency of energy utilisation warrants a downward revision of the requirement norm from its customary 'average' value; a great deal of current controversy over the assessment of poverty springs from this issue; (ii) whether the functional capabilities of a person remains invariant within a range of physical growth achieved in the childhood; this issue has relevance for both dietary assessment of poverty and anthropometric assessment of capability, (iii) the complications arising from the fact that undernutrition has both non-unique origin and non-unique outcome; both assessment of poverty and capability as well as formulation of policy ought to take note of these complications.

The principal conclusions emerging from our analysis can be summarised as follows:

(1) The claim that the norm for energy requirement should be set two standard deviations below the average cannot be

sustained. Sukhatme tried to justify this claim by using two quite different models of variable efficiency - one stochastic and the other adaptive. In the stochastic model, efficiency of energy utilisation varies in a spontaneous manner and intake varies pari passu, whereas in the adaptive model variation of efficiency is induced by prior variation in intake. The justification of the lower cut-off point in terms of the stochastic model is logically wrong because this model only ensures that to have one's intake above the point is a necessary condition for being wellnourished, but it does not ensure sufficiency. If on the other hand the adaptive model is accepted, then it would be logically correct to set the norm at the lower limit of adaptation rather than at the average value. But the limit of adaptation cannot be identified with Sukhatme's cut-off point which was derived from the stochastic model. This is a consequence of the fact that stochastic variation is not the same thing as adaptation, for the simple reason that spontaneous and induced variation in efficiency cannot be the same biological phenomenon. The limit of adaptation must be found from independent scientific evidence on 'pure' adaptation in efficiency. However, the existing scientific knowledge does not provide any evidence in support of pure adaptation of this kind.

(2) On the controversy over the possibility of costless adaptation in physical growth, we found it useful to distinguish between two interpretations of the 'small but healthy' hypothesis - one causative and the other associative. The causative interpretation was found difficult to refute in the light of existing evidence, which implies that the dietary assessment of nutritional status ought to recognise the possibility of intra-individual variation in requirement arising from physical adaptation among the children. However, one cannot be equally sanguine about the associative interpretation because physical

retardation may be associated with the loss of one kind of nutritional function (viz. cognitive skill) even within the range of causal invariance. This doubt raises serious problems for the anthropometric measurement of nutritional status.

(3) Even if all the debates on adaptation in energy efficiency and physical growth were to be satisfactorily resolved, both dietary and anthropometric approaches will fail to capture the set of undernourished people fully. The dietary approach will fail to capture those who have an intake that is high enough for an ideal environment, but not high enough to meet the higher requirement associated with the poor environment in which they live. The problem essentially is that undernutrition has multiple origin, in which both food and hygiene have a role to play, but the dietary approach focuses on food alone by assuming ideal hygiene. On the other hand, the trouble with the anthropometric approach is that undernutrition has also multiple outcome, and anthropometry picks up only one of them. Faced with a nutritional stress, a person may reduce either his bodysize or his level of activity. Concerned as it is with bodysize alone, anthropometry will fail to identify those who have responded to the stress by reducing activity.

(4) The review of nutritional issues (as summarised in the preceding three points) indicates that the traditional dietary approach to the measurement of undernutrition using average requirement as the cut-off norm has to contend with at least three difficulties: intra-individual variation in requirement arising from adaptation in physical growth, inter-individual variation in requirement due to genetic differences, and variation in requirement due to differing conditions of environmental hygiene. The common consequence of all three problems is a very likely misclassification of

the undernourished. Its implication for the measurement of poverty is that the set of poor will not in general be identical to the set of the undernourished. The question then arises: does the divergence between the poor and the undernourished render the use of 'average' norm an inappropriate method of measuring poverty? We have argued that the answer depends on how one perceives the conceptual foundations of poverty. If one takes the opulence view of poverty, then there is nothing in principle wrong in persisting with the traditional approach. However, if the capability view is accepted, the methodology will have to be revised quite radically.

(5) In the recent development of the capability approach to welfare, anthoropometry has come to play an important role. It is used as an indicator of capability on the assumption that there exists a monotonically increasing relationship between physical growth and nutritional capabilities. However, the possibility of trade-off between physical growth and physical activity invalidates this assumption. As a consequence, anthropometry by itself turns out to be an uncertain indicator of capability. Proper assessment of capability requires information on both anthropometry and activity of the populations concerned.

(6) Much of the policy debates arising from the recent nutrition controversy appears to be a classic case of much ado about nothing. Examples are the claim that the adaptationist view will encourage complacence on the policy front and the counterclaim that only the adaptationist perspective can ensure the most rational as well as equitable use of scarce resources. However, a couple of useful lessons come out quite clearly. First, supplementary feeding programmes should be targetted more specifically to the wasted children (and the severely stunted ones), leaving the problem of moderate stunting to be taken care of by

broader socio-economic policies. Secondly, policies aimed at the improvement of environmental and personal hygiene should be accorded a much greater role than has been done in the past.

(7) The stress on hygiene points to the need for taking a much broader view of food problem (and its solution) than has been done in the past. There was a time when the solution of the food problem was seen to lie in higher rates of aggregate production of food. Recently, the focus has rightly shifted from aggregate availability to individual entitlement to food. It would now appear that one ought to go one step further and recognise that entitlement to food is not all that there is to the problem of food and nutrition. Entitlement to hygiene is an equally important dimension of the food problem confronting the developing world. Both of these entitlements need to be addressed simultaneously, although the appropriate balance between the two will depend on the relative cost-effectiveness of alternative policy mixes. However, there is also a significant political economy dimension in the choice of strategy. In many societies, entitlement to food can be radically improved only by a drastic redistribution of productive assets which is bound to come up against resistance from powers-that-be. On the other hand, entitlement to hygiene can often be improved at least upto a point even within a given structure of ownership of assets and the balance of power that goes with it (vide the case of Sri Lanka). Thus the emphasis on hygiene is not only justified in its own right, it also makes a lot of tactical sense in the face of socio-political constraints which are holding down the quality of life in much of the Third World.

Appendix

Deducing Autocorrelation in the Calorie Intake of a Reference-type person

Some critics (e.g. Mehta, 1982; Rand and Scrimshaw, 1984) seem to believe that autocorrelation in calorie intake was deduced by Sukhatme by analogy with the protein model in which similar autocorrelation was earlier noticed by Sukhatme and Margen (1978). But this is not quite true. The autocorrelation in energy intake was actually deduced by a statistical analysis of intakes and expenditure of energy reported for a group of subjects by Edholm et al. (1970).[1] But then there is a problem here too. The variable for which autocorrelation was studied was neither intake nor expenditure, but the difference between the two, or 'energy balance' as it is called by the authors. And, as Mehta (1982) rightly notes, it is not immediately clear how autocorrelation in 'energy balance' implies autocorrelation in either efficiency or intake or expenditure.

Note that Sukhatme's objective was to show that efficiency varies from day to day and intake varies pari passu, even when a person is maintaining bodyweight and activity at the same levels. This he could not do simply by looking at actual variations in intake, because these may reflect the combined effects of three different factors:

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1. It is true however that the deduction was made only indirectly, as the nature of data prevented a direct test of autocorrelation by actually fitting an autoregressive model. It was this lacuna that made it necessary to draw analogy with the protein model for which an autoregressive model was actually fitted. However, the purpose of the analogy was to spell out the full implications of the autoregressive model, not to deduce the model in the first place.

variable efficiency, change in the amount of activity and change in the bodily stores of energy (i.e. change of bodyweight).[2] It was therefore necessary to eliminate the last two factors in order to isolate the effect of variable efficiency. The use of 'energy balance' was intended to do precisely that.

But exactly how the use of 'energy balance' data achieves this purpose has remained somewhat obscure. The difficulty arises from a pervasive confusion about the nature of 'expenditure' data reported by Edholm. The confusion is best illustrated by an apparent conundrum noted (but not resolved) by Mehta (1982). If 'expenditure' refers to actual total expenditure of energy in a day, then by the first law of thermodynamics the difference between intake and expenditure (i.e., 'energy balance') must be equal to the change in energy stores within the body i.e., to the change in bodyweight. However, Edholm's data show, and Sukhatme lays much emphasis on it, that cumulative 'energy balance' over a week does not equal the weekly change in bodyweight. The first law is thus seen to be violated. On the other hand, if 'expenditure' refers only to a part of total expenditure (for instance the part actually used by the body, excluding the part dissipated as heat), then of course the first law is no longer violated; but it then becomes impossible to sustain Sukhatme's proposition that average intake equals average expenditure in the long run,

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2. Although Edholm's subjects were engaged in broadly similar activities from day to day, their level of activity did not remain exactly constant. Similarly, they maintained bodyweight within a narrow range, but not at a fixed level. Thus all three factors mentioned here could account for the observed variation in intake.

for a part can never be equal to the whole.[3] Thus if Sukhatme's analysis is to make any sense, then the 'expenditure' in Edholm's data can not refer to either actual total expenditure or a part of it.

What then does it stand for? We suggest that it stands for the presumed total expenditure based on average efficiency. In other words, 'expenditure' was computed by multiplying the total amount of each kind of activity with a constant value of energy cost (per unit of each activity) based on average efficiency.[4] Once this interpretation is accepted, it becomes easy to see how the variation in'energy balance' reflects pure variation in efficiency.[5]

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3. See the discussion in the text on the implications of autocorrelation.

4. That 'total' expenditure rather than a part of it was measured is evident from Edholm et al. (1970): "The total energy expenditure was computed from midnight to midnight" (p. 1095), covering all kinds of activities including sleep. On the other hand, although it is nowhere mentioned explicitly that 'presumed' rather than actual expenditure was being measured, it seems to be implicit in the methodology of calculating expenditure. In fact, Garrow has noted this point explicitly while referring to Edholm's data in his comments on Beaton (1985): "The energy costs of each activity were assumed to be constant. The variability was in the amount of activity" (p. 232).

5. Surprisingly, Sukhatme himself never spells out this interpretation. On the contrary, he often gives the impression that he is talking about actual total expenditure. To get around the conflict with the first law of thermodynamics that such an interpretation involves, he suggests the view that in a biological system the law need not hold at every instant, but can operate with a lag. But it is very difficult to justify this view. There are of course some theoretical problems in interpreting the laws of thermodynamics in a disequilibrium system, as the biological system happens to be (Morowitz, 1978); but the notion of a lag does not emerge form all this.

Note first of all that Sukhatme measured both intake and 'expenditure' per unit of bodyweight so as to eliminate the effect of weight change over time.[6] Let this normalised 'energy balance' on any day t be expressed as $\beta_t = I_t - E_t$, where I_t and E_t refer respectively to intake and 'expenditure' per unit of bodyweight. Next note that since 'expenditure' is measured in terms of constant efficiency, daily variation in E_t is due <u>solely</u> to variation in the amount of activity; but the variation in I_t is due to variation in <u>both</u> activity and efficiency.

Let us now eliminate from I_t and E_t the common component (Δ_t) of daily variation which results from the deviation of actual activity in a day from the mean activity level. We are then left with (i)$I'_t = I_t - \Delta_t$, which can be defined as the presumed daily intake when activity is held at the mean level, and (ii) $\mu = E_t - \Delta_t$, which is the average expenditure when <u>both</u> efficiency and activity are at the mean level. Since bodyweight has already been held constant, μ can also be interpreted as the presumed <u>intake</u> corresponding to the average values of efficiency and activity. Thus, μ is the intake when neither efficiency nor activity varies and I'_t is the intake when only efficiency varies. Therefore, the difference ($I'_t - \mu$) reflects the variation in intake arising solely from variable efficiency. However, the difference ($I'_t - \mu$) is nothing but the 'energy balance' (β_t) as defined earlier, because

$$\begin{aligned} \beta_t &= I_t - E_t \\ &= (I_t - \Delta_t) - (E_t - \Delta_t) \\ &= I'_t - \mu \end{aligned} \qquad (1)$$

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6. "Since we are essentially concerned with the analysis of intake and expenditure in man <u>maintaining bodyweight</u> and engaged in defined tasks, both intake and expenditure are expressed per kilogram bodyweight basis" (Sukhatme, p. 26; emphasis added)".

Therefore, the observed variation in 'energy balance', as defined by Sukhatme, can be taken to reflect pure variation in efficiency.

The next steps in the argument consist in showing that (i) Edholm's 'energy balance' data reveal autocorrelation, which is interpreted, in view of the preceding argument, as revealing autocorrelation in the daily variation of efficiency, and (ii) autocorrelation in efficiency implies autocorrelation in the intake and expenditure of an unconstrained healthy person maintaining bodyweight and engaged in a fixed level of activity.

The first step in this argument, viz. the existence of autocorrelation in 'energy balance', was established through the following chain of reasoning:

(a) If the apparent 'balance' was due to measurement error alone, the cumulative 'balance' over a period of time would have been close to zero. But the cumulative weekly 'balances' are actually very large, which indicates that the variation in 'balance' reflects genuine variation in efficiency.

(b) If the variation of daily 'balance' were purely random, the variance of mean 'balance' would decline inversely as the length of the period over which mean is taken. However, the data show that the variance declines much more slowly than the inverse of the length of period. This indicates that the daily 'balances' are autocorrelated with each other.[7]

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7. For the mathematical details of this inference, see Srinivasan (1981), p. 7.

(c) When hypothetical variances of 'mean' balance were calculated by assuming a first-order autocorrelation coefficient of 0.3, these hypothetical values corresponded fairly closely to the values estimated from Edholm's data.

The evidences (b) and (c) thus together provide an indirect proof that daily 'energy balances' are related to each other through first-order autocorrelation. One can therefore postulate the model,

$$\beta_t = \rho \, \beta_{t-1} + u_t \qquad (2)$$

where ρ is the coefficient of autocorrelation and u_t is a random error with zero mean and constant variance.

Now since $\beta_t = I_t' - \mu$ (eqn. 1), and μ is a constant, it follows that I_t' is also serially correlated i.e.,

$$I_t' = \rho \, I_{t-1}' + e_t \qquad (3)$$

Recall that I_t' is the presumed energy intake when both bodyweight and activity (but not efficiency) are held constant. Therefore, it can be interpreted as the energy intake of a reference person maintaining bodyweight and engaged in a fixed level of activity (set at the average activity level). Also recall that the intake of a reference person is by definition equal to his requirement. It follows therefore that the intakes and requirements of healthy individuals maintaining bodyweight and engaged in fixed activity are not fixed but vary from day to day in an autocorrelated manner.

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