No Fundamentals—No Food—No Future

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IN OUR PART OF THE WORLD, scientific developments in the means of production and transportation have so far prevented the food shortages predicted by Malthus. But a new facet of this problem has arisen that is of immediate concern to us. If the underdeveloped nations do not obtain their essential food requirements, we will lose them as allies in this struggle of the ideologies, whether this takes the form of a troubled peace, cold war, or contained war. The problem is real, not academic or idealistic.

As food production rises in these countries, the processing and preserving of the product will assume increasing importance. Losses through deterioration are appallingly high everywhere, and in an underdeveloped country such losses could nullify an appreciable portion of the increased production. Again, products we waste for economic reasons, or through inferiority in quality, will have to be preserved in a starving nation. Success in combating these problems depends on food research and technology.

Research is an uncertain game and the desired result cannot be guaranteed in advance. The probability of success is highest in institutions having the facilities, research personnel, and a record of research accomplishment. Technical aid agencies should recognize that they will get the best value for their research dollars by making grants-in-aid to such institutions as these.

When the specific needs of a given area have been determined, the research institution accepting funds for their investigation must not divert its effort to other fields even if the level of scientific achievement seems unrewarding. Remember, no food—no future. Processes can be transferred to laboratories or plants in the needy area when technological success is assured.

International food technology has another urgent need—fundamental research into basic principles. The results of such investigations will benefit all countries regardless of their stage of development. Such work need not be supported from technical aid funds. Throughout the world, basic research bearing on food technology is being done now. Considering its importance, the extent of this effort is too small, and since such work is of a long-term nature, it may soon be too late. Technological development is now so rapid that the pool of basic knowledge must be even more adequate than in the past; the present-day trend seems to be in the opposite direction. Food research has reached maturity and must accept responsibility for advancing the basic biological, chemical, and physical sciences. This responsibility is accepted by institutions whose primary interests are agricultural or medical. The food investigator must not lean on sister disciplines for basic knowledge, but rather add its own contribution to fundamental knowledge.

Psychologically the problem of the food technologist in the past has been difficult. Food was something everyone knew all about, specialized training was considered unnecessary, and research on the subject was regarded as a waste of time and money. Recently the attitudes have changed and science is gradually replacing art in food technology. A high degree of empiricism remains; a given process may work but we do not know why. Like Appert, we may be right (heating food in hermetically sealed containers) for the wrong reason (exclusion of air).

Recorded history shows that such new approaches, usually arising from fundamental work, have solved many major problems in food technology. A century ago, Britain was at her industrial zenith, but the population had increased beyond the home supply of perishable food. Meat was plentiful abroad but there was no method of transporting an acceptable product. As late as 1870 this problem was so acute that a special committee of the House of Commons was studying the problem. Many proposals were made, but I found no mention of mechanical refrigeration. Within a few years science had advanced sufficiently to produce commercial marine refrigeration. Britain's problem was solved by an unpredictable new method. A similar new discovery could solve many of the present-day problems.

(Excerpts from an address presented on the occasion of the dedication of the John Thompson Dorrance Laboratory, Massachusetts Institute of Technology, June 25, 1953)