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SLIDING TOWARD NUTRITION MALPRACTICE: Time to Reconsider and Redeploy¹

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In the past several years the receptivity of the international development community toward pursuing the goal of improved nutrition has changed markedly. Today much of that community takes seriously the need for better nutrition. Nutrition initiatives are now widely understood not only as a consumption good that promotes human welfare but as an investment that directly influences productivity of the labor force and school force, and as a key factor in development. Major nutrition initiatives are now supported by development economists, who no longer have to be convinced about their usefulness. Increasingly, they acknowledge that something has to be done

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about malnutrition. No longer, for example, can they make structural adjustments in the economy without concern for their effects on nutrition.

This is particularly significant for the disadvantaged countries of the world, where the arithmetic of nutrition has barely changed in recent decades. Malnutrition is still staggering, and in some places, particularly sub-Saharan Africa, it is getting worse (2).

We of the international nutrition community, I am embarrassed to say, have made a disappointingly small dent in improving that condition. Many of us have spent decades working to solve nutrition problems in developing countries. In the past twenty years \$1.6 billion in US government-sponsored research alone has been directed to these problems.

Of course, progress in the nutrition status of some populations has been made, but most of the improvement has occurred not so much because of our nutrition community, but largely because of rising incomes in parts of Asia. True, some direct nutrition programs also have been successful in Iringa in Tanzania, Chile, Thailand, and Tamil Nadu in India. But in relatively few other actions has the nutrition community had a major role in bringing about improved nutrition of sizable populations. We have a great deal to show in scientific and technological advances, but very little to show in terms of combatting malnutrition in the world.

With the aid of vitamin A technology, for example, we now have the ability to prevent the deaths of as many as 2.5 million children each year (6). We are, of course, nowhere close to achieving that goal. Another micronutrient—iodine—provides an even starker case. Ever since Dr. V. Ramalingaswami's classic study of salt fortification in the Kangra Valley of North India more than a quarter of a century ago, we have known what needed to be done (5, 7). Yet today, still more than 200 million people have goiter, probably several times that number have subclinical iodine deficiency, and we now know that even mild and moderate iodine deficiency affects cognitive capacities.

From work that we had the opportunity to help initiate, we know that salt also can be fortified with iron to fight the one billion cases of iron-deficiency anemia in developing countries. Moreover, for some years, the idea of fortifying salt with a combination of iron and iodine, making possible a major reduction of two important public health problems with one shake, has been discussed. But where are the people trying to make it happen?

Is it not something of a scandal that we have done so little in applying our scientific knowledge? At the 1991 meeting of the United Nation's Sub-Committee on Nutrition, which encompasses the major UN and bilateral assistance agencies involved in nutrition, it was noted that our performance on micronutrients should be regarded as a collective embarrassment. We understand precisely the etiology and the consequences of, say, iodine deficiency;

we know who suffers from it and where they are; we know exactly what needs to be done, and we have in hand very low-cost technologies to do it. Still, there are upwards of 200 million iodine-deficient people in the developing countries. Such a performance can only be called *nutrition malpractice*.

The Perceived Obstacles

What is the reason for our failure to make larger dents in malnutrition? Asking more than thirty leading international nutritionists that question, I commonly hear three arguments. First, there are insufficient resources for large-scale operations. Second, the political commitment in developing countries themselves is inadequate. And third, not enough money is available to support nutrition research programs of academic institutions. I would contend, however, that the above do not explain why the nutrition community's contribution has been so limited.

For some time now, more money has been available to support nutrition operations than there are good projects to support. For example, in the three fiscal years ending June 1991, World Bank-assisted nutrition operations involved about \$1 billion in investments, slightly over half of that financed by the Bank and most of the rest by the benefitting countries themselves. Projections for the next three fiscal years are nearly double that. These figures do not reflect contributions to nutrition efforts often now included in structural adjustment operations. They are impossible to quantify, but their magnitude can be judged by the more than sevenfold increase—from \$102 million in 1989 to \$761 million in 1991—in Venezuela's targeted food and nutrition programs.

Expenditure on UNICEF nutrition programs has more than tripled in the past twenty years, from an annual average of \$9 million in the 1970s to \$24 million in 1987 and \$29 million in 1990. This grossly understates the real figures because of the major reorientation to nutrition in much of UNICEF's other work. Nutrition serves as an organizing principle for a substantial part of UNICEF's program in the 1990s. Interest in nutrition is also growing in several bilateral assistance programs. The German aid agency, for instance, now screens all relevant projects for their nutrition effects, and Canada is providing new multimillion dollar support for micronutrient programs. Other countries' resources for nutrition have also begun to increase as a result of the 1990 Children's Summit. There are exceptions but, overall, resources and attention for nutrition—even in an era of tight resources and compassion fatigue—are clearly on an upsurge. We in nutrition no longer are trying to walk up the down escalator.

The funding agencies are increasingly committed, but is there political commitment in the developing countries themselves? If we define political commitment to mean commitment to spend money and give speeches in favor

of feeding large numbers of people, the answer in many countries clearly is yes. True, what is sometimes promoted in the name of better nutrition makes little real contribution to improved nutrition: untargeted food subsidies in many countries are the best example of this. But even if we limit our definition to those governments with a genuine interest in the malnutrition problem, and who seek to undertake actions for reasons beyond pure political popularity, there is a good deal of commitment. A number of governments want to start large new programs.

Of course, the nature and degree of political commitment to overcome malnutrition varies with forms of governance and the other problems facing a country at any given time. Clearly, political will is inadequate in some places. But those international agency staff who deal regularly with planning ministries and finance ministries not uncommonly are surprised by how much interest in nutrition exists in a developing country. Often they find that a country's needs are not so much for more political commitment as for good program design and management, including a better understanding of how to get the fruits of existing technologies into the hands and stomachs of those who need them.

A different type of perceived constraint is the lack of outside funding for nutrition research. "University starving," sometimes suggesting almost a divine right to receive research funds from public institutions, is a recurrent theme among academic nutritionists. Although universities clearly are in need of more money, arguably the level of funding is not the only issue. In the 1970s, the US Agency for International Development (AID) offered generous funds under its so-called 211(d) grant program, which sought to strengthen university capacity to contribute to international development efforts. In nutrition, one after another of those grants led to disappointment. In several cases, universities used the money to do things they were going to do anyway, rather than adding the policy and programmatic dimensions to nutrition as they had agreed. How would we grade those results? With some exceptions, the record is poor for the academic community as a whole, especially in applied work.

The fault does not lie with the academic community alone. Our operational nutrition community is also responsible for much of the malpractice: for opportunities lost, efforts misdirected, local needs and preferences ignored. And even more so is the failure of operational and academic communities to learn to work together.

Assuming, then, that at least in some countries the obstacle is not a lack of operational resources or political commitment and is not simply due to inadequate funding available for universities, why then has the nutrition community not made more of a dent in malnutrition? Could the answer lie in how we go about trying to solve the problem?

The Research Chain

We confront two main problems. We have been emphasizing the wrong research issues and we have been negligent in preparing people to work operationally in nutrition. A chain of questions must be addressed to bring about large-scale improvements in nutrition. Those questions begin, on the malnutrition side, with *why* and move through *who* and *where*, *what*, and *how* to the nutrition-improvement side of the chain.

Consider the example of vitamin A deficiency. Under the *why* category falls research into why a deficiency occurs and why bother with it: its biomedical pathways, its socioeconomic determinants, and its consequences. Next is work to determine *who* is vulnerable to vitamin A deficiency and *where* they are. The *what* category involves research on the size and frequency of dosage to prevent vitamin A deficiency and the mechanisms to deliver it. The *how* category deals with the organization and management of vitamin A delivery, how the families of the intended recipients perceive the problem and the proposed intervention effort, how a delivery program can be responsive to families' perceptions, the effects of public information efforts, program evaluations, and so on.

What is the relative importance of each of these research categories? Work in connection with the *World Food and Nutrition Study*, which was undertaken in the mid-1970s for the National Academy of Sciences, found that of funds obligated by US government agencies for international nutrition research, some 67% was directed to the *why* question, about 20% to *who and where* (mostly survey work), about 11% to the question of what to do about it, and less than 2% to *how* to do it (4). Although it is impossible to specify that distribution today, budgets of several involved agencies suggest hardly any change over the years, even though the state of nutrition knowledge has changed dramatically.

We know enormously more now than we did one or two decades ago about the causes and consequences of malnutrition. We know so much about who and where the vulnerable groups are that it is reasonable to question whether we are getting adequate return on investment in more nutrition status surveys, at least as they have traditionally been undertaken—sometimes measuring simply for the sake of measuring. We also know what to do about nutrition problems in many circumstances. A number of techniques and technologies with the potential for sizable impact have been developed and are part of today's nutrition arsenal.

Yet almost no one seems to be trying to address the key question of how to reach the payoff. Although research needs have changed markedly, we continue to do what we know how to do. Remarkably little intellectual attention has been given to the *how* end of the chain. A chasm separates all

we have learned through basic research from the actions needed to cause something basic to happen. So much knowledge build up. So little benefit.

Unless we give a lot more attention to *how* and somewhat more to *what to do* (the latter particularly to address the rural problem in the Sahel and other impoverished rural areas of sub-Saharan Africa), the value of other research is close to nil. In an economic sense, the return to nutrition research, say, in terms of lives saved, for work now on the *why* and *who* side of the chain is low, whereas the potential benefits at the opposite end of the chain are very high. With warehouses filled with potentially useful research papers on *why*, more research value would seem to lie in determining *how*, for example, a particular fortified food could be pushed through knots in the distribution system, how it is perceived by the people who are supposed to eat it, how it is allocated within a family, and what it will take to make a program effective. Why are we not concentrating on this kind of research?

Training for Operations

The second major problem in international nutrition is the lack of appropriately trained and experienced people to design and manage large-scale nutrition policies, programs, and projects. Today there is more money available for nutrition than there are good projects. In the World Bank, if twenty more solid proposals that met specificity and other standards were to emerge, I am confident that at least eighteen would be financed, and I hear the same from other agencies. The climate is favorable. But who has the capacity to prepare those proposals? And, once projects are financed, who has the capacity to manage them?

For years, nutritionists have challenged the broader development community to take nutrition seriously. Now it has, and we are not able to provide the people to meet the demand. The World Bank over the last year has added half a dozen new nutrition staff members and this number is likely to grow. Much of the work until now has been done by persons with no nutrition background. They know planning and management, but not much about nutrition. We need people who know both. Currently, UNICEF is recruiting some forty new nutrition-oriented staff members. The Inter-American Development Bank is recruiting in nutrition for the first time. Both within the Bank and outside, demand exists for people trained or experienced for operational jobs or consultancies in nutrition. But identifying qualified people to do this work is difficult. We can find people to do one more survey or once again formulate a more nutritious weaning food. But what if we want better understanding of the nuts and bolts of a program and its cultural setting so that we can figure out how to make it work better? The population field has such people. The population curriculum at Johns Hopkins University provides

courses in Policies and Programs, Family Planning Administration, Evaluation, Family Planning Communication, and Economics of Population and Its Planning. Why are Johns Hopkins as well as the Universities of Michigan and North Carolina preparing students to do practical, hands-on work in population programs but not in nutrition?

Most graduate students in nutrition are equipped to teach and do fairly narrow research. Rarely do doctoral dissertations focus on broad-based applied research: In the early 1970s, only 5% bore titles in international applied nutrition, and the percentage had fallen close to zero by 1990. Among the dozen-or-so programs at leading US universities that fifteen years ago were training students for international nutrition, including some applied work, two of the strongest programs have either disappeared (Massachusetts Institute of Technology, MIT) or virtually disappeared (Harvard) from the list. In seven other universities, the program size has withered, in several cases substantially. Only two universities have increased their commitment in this area.

Reports I have received indicate that enrollment in international nutrition is down, enrollment in nutrition generally is down, and the number of applicants for nutrition programs is down even more. Cornell in 1990 had 140 applications for nutrition training, compared to annual requests a decade ago that numbered in the 300s. Tufts accepted two out of five applicants in 1985; it accepted two out of three in 1990. Even though the level of student enrollment levels in some universities is being maintained, the pool from which to select those students is shrinking.

It is not inconceivable that one explanation for the decline in student interest is the relevance of the programs. Not too many years ago, when I was a member of a small panel reviewing a prestigious Ivy League university's nutrition department, I was struck by the lack of fit between the interests and expectations of the students I encountered and the interests and the work of the faculty. Although the descriptions in the course catalog appeared relevant, the teaching commonly reflected narrow interests of individual professors. A number of the foreign nutrition students we interviewed suggested that the university should be sued for false advertising.

Here is a laudable statement of goals from the current course bulletin of a major East Coast university: "The major objective . . . is the development of logical approaches for the prevention of malnutrition in less developed countries . . . , faculty from a variety of disciplinary backgrounds . . . work collaboratively on divisional teaching, research, and service activities." But the catalog lists only one obviously applied nutrition course offered by the department each year. The course catalogs of six other prominent universities reveal similar situations.

Of course some universities give more attention to applied concerns, and within universities some faculty are more sensitive to these needs. But, overall,

the problem of relevance is real. By and large, institutions of higher learning, both in industrialized and developing countries, have not equipped their students for the broad role of designing and managing nutrition efforts. How different might be the state of nutrition in the world had they done so.

The Constraints to Applied Research

What is the reason for this unfortunate situation of misdirected research and a lack of appropriately trained people? There probably are several explanations. The academic nutrition community has not addressed the important question of how to implement the benefits of nutrition research either in its research programs or in its training. This is partly because the reward system of academia leads in other directions.

The lack of interest in applied research partly stems from the way research is defined. The director of one of the largest university nutrition programs, who reflects the view of many, described research as a process that produces knowledge that is universally applicable. This rules out sizable portions of the needs in international nutrition. The knowledge produced by answers to the *how* question often is culture- or site-specific: it applies to one program at one place at one time. (This is not to say, of course, that applied research cannot sometimes produce approaches or principles with broad applicability.)

It is also often hard to measure things precisely when dealing with the *how* question, particularly since a good part of the nutrition problem is poverty, alienation, disorientation, and inability to cope. Because the search for answers moves into the economic, political, social, and administrative realms, writing a good academic paper is difficult. The pressure in the academic world to publish has become a widespread infectious disease that already has spread to academia in developing countries, so that applied work there too carries less weight.

The Nutrition Market

Research is surely driven by research grants. For what kind of research is money available these days? Although total resources for nutrition research have not fallen, the largest portion in the US today focuses on the domestic market and the nutrition needs of well-off consumer. Unlike 20 years ago, much more money is spent on studying ways to reduce excess fat in the diets of affluent adult males than on ways to add fat to the diets of children who do not have enough to supply the energy they need. Furthermore, the percentage of articles in the *American Journal of Clinical Nutrition* that are devoted to problems in developing countries is less than half of what it was in the 1970s. That is all the more reason why most of those financial resources that still are available to solve international nutrition problems need to be dedicated to answering the questions of *what* and *how*.

It is no accident that some international nutritionists, instead of shifting from basic to applied research, are moving into domestic research, in response to where the money is. Certainly, work on the relationship of nutrition to cancer and heart disease is important, but research needs in international applied nutrition are being neglected because domestic concerns are so much better funded.

We in operating agencies probably are being unfair or unrealistic in our expectations. Because of the very considerable contributions that those in the universities have made to nutrition, particularly in the early years, we look to them for leadership. It was the academic nutrition community, after all, that generated the initial interest that brought the problem of malnutrition to world attention, and over the years it has tried to better understand the nature of the problem and its consequences. Its members developed and organized the nutrition institutes around the world and prompted the formation of the Protein Advisory Group, which evolved into the United Nations Sub-Committee on Nutrition. They have made important contributions in a hundred other ways. So it is natural that we turn to them.

With their main orientation toward biomedical research, though, nutrition policies and programs are seen as appendages of research, rather than the reverse. Nor do they generally have the disposition or inclination to deal with constraints that are addressed by policy- and project-oriented research. It would not be fair—and clearly not realistic—to expect all those working on nutrition in universities suddenly to address *how* problems. But, at least, the climate can be made more hospitable to that small band of academics who want to address applied problems, and to efforts made to expand that band.

Nor is neglect of applied research limited to the academic community. Those of us working in policy and operations also seldom make a systematic effort to see how and what difference a policy or a technique or a product makes. Practitioners generally give remarkably little attention to project evaluations, for instance. One recent analysis of 104 mostly government child-feeding programs in Latin America found that only 10 included any kind of evaluation, and of these evaluations only three were judged to be adequate (3). One reason for the lack of objective standards is humanitarian. Many people believe that they are doing God's work and that it cannot fail to pay off; common sense tells you that if hungry children are given food, they will be better off. The second reason is bureaucratic. A manager who is paid to deliver food, and whose performance is judged by how much food he distributes, is going to concentrate on moving the food.

The two obstacles are very different but have an identical impact on whether things get done right. The harassed missionary trying to feed hungry children is not going to choose to spend a peso of his limited funds on evaluation. And

the harassed bureaucrat, who never has enough money and is trying to hold his staff together on low pay, runs the risk of an evaluation's discovery that what was assumed to be a reasonable effort is, in fact, not so—an outcome that is more likely to make him look bad than good.

There are people who defend food coupons to the death and people who think they are the work of the devil; but after coupons are distributed, hardly anyone ever checks on what happens. The same is true of milk distribution and flour distribution. We hear over and over again that if a family is given a kilo of food and told that it is for the two-year-old child, the two-year-old may not get more than 200 or 300 grams of it. But nobody knows what determines whether the child gets 50 or 100 or 500 grams. So we simply make assumptions about dilution and plan rations on that basis. But we do not have a clue whether such assumptions are right. Yet whether the child actually gets half of the ration or a tenth may make the difference between life and death.

But then consider why the Iringa (Tanzania) and Tamil Nadu (India) projects have been successful in markedly reducing malnutrition: operations research, including evaluations, was conducted at each step. Over the first six years in Tamil Nadu, the government undertook or sponsored through local universities and research groups 37 discrete pieces of applied research. Many findings led to changes. For example, studies examined the causes of relapse into malnutrition of previously recovered children in an attempt to determine if there were predictable patterns. Other studies sought to learn why children were absent from growth-monitoring sessions. A study of the effectiveness of various growth cards led to use of the bubble chart (1). And a study of how long it took worm loads to reach debilitating levels in children had important cost-benefit implications, since the cost of deworming twice a year was half that for deworming four times a year. At the outset of the project the Tamil Nadu government set up a separate fund for the studies, and 2.1% of this \$81 million nutrition project was earmarked for monitoring and evaluations.

Clearly, a marriage of research and operations is required. Understanding nutrition behavior in a given setting, through a systematic search, is important knowledge. And if such a search is not academically acceptable, then academic norms of acceptability need reexamination. According to E. J. R. Heyward, for many years Deputy Executive Director of UNICEF, "the results of the nutrition profession are disappointing because, although the science has made good progress, the context for its application has been neglected. Improving the state of nutrition means changing peoples' behavior. Who studies their perceptions and their constraints—not in the abstract but in the context of launching a new nutrition program?"

Often, what is most needed are pilot and demonstration activities, with strong evaluation components; quasi-experimental programs; behavioral re-

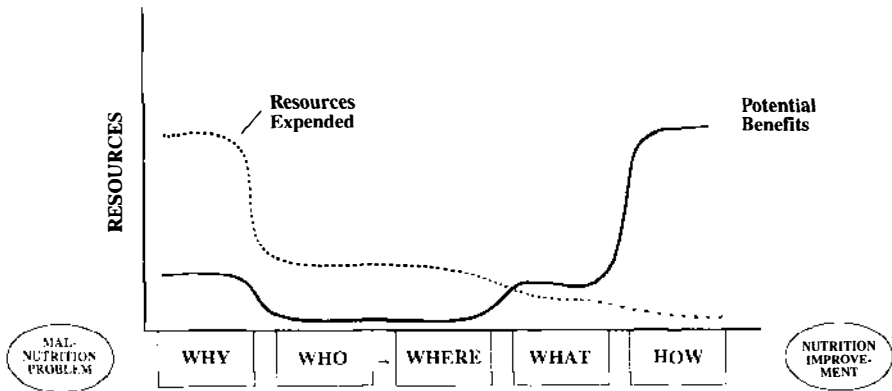


Figure 1 Relationship between potential benefits and resources expended on nutrition research in the past 25 years.

search studies of the social marketing type, including not just the behavior of clients of services, but the behavior of the deliverers of those services as well; and participatory research that helps communities identify their own solutions. Whatever we call it, we need to have a better understanding of how things work and the reasons they do not work.

I do not mean to suggest that all the necessary research answers on the *why* and *who* side of the nutrition chain are in hand, but the research agenda is out of balance and its results skewed so that the potential benefits do not justify the current expenditure patterns.

Figure 1 illustrates how the relationship has changed between potential benefits and resources expended on research questions over the past 25 years. We in the nutrition community will ultimately be measured by our return on investment. It is time for some serious stock-taking.

The Prescription

In the area of training, universities should be asking what kinds of policies and programs are necessary to alleviate malnutrition and what knowledge and skills are necessary to make that happen. Have current training programs equipped people to build careers in nutrition service? What will it take to

provide nutritionists with the skills needed to design and manage and evaluate nutrition programs? Is the faculty capable, through its experience, of teaching such practical skills? Hard decisions need to be made on how to use new university staff resources, and even harder decisions on how to reallocate existing resources. What I am proposing is not just a stock taking but a redeployment.

What we need are students trained in economics, administration, logistics, planning and budgeting, and the dozen other necessary skills in addition to nutrition science. We need people who are at home in both the scientific and bureaucratic worlds, people who can do nuts-and-bolts work in getting projects going in new sets of circumstances month after month. In short, what we need are *nutrition engineers*. Webster defines an engineer as "a person who carries through an enterprise and brings about a result." Unlike many other fields, nutrition does not have the equivalent of engineers—it has the equivalent of physicists, but not of engineers. We need engineers badly. We need to stop inventing and reinventing wheels, and to start putting wheels on the wagons we have.

Nutrition programs designed without an understanding of the relative importance of real income, belief patterns, and infection in the causation of malnutrition, or of the more structural, less proximate determinants of malnutrition, are likely to be poorly designed programs. Those responsible for the design of nutrition programs would be as negligent in ignoring such factors as would a structural engineer in ignoring such factors as wind velocity and ground swells.

How could academic programs be changed to produce nutrition engineers? Several avenues have been suggested, from revamping university nutrition departments by encouraging financial support for innovative programs committed to *how* concepts, to encouraging the development of a nutrition stream in management and public policy schools.

A preferred option would be to resuscitate the concept of an integrated program, such as that tried by MIT 20 years ago. Nutrition normally cuts across university departments, including food science, biochemistry, political science, agriculture, and medicine. The MIT program tried to bring cohesion to this work by looking at the totality of the problem. It used case studies in the style of Harvard's Business School. It addressed implementation and management issues. It followed projects from beginning to end. It emphasized evaluation.

For a while the MIT program encompassed both doctorates in nutrition planning and short courses for mid-career practitioners. Former students of that program are now in important nutrition positions around the world—in UNICEF, the International Food Policy Research Institute, UNESCO, the World Bank—as well as working as principal nutritionists for a number of

governments. For a while it did work, but because it was viewed as partly nonacademic, the program was at odds with the university culture and reward system, and eventually faded away.

Maybe it was an idea before its time. If so, maybe now is the time to recapture it and develop institutes dedicated to applied work and the training of nutrition engineers. Not that the MIT program should be copied exactly, but the concept and experience could be used to build something new and appropriate for today's needs. For a foundation looking for an entry point into international nutrition, a center or institute that addresses this neglected area of work, critical for much else that has been done to have a substantial impact, would be an attractive option for funding.

Any university that wished to pursue this direction would need to follow several basic tenets: (i) Place greater emphasis on applied subjects for research and training, including nondegree as well as degree programs, and teach the techniques of location-specific empirical analysis, illustrated with examples of real research. (ii) Incorporate research and training and the analysis and evaluation of operations in a feedback loop. (iii) Stimulate more interaction with local nutrition managers and local institutions, and between universities and programs in the field. Some things cannot be learned in the classroom, so training must take place in the field.

Whatever is done, the academic nutrition community will have to question some basic values if it is to contribute more. Perhaps the number of articles contributed to professional journals is not so important after all. Perhaps we need to forget about the third and fourth decimal points on old issues and examine the whole numbers on new issues that matter. Perhaps requirements for faculty positions have to be reconsidered. To train nutrition engineers will mean attracting faculty members who themselves are program designers and managers and who have spent much of their careers in developing countries, people who often do not have conventional academic qualifications. I do not suggest that standards be compromised, but that their order of priority be changed. For these new programs the primary standard should be how much difference the program will make in overcoming malnutrition.

New Institutional Setting Needed

In the area of research, the academic culture is less likely to change. Even with programs that emphasize applied training, universities are unlikely to address many practical needs. Operating agencies should not try to turn universities into instruments to satisfy their own nutrition research requirements, for universities legitimately have a different role and objectives. So instead of converting universities into something they are not, perhaps the nutrition community should give attention to creating another kind of institution.

The experience of the US Agency for International Development in population programs may be instructive. In the mid-1970s, it gave universities considerable sums to study population growth, but those studies were leading to little change. Then AID changed course and made more resources available to mount pilot and demonstration activities in family planning (including evaluation components) on a scale large enough to be illustrative if scaled up to a national level. To get what it wanted, AID entered into agreements with the Population Council and other private contractors. The payoff was substantial; governments in Bangladesh, Egypt, Kenya, Taiwan, and Thailand, for example, looked at the results obtained in those small quasi-government settings and used those programs as the basis for what are now national programs.

The Population Council is a kind of halfway house between university and consulting firm: it is problem driven, unlike the university, and unlike the consulting firm which is essentially client-driven. The Council exists specifically to find ways to solve a problem, in this case the population problem. And it is flexible enough and has an agenda broad enough to permit attempts at understanding many dimensions of the problem. In addition to an operational interest, it has a research interest (mostly operations research). It is very much involved in the field testing of ideas. Endowed by foundations with an independent international board and its own funds, it can set its own agenda.

Improving the capacity *within* developing countries to produce nutrition engineers and to develop demonstration projects should be the primary objective of, respectively, a university-based institute and a nutrition equivalent of the Population Council. A major goal of all that is done in the international nutrition effort should be to strengthen those who will be the mainstay in running nutrition programs and to develop or expand the capacity of local research and training institutions to incorporate a policy and programmatic orientation.

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What the nutrition community has learned in a few places like Iringa and Tamil Nadu is opening up some large-scale operational opportunities. International funding agencies are spending increased amounts of money: in some cases on activities where we know we are on the right track; in other cases where perhaps we are doing it right but inefficiently for lack of better *how* answers and better-equipped people; and, finally in those cases where we probably are making mistakes but should at least be learning in the process.

What is needed now is the leadership and institutional underpinnings required to move in new directions. A first step toward achieving this goal will be to corral intellectual energies and try to recapture the excitement of earlier years—to convince the nutrition community that applied work on the

how end of the chain is not only beneficial but also intellectually satisfying. What could be more satisfying than addressing the most important constraints that prevent us from making a serious dent in malnutrition?

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