



Sliding toward nutrition malpractice: time to reconsider and redeploy¹⁻³

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Today much of the international development community takes seriously the need for better nutrition. A few years ago that was not the case. Nutrition is now widely understood not only as a welfare good, but as an investment that directly influences productivity of the labor force and the school force and as a key factor in development. Increasingly, development economists acknowledge that something has to be done about malnutrition. Further, seldom is structural adjustment any longer instituted without prior concern for the effects on nutrition.

This is particularly significant for the disadvantaged countries of the world, where the nutrition arithmetic has barely changed in recent decades. Malnutrition is still staggering, and in some places, particularly sub-Saharan Africa, getting worse (1).

We of the international nutrition community, I am embarrassed to say, have made a disappointingly small dent in this problem, even though in the past 20 years something on the order of \$1.6 billion in US government-sponsored research alone has been directed to malnutrition. We do have a great deal to show in scientific and technological advances, but very little to show in terms of marked declines in malnutrition in the world. To the extent there has been improvement, it has occurred not so much because of our decades of work but largely because of rising incomes in parts of Asia. True, some direct nutrition programs have been successful in Chile, Tanzania (Iringa), Thailand, and India (Tamil Nadu), but how many other actions can we point to that have brought about improved nutrition to sizable populations?

Yet we have in hand, for instance, vitamin A technology to prevent the deaths of as many as 2.5 million children each year (2), though we are nowhere close to achieving this goal. Another micronutrient—iodine—provides an even starker case. Ever since Ramalingaswami's (3, 4) classic study of salt fortification in North India in the 1960s, we have known what has to be done. We understand the etiology and consequences of iodine deficiency; we know who suffers from it and where they are and we have available very-low-cost technologies to correct it. Still, more than 200 million people have goiter and probably several times that number have subclinical iodine deficiency, and we now know that even mild and moderate deficiency affects cognitive capacities. Such performance can only be called nutrition malpractice.

Salt can also 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 dis-

cussed. 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?

The perceived obstacles

What is the reason for our failure to make larger dents in malnutrition? Asking more than 30 leading international nutritionists that question, I commonly heard three arguments: first, insufficient operational resources to deal with problems on a large scale; second, inadequate political commitment in developing countries themselves; and third, not enough money available to support the work of academic institutions with nutrition programs. My contention is that these often are not the real problems at all.

For some time now, there has been more money available for nutrition projects than there have been good projects to support. For example, World Bank-assisted nutrition operations in the three fiscal years ending June 1991 involved about \$1 billion in investments, slightly over half 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 included in adjustment operations. They are impossible to quantify, but their magnitude is increasing. For instance, Venezuela's targeted food and nutrition programs, in conjunction with structural adjustment, have risen more than sevenfold—from \$102 million in 1989 to \$761 million in 1991.)

Expenditure on UNICEF nutrition programs has more than tripled in the past 20 years, from an annual average of \$9 million in the 1970s to \$24 million in 1987 and \$29 million in 1990. These amounts grossly understate the real figures because of the major reorientation to nutrition in much of UNICEF's other work in the 1990s. The German aid agency now screens all relevant projects for their nutrition effects, and Canada is pro-

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viding 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.

The funding agencies are increasingly committed, but is there political commitment in the developing countries themselves? If spending money and giving speeches in favor of feeding large numbers of people is commitment, 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 consumer food subsidies in many countries are the best example.

Of course, the nature and degree of political commitment to overcoming malnutrition varies with forms and philosophies of governance, and we who work on malnutrition need always be cognizant of its political and economic context. But international agency staff who deal regularly with planning ministries and finance ministries not uncommonly are surprised by the degree of interest in nutrition in a developing country. Often they find its needs are not so much for more political commitment as for good program design and management, along with better understanding of how to get the fruits of existing technologies into the hands and stomachs of those who need them.

A different sort of perceived constraint is the lack of outside funding for nutrition research. "University starving," sometimes suggesting almost a divine right to research funds from public institutions, is a recurrent theme among academic nutritionists. Arguably, lack 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 aimed at strengthening 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? There are exceptions but, for the academic community as a whole, the record is poor, especially in applied work.

The fault does not lie with the academic community alone. Operational nutritionists are also responsible for opportunities lost, efforts misdirected, and local needs and preferences ignored. And more so than that is the failure of operational and academic communities to learn to work together.

Assuming then that for some countries the obstacle generally is not lack of operational resources, political commitment, or inadequate funding for universities, why then not more of a dent? Could the answer lie in how we go about trying to solve malnutrition?

The research chain

Basically, two main problems pertain: emphasizing the wrong research issues and negligence in preparing people to work operationally in nutrition. A chain of questions must be addressed to bring about large-scale improvements in nutrition. These questions begin, on the malnutrition-problem side, with *why?* and move through *who and where?* *what?* and *how?* to the nutrition-improvement side of the chain.

Why research covers *why* a deficiency occurs, its biomedical pathways, its socioeconomic determinants, and its consequences.

Next we need to determine *who* is vulnerable and *where* they are. *What* involves the size and frequency of the dose or the change of diet or other change necessary to prevent the deficiency and the mechanisms to deliver it. *How* research gets into *how* families of intended recipients perceive the problem and the proposed intervention effort and *how* a delivery program can be responsive to families' perceptions, program evaluations, the organization and management questions, and economic and sometimes political issues.

What is the relative importance of each? Work in connection with the *World Food and Nutrition Study*, undertaken in the mid-1970s for the National Academy of Sciences, found that in 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 *what* to do about it, and less than 2% to *how* to do it (5). Although it is impossible to pin down 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 about the causes and consequences of malnutrition, and about who and where the vulnerable groups are. It is reasonable to question whether we are getting adequate return on investment in more nutrition-status surveys—that often measure for the sake of it. We also know what to do in many circumstances, because of numerous techniques (including economic techniques) and technologies in today's nutrition arsenal. Yet, almost no one seems to be trying to unlock the question of *how* to reach the payoff. Research needs have changed but we continue to do what we know how to do. Remarkably little intellectual attention has been given to getting to the end of the chain. There is a chasm between all we have learned through basic research and 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 for rural Sahel and other impoverished 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 and the potential benefits to *what* and especially *how* are very high. With warehouses filled with potentially useful research papers on *why*, it would seem that now feeding a fortified cereal to rats one more time is not likely to tell us a lot that is new. Meanwhile, more research value would seem to lie in checking out how a particular fortified food could be pushed through knots in the distribution system, how it is perceived by 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

International nutrition seems to lack people trained and experienced to design and manage large-scale policies, programs, and projects. In the World Bank, if 20 more solid country proposals that met specificity and other standards were to emerge, I am confident that at least 18 would be financed, and I hear the same thing from other agencies. The climate for good proposals is favorable. But who has the capacity to prepare those

proposals? And, once financed, who has the capacity to manage them?

Both within the World Bank and outside, there is a demand for people trained or experienced for operational jobs or consultancies in nutrition. Over the last year, the World Bank has added half a dozen nutrition staff and this number is likely to grow. UNICEF is recruiting some 40 nutrition-oriented staff. The Inter-American Development Bank is recruiting in nutrition for the first time. Identifying such qualified people is difficult. 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. We can find people to do one more survey or to take one more crack at formulating a more nutritious weaning food, but what if we want better understanding of a program's nuts and bolts and its cultural setting so that we can figure out how to make it work better? The population field has such people. Johns Hopkins University and the Universities of Michigan and North Carolina, for example, are preparing people to do practical work in population programs. Why not similar programs for 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. In the dozen-or-so leading universities that 15 years ago were training students for international nutrition—including some applied work—most programs have withered, two of the strongest have disappeared or virtually disappeared [the Massachusetts Institute of Technology's (MIT's) and Harvard's], and only two have increased their commitment.

A conceivable explanation for the decline in enrollment in international nutrition programs is that students perceive little relevance in courses offered by nutrition departments. 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 the state of nutrition might be in the world had they done so.

Constraints to applied research

What are the reasons for this unfortunate situation—of the imbalanced research emphasis and the lack of appropriately trained people? There probably are several. The academic nutrition community has not addressed the important question of how to implement the benefits of nutrition research, either in its research program or in its training, partly because the reward system of academic culture leads in other directions. The lack of interest in applied research partly stems from the way research is defined, commonly 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 are often culture, or site-specific, applying 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 because a good part of the

nutrition problem is poverty, alienation, disorientation, and inability to cope. Because the search moves into the economic, political, social, and administrative realms, writing a paper acceptable to professional journals is difficult. The pressure to publish has already 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—by what money is available for what. Where is the money these days? Although total resources for nutrition research have not fallen, the largest portion in the United States focuses on domestic research and affluent populations. Unlike 20 years ago, much more money is now spent on studying ways to reduce fat in the diets of affluent adult males than on getting more fat into the diets of children who do not have enough in the first place. Another indicator is that the percentage of articles devoted to problems in developing countries in *The American Journal of Clinical Nutrition* is less than half the percentage in the 1970s. All the more reason to push those declining resources that are available for international nutrition toward the *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 research money is. Certainly work on the relationship of nutrition to cancer and heart disease, for example, is important, but research needs in international applied nutrition are being held hostage because domestic concerns are so much better funded.

Are we in operating agencies being unfair or unrealistic in our expectations? 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 been trying 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 for leadership.

With their main orientation toward biomedical research, though, policies and programs are seen as appendages of research rather than the reverse. Nor do many of them have the professional disposition or inclination to deal with constraints addressed by policy- and project-oriented research. To be sure, 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 expanding that band.

Nor is neglect of applied research limited to the academic community. Those 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 little attention to project evaluations, for instance. Philip Musgrove (6) recently analyzed 104 mostly government, child-feeding programs in Latin America and found only 10 with any kind of evaluation and only three evaluated halfway decently. One reason, he reports, is humanitarian. Many people view what they are doing as 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 on how much food he distributes, is going to concentrate on moving the food.

The two obstacles are different but have 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 finding that what was assumed to be a reasonable effort is not so, which 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 given out, hardly anyone checks on what happens. The same is true of milk and bags of flour. If a family is given a kilo of food and told that it is for their 2-year-old child, the child may not get more than 200 or 300 g of it. But nobody knows what determines whether the child gets 50, 100, or 500 g. 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 the ration or one-tenth may make the difference between life and death.

But then consider why the projects of Iringa and Tamil Nadu have been successful in reducing malnutrition so markedly: because operations research, including evaluations, was conducted at each step. At the outset, 2.1% of Tamil Nadu's \$81 million project costs were earmarked for monitoring and evaluations. Over the first 6 years, 37 discrete pieces of applied research were conducted, and findings led to many changes. A study of the effectiveness of various growth cards led to the use of the bubble chart (7). A study of how long it took worm loads to reach debilitating levels in children had important cost-benefit implications, because the cost of deworming twice a year was half that for four times a year.

Clearly, there needs to be a marriage of research and operations. Understanding nutrition behavior in a given setting is important knowledge. What often is most needed are pilot and demonstration activities, with strong evaluation components; quasiexperimental programs; behavioral research studies of the social marketing type, including the behavior of both clients and deliverers of those services; 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.

None of this is to suggest that all the necessary research answers on the *why* and *who* side of the nutrition chain are in hand (although it is tempting to seek a cessation on new basic research efforts until we know better how to apply what we already know). But the research agenda is out of balance, its results skewed so that the potential benefits do not justify the current expenditure patterns. It is not wrong to insist that we in the nutrition community will ultimately be measured by our return on investment. It is time for some serious stock-taking.

A 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. Has their training equipped people to build careers

of performing service in this area? What will it take to provide nutritionists with the skills needed to design and manage and evaluate such programs? Is the faculty capable of guiding students on the myriad of social and economic structure and policy issues related to, say, consumer food subsidies, or even on implementation measures for growth-monitoring and growth-promotion programs as opposed to advising on basic science? 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, sociopolitical analysis, and the dozen other necessary skills in addition to nutrition. 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 to stop doing physics, stop inventing and reinventing wheels, and start putting wheels on the wagons we have.

The use of the term nutrition engineer should not suggest that malnutrition is largely a technical problem rather than a complex of social, economic, political, and sometimes technical problems. Nutrition programs designed without understanding of, for example, the relative importance of real income, belief patterns, and infection in malnutrition causality, or of the more structural, less proximate determinants of malnutrition, are likely to be poorly designed programs. The people responsible for the design of nutrition programs would be as negligent in ignoring such factors as would civil engineers be 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 re-vamping university nutrition departments by encouraging financial support for innovative programs committed to *how* issues, 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 should cut across university departments, including food science, biochemistry, political science, economics, and agriculture. The MIT program tried to bring cohesion to this work, providing a focal point for nutrition. It had case studies in the style of Harvard's Business School. It addressed implementation and management issues. It saw things all the way to the end. It emphasized evaluation.

For a while that program encompassed doctorates in nutrition planning and short courses for midcareer practitioners. Former students of that program are now in important nutrition positions around the world—UNICEF, the International Food Policy Research Institute, UNESCO, the World Bank, as well as principal nutritionists for many governments. For a while it did work, but eventually the program faded away. Trying to do something well in an academic setting that was seen as partly nonacademic, the program was at odds with the university culture and reward system.

Maybe it was an idea before its time. If so, maybe it is time to recapture it and develop institutes dedicated to applied work and training nutrition engineers—not that the MIT program should be copied exactly, but that concept and experience could be used to build something new and appropriate to 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. Obviously, place greater emphasis on applied subjects for research and training, including nondegree as well as degree programs, and at least teach the techniques of location-specific empirical analysis, illustrated with examples of real research. (Tufts and Cornell, both of which picked up elements of the MIT concept, already are doing some of this.) Second, incorporate research and training and the analysis and evaluation of operations in a feedback loop. Third, stimulate more interaction with local nutrition managers and local institutions, between universities and programs in the field. Some things cannot be learned in the classroom; therefore, training must take place in the organic setting.

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 the decimal points on old issues and examine the whole numbers of new issues that matter. And perhaps requirements for faculty positions should 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, with the primary standard being how much difference the program will make in overcoming malnutrition.

New institutional setting needed

Even with programs that emphasize applied training, universities are unlikely to address many practical needs laid out here. Operating agencies should not try to turn universities into instruments to satisfy their own nutrition-research requirements, because universities legitimately have different roles 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 US AID in population may be instructive. In the mid-1970s, it gave universities considerable sums to study population growth, but those studies were leading to little betterment. 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 turned to the Population Council and other private contractors. The payoff was substantial; governments in Bangladesh, Egypt, Kenya, Taiwan, and Thailand, for example, looked at what happened in those small quasi-government set-

tings 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—being problem-driven unlike the university and unlike the consulting firm, which is essentially client-driven. The Council exists specifically to find ways to solve 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 operations, it has a research interest (much involved in the field testing of ideas) and a knowledge interest. Endowed by foundations and with an independent international board and its own funds, it is capable of setting its own agenda.

Improving capacity within developing countries would be a main purpose of both a university-based institute to produce nutrition engineers and a nutrition equivalent of the Population Council to do demonstration projects. A prime objective of all 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 policy and programmatic orientation in their work.

What the nutrition community has learned in a few places—like Iringa and Tamil Nadu—is opening up some large-scale operational opportunities for which increased amounts of money are available. What is needed now is the leadership, nutritionists equipped for the task, and institutional underpinnings required to move in new directions, step by step, location by location, year by year, and initiative by initiative. A first step toward achieving this 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 intellectually satisfying. What could be more satisfying than addressing the most important constraints to making a serious dent in malnutrition?

Related letters to the editor, solicited by the Editor-in-Chief and written in response to this commentary, can be found on pages 86 and 89. Additional discourse on this controversial topic is expected in coming issues. 

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