SUPPORTING PAPERS: WORLD FOOD AND NUTRITION STUDY

VOLUME II

Study Team 4

Resources for Agriculture

Study Team 5

Weather and Climate

Digitized by Google

Digitized by Google

SUPPORTING PAPERS: WORLD FOOD AND NUTRITION STUDY

VOLUME II

Study Team 4

Resources for Agriculture

Study Team 5

Weather and Climate

Commission on International Relations
National Research Council

NATIONAL ACADEMY OF SCIENCES
WASHINGTON, D.C. 1977

The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the Committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

Library of Congress Catalog Card Number 77-087579

International Standard Book Number 0-309-02726-8

Available from:
Printing and Publishing Office
National Academy of Sciences
2101 Constitution Avenue, N.W.
Washington, D.C. 20418

Printed in the United States of America

This is Volume II of Supporting Papers, World Food and Nutrition Study. The other volumes are:

Volume I

Study Team	1	Crop Productivity
Study Team	2	Animal Productivity
Study Team	3	Aquatic Food Sources

Volume III

Study Team	6	Food Availability to Consumers
Study Team	7	Rural Institutions, Policies, and Social Science Research
Study Team	8	Information Systems
Study Team	10	Interdependencies

Volume IV

Study Team	9	Nutrition
Study Team	12	New Approaches to the Alleviation of Hunger

Volume V

Study Team	14	Agricultural Research Organization

PREFACE

Shortly after the World Food Conference held in Rome in 1974, the President of the United States wrote to the President of the National Academy of Sciences asking the Academy to make an assessment of the problem of hunger and malnutrition and "develop specific recommendations on how our research and development capabilities can best be applied to meeting this major challenge."

The study was begun in June 1975 by a Steering Committee appointed by the President of the Academy. It produced two reports for the President: an Interim Report, published by the Academy in November 1975, and a final report, World Food and Nutrition Study: The Potential Contributions of Research, published in June 1977. The Steering Committee was assisted by 14 study teams appointed by the Academy to analyze and make recommendations to the Committee on various portions of the study (Table 1).

This publication is one of five volumes containing the reports of Study Teams 1-10, 12, and 14. Study Team 11's report overlapped the other reports and has been integrated with them. Study Team 13's report of its ranking of research priorities was an integral part of the work of the Steering Committee. Consequently, it is not published here. Study Team 13's work is explained in detail in Appendix B of the Steering Committee's report.

The Steering Committee is deeply grateful to the chairmen and members of the study teams for their dedicated work, undertaken for the most part under heavy pressure of time. This work provided the greater part of the source materials for Chapters 2 and 3 of the Steering Committee's report, dealing respectively with "High Priority Research" and "How to Get the Work Done." The study teams are responsible for the content of their reports, which were reviewed by appropriate members of the Steering Committee.

Study Teams 1-12 were asked to identify areas of research and development that had outstanding prospects for helping to meet world food and nutrition problems

Table 1. Study teams, World Food and Nutrition Study

Study Team	Title
1	Crop Productivity
	Subgroup A Pest Control
2	Animal Productivity
	Subgroup A Animal Health
3	Aquatic Food Sources
4	Resources for Agriculture
	Subgroup A Farming Systems
	B Land and Water
	C Fertilizers
	D Energy and Equipment
5	Weather and Climate
6	Food Availability to Consumers
	Subgroup A Food Losses
	B Food Processing and Preservation
	C Food Marketing and Distribution
7	Rural Institutions, Policies, and Social Science Research
	Subgroup A Policies and Program Planning
	B Research, Education and Training, and Extension
	C Finance, Input Supplies, and Farmers' Organizations
8	Information Systems
9	Nutrition
10	Interdependencies
	Subgroup A Population and Health
	B Energy, Resources, and Environment
	C International Trade Policy and Comity Between Nations
	D National Development Policies
11	New Approaches to Increasing Food Supplies
12	New Approaches to the Alleviation of Hunger
13	Research Priority Assessment
14	Agricultural Research Organization
	Subgroup A Research Organization in the United States
	B Global Agricultural Research Organization
	C Development of Research Personnel

over the next several decades. For each such area, they were asked to respond to three questions:

-- What advances in knowledge will specific areas of research produce, and what is the scientific or technological significance of these advances?
-- If the research produces results, what effect would they likely have on reducing global hunger and malnutrition over the next several decades?
-- What supportive action will be required to conduct research for the accelerated activity recommended (e.g., more resources, policy changes, organizational changes)?

The study teams were asked to base their selection of research areas on their answers to the first two questions. Answers to the first and third questions provided insight on the feasibility of each research area. This information was used by Study Team 13 and the Steering Committee in the selection of research priorities. It also provided raw material for Study Team 14 and the Steering Committee when they considered steps to mobilize and organize resources to implement the proposed research.

Each study team's selection of high priority research areas involved two steps. In the first step, the study teams reviewed research recommendations and possibilities for research provided by existing reports, by the study team members themselves, and by hundreds of other people who were consulted, including many from other countries. From the hundreds of research possibilities, each team selected a limited set that would likely have the greatest global effect on hunger and nutrition. The second step narrowed this set to research areas whose potential was thought to stand out well beyond that of the rest of the group.

The Steering Committee hopes that these study team reports will provide rich source materials for those interested in pursuing the various subject areas in greater depth than could be done in the report of the Steering Committee.

Harrison Brown
CHAIRMAN
Steering Committee
World Food and Nutrition Study

Report of Study Team 4

RESOURCES FOR AGRICULTURE

STUDY TEAM 4

Jan van Schilfgaarde, CHAIRMAN, U.S. Salinity Laboratory, U.S. Department of Agriculture

SUBGROUP A FARMING SYSTEMS

Edwin Oyer, CHAIRMAN, ARD-Litbang, Bogor, Indonesia
Donald L. Plucknett, LEAD ANALYST, University of Hawaii and U.S. Agency for International Development
Lee R. Martin, University of Minnesota and U.S. Agency for International Development
Jose Vicente-Chandler, U.S. Department of Agriculture
Frank G. Viets, Fort Collins, Colorado

SUBGROUP B LAND AND WATER

G. R. Stairs, CHAIRMAN, University of Arizona
Pedro Sanchez, LEAD ANALYST, North Carolina State University
William E. Martin, University of Arizona
Arnold C. Orvedal, Lanham, Maryland
Dean Peterson, U.S. Agency for International Development

SUBGROUP C FERTILIZERS

Lewis Nelson, CHAIRMAN, Tennessee Valley Authority
Paul Stangel, LEAD ANALYST, International Fertilizer Development Center
Douglas Lathwell, Cornell University
William P. Lockeretz, Washington University

SUBGROUP D ENERGY AND EQUIPMENT

W. E. Splinter, CHAIRMAN, University of Nebraska
David Wolf, LEAD ANALYST, Sperry-New Holland Company
B. Delworth Gardner, University of California, Davis
Roy Harrington, Deere and Company
Gary H. Heichel, University of Minnesota
and U.S. Department of Agriculture

TABLE OF CONTENTS

INTRCDUCTION, 7

SUBGROUP A FARMING SYSTEMS

INTRCDUCTION, 12

Profile 1 A Methodology for Farming Systems Research, 13
Profile 2 Economic and Socioinstitutional Environments Affecting Agricultural Production, 52

SUBGROUP B LAND AND WATER

INTRCDUCTION, 66
Land, 66
Water, 69
Transfer of Science and Technology, 72
Yields, 72
Maximum Possible Production, 73

Profile 1 Soil and Water Management Systems for the Tropical Oxisols and Ultisols, 74
Profile 2 Soil and Water Management Practices for Multiple Cropping Systems, 85
Profile 3 Water Management and Utilization in Rainfed Agriculture, 92
Profile 4 Technical Land Classification Systems, 101
Profile 5 Plant Adaptation to Soil and Water Stresses, 108
Profile 6 Biological Utilization of the Rhizosphere, 115
Profile 7 Utilization of Irrigation Water, 124
Profile 8 Use and Predictability cf Weather Information for Farm Management Decision Making, 136

SUBGROUP C FERTILIZERS

INTRCDUCTION, 146

Profile 1 Low-cost Sources of Nitrogen for Use in Fertilizers, 149
Profile 2 Plant Nutrient Efficiency, Particularly in Tropical Regions, 159
Profile 3 Mining and Processing Methods for Phosphate Ores, 168
Profile 4 Recycling Organic Wastes as Nutrients and Soil Conditioners, 179
Profile 5 Factors that Determine the Supply, Price, Distribution, and Use of Fertilizer, 186

SUBGROUP D ENERGY AND EQUIPMENT

INTRCDUCTION, 196

Profile 1 Operational Parameters for Humans and Animals, 201
Profile 2 Optimal Alternative Agricultural Power Systems, 207
Profile 3 Nonpetroleum Energy Sources, 217

INTRODUCTION

Any consideration of ways to increase world food production must be based on the conservative management of inherently limited natural resources--that is, on sustained productive use that maintains or enhances these resources over time. The use of these resources in agriculture cannot be considered in isolation from other social needs. Nor can questions concerning technological natural resource management for food production be separated from institutional factors.

As elaborated upon in the introduction to the report of Subgroup B, "Land and Water," several worldwide inventories of potentially arable lands have been prepared. Only about 41 percent of the physically arable land in the world is currently under cultivation. While the remaining areas often do not coincide with regions of dense population or present special problems, such as excessive erosion hazards, substantial expansion of the world's total cultivated land seems inevitable. At the same time, better soil and water management can substantially increase crop yields, and potentially available water resources would permit substantial expansion of irrigated lands to perhaps twice their current area. Thus the possibility exists for both significant increases in production per unit of area and expansion of the total world area under cultivation.

It should be stressed, however, that land and water development decisions are frequently irreversible, or nearly so. The tragic deterioration of land used for cotton in the southeastern United States during the nineteenth century is a good example. A great deal is known about methods of erosion control and land conservation practices; yet the record with reference to their use is not always encouraging. For purposes of the present study, it was assumed that any new technology or transfer of existing technology for increasing the effective use of soil and water resources would, a priori, be directed toward conservative use. Thus the omission of any profile dealing specifically with erosion control pactices was

deliberate: good farming practices naturally conserve the resource base.

Pereira (1975), after noting that erosion in the African tropics has been increasing over the last 30 years, postulated that the soil and water resources of the tropical world will not be sufficient to sustain projected increases in food production because of their destruction. Such problems are even more acute in parts of Asia and Latin America. As population pressures increase, subsistence agriculture will expand into areas with steeper slopes, shallower soils, and lower and more erratic rainfall. Agricultural development always modifies the hydrology and generally results in substantial increases in both total and peak runoff. Even where on-site damage is prevented, substantial problems can develop downstream. Conservative practices are mandatory; they are only acceptable to the farmer, however, if they are accompanied by immediate increases in crop yield. Public policy may well have to intervene. The key to sustained higher production is the simultaneous introduction of farming methods that conserve resources and raise yields. While there are numerous questions which require further research, the current state of knowledge does permit constructive progress.

Technologies to improve crop production are not applied in a vacuum. They form part of a farming system which, in turn, makes up part of a socioeconomic system. Thus we elected to place a discussion of farming systems first in our report. Since all efforts directed toward increasing food production should be related to existing or evolving farming systems, the first profile may be viewed as a basic philosophical framework for establishing priorities in research and development. To stress the importance of viewing any technological advances within the framework of the total environment, the second profile stresses the socioeconomic aspects of agricultural production. Economic analysis must be part of the evaluation of any technological innovation; such evaluation, however, has little validity unless it considers the total matrix of forces affecting the producer and the consumer.

The remaining profiles may be considered as components under the broader systems umbrella. Choices had to be made. It was recognized, for example, that serious problems remain in densely populated vertisol areas of India, and that the semiarid regions of the world have aroused much recent concern. The emphasis on oxisols and ultisols of the African and South American tropics reflects the viewpoint--not shared by all--that these land resources present not only the greatest potential for future development, but also some of the least well-understood problems. These

areas are certain to be developed, and substantial long-term damage may be done unless adequate technology is developed on a timely basis.

A primary question in semiarid regions concerns the interface between rangeland and cropland. The problems of the Sahel, for example, reflect a forcing of this boundary beyond ecologically safe limits. In one sense, the problem here is one of management rather than technology, of socioinstitutional relations rather than natural science. Yet there is some concern that the interactions among livestock production, forestry, and crop production have not received their proper emphasis. Possibly this is justified by the relatively small impact of these resources on food production per unit of area. Still, there may well be inadequate attention given to mixed livestock and cropping systems and their special problems.

Substantial emphasis is given to fertilizer production. This should be seen in perspective, however, since there are alternative means of providing for adequate plant nutrition. The profiles on the recycling of organic wastes, on breeding for stress tolerance, and on modulation of the rhizosphere are intended to provide the proper balance. In view of ultimate, if not short-term, shortages of natural resources--fossil fuels, phosphate rock, as examples--the long-term significance of effectively minimizing dependence on outside fertilizer inputs is likely to be quite high.

Throughout the profiles, the need for good data bases is recognized. A particularly pertinent example is the need for soil taxonomic information to transfer the results of field experiments to other geographic areas. Too often, research is repeated time and again when already existing information could have been used for better structuring of hypotheses. Such information has limited value unless it is accessible.

Finally, Subgroup D, "Energy and Equipment," bases its recommendations for equipment research on two basic premises: (1) the proper mix between human and animal power and mechanical power cannot be approached rationally without good ergonomic data on humans and draft animals, and (2) with the exception of intercropped systems, the necessary basic equipment is generally available somewhere in the world. Thus the need is for engineering analysis and adaptation rather than for totally new designs. Although finding alternatives to petroleum fuels is a worldwide problem, many developing countries in particular suffer from a shortage of petroleum products.

In summary, we recognize exciting possibilities for breakthroughs in new technologies, but wish to stress the potential contribution that may result from the

innovative and imaginative transfer of existing concepts and ideas to new situations.

Report of Subgroup A, Study Team 4

FARMING SYSTEMS

INTRODUCTION

Farmers normally grow more than one crop, and they must integrate a wide range of physical, biological, economic, and sociological factors into a comprehensive system. Traditionally, however, agricultural research has tended to address isolated parts of the total system, leaving the application of the findings to the farm operator. Athough good research has always, in an informal sense, been based on the research worker's awareness of the total environment in which the results would be applicable, the formal concept of applying agricultural research tc the overall farming system is relatively new, and its methodology has not yet been worked out satisfactorily.

Since the benefits of physical and biological research in agriculture can only be realized within a socioeconomic framework, it can be argued that all such research has important social and economic components. This means that economists and social scientists must play a role in helping to solve the world's food problems. That role is essential if whole farming systems are to be evaluated rather than the consequences of single technological innovations in the abstract.

Thus the members of Study Team 4 chose to begin this report with research profiles on farming systems and on economic and socioinstitutional considerations. The first profile provides a framework for a methodology for farming systems research; the second eliminates the need for repeatedly defining the social components of research studies recommended in the profiles of the other subgroups, and emphasizes the systemic nature of socioeconomic effects.

Profile 1

A METHODOLOGY FOR FARMING SYSTEMS RESEARCH

INTRODUCTION

Contemporary authors (e.g., Wortman 1976) generally agree that a logical strategy for meeting the world's needs for more food and better nutrition is to stimulate food production in the developing countries where the need is greatest. This strategy also is propounded in the Interim Report of the Steering Committee of this Study (NRC 1975d).

McNamara (1974) estimates that 70 percent of the people in developing countries live in rural areas, 700 million of whom are landless laborers or farm families who cultivate less than 5 hectares of land. He also estimates that 100 million farmers in the developing countries are in the "small" category, and states that these small farmers constitute, in many developing countries, the base for increased agricultural production.

The typical small farmer in a developing country manages a complex farming system. A number of commodities are produced both for family subsistence and cash sale. Animals are almost always present as a source of power, food, fuel, or income. Many and varied plant species are managed as sources of food for the family and its animals, as well as for fuel and for sale to others. Some enterprises (e.g., tree crops) may be more specialized than others, but providing for the well-being of the household at the lowest cost and risk is usually the priority objective.

Most modern technology is designed to improve a single crop or animal species rather than a complete farming system. As a result, the small farmer generally has not benefited greatly from modern agricultural technology, which tends largely to be specific to single crops and requires capital investment that the small farmer may not be able to afford or choose to risk. Wrigley (1971) states that the traditional farming systems used in the developing

countries will not be able to feed growing populations and produce cash crops without the introduction of modern techniques. Nonetheless, he remarks that these traditional systems, despite their apparent lack of efficiency and orderliness, are quite well adapted to sustained production with available tools and points to the need to be aware of the effect of any new practice on an entire system as well as the need to provide the small farmer with both the education necessary to perceive changing situations and the incentives to adapt to change. Too often, near-crisis situations in tropical areas lead to simplistic approaches to increasing food production, such as applying methods better suited to temperate countries. In addressing this point, Wrigley states: "To repeat such mistakes would be inexcusable and tropical agriculture must rest on its own foundations." But does an adequate foundation exist? While increasing attention has recently been given to agricultural research in tropical areas, the level of research is still minuscule in relation to need or in comparison to that in temperate areas.

Terms of Reference and Definitions

It is doubtful that one universally satisfactory definition of a farming system exists. Duckham and Masefield (1970) enumerate five factors which influence the occurrence and importance of plants and animals in a given area: (1) ecological factors, (2) infrastructural features, (3) external economic constraints, (4) internal operational factors in the farm enterprise, and (5) personal acceptance of the required activities by the farm family. Ruthenberg (1971), in describing farming systems in the tropics, speaks of the "natural, economic and socio-institutional conditions of agricultural production" which vary widely from place to place and over a period of time. He points out that "in the process of adopting cropping patterns and farming conditions to each locality, more or less distinct types of farm organization have developed." Ruthenberg classifies farming systems as either collecting, cultivating (with six subdivisions), or grassland utilization (with five degrees of stationariness).

Because of the environment-specific nature of farming systems, we refrain from making specific recommendations on known systems. We believe that our scientific community does not know how best to conduct research on farming systems, and we propose a methodology for studying agroclimatic and socioeconomic systems.

Some limits must be placed on the scope of the problem to be considered. Since a farming system is influenced by many factors, recommendations for research on farming systems might address concerns ranging from the individual farm through the socioeconomic infrastructure of the numerous national governments to the international system of trade, markets, etc. The systems can be separated into micro- and macrolevels, the microlevel being the system practiced on individual farms, and the macrolevel relating more to regional or national distribution of inputs and products, international trade, and the like. A description of the various levels of agricultural systems is given in the National Academy of Sciences (NAS) publication, African Agricultural Research Capabilities (NRC 1974a). We limit our concerns to farming systems at the microlevel, and believe that the recommendations that follow are applicable to farming systems everywhere. We define a farming system as an agricultural system that has evolved over time as a result of the interactions among the soil, water, and climatic base; human capabilities; and economic factors such as capital and markets in the region. We recognize that adjustments in an existing system must be made cautiously, and that, except at the margins of the different systems, shifts between systems are difficult to achieve because of costs, capital requirements, and cultural values.

RECOMMENDATION AND RATIONALE

This subgroup makes only one basic recommendation:

- Initiate logical, methodical, and sensitive research into the farming systems of the developing countries, with emphasis on their present status and potential for improvement.

Improvement of farming systems is one way of effecting an increase in food production and improving the nutrition of millions of small holder families in the developing countries. We will suggest a framework and philosophy for such an approach. Our concern centers on how research about farming systems should be done. Major benefits of such research should be: (1) improved problem identification and more focused research; (2) better understanding of appropriate technology for the farm itself; (3) a real basis for multidisciplinary research; (4) reasonable and reliable partnership with the farmer in solving problems and in making adjustments in the system; (5) a better

understanding of the land, human, and capital resources available to individual farm units; (6) a better understanding of important household matters--nutrition, fertility, education, health, etc.; and (7) a better understanding of the economic, social, and political environment within which farms, households, and the related private industries and public agencies operate.

Selecting Candidate Systems

How might farming systems be selected for study? We propose that the following questions be raised in order to help set priorities for the study of various farming systems within a country:

-- How many people are fed by or are dependent upon each system?
-- What are the nutritional benefits of each system?
-- Is land and water utilization in each system degenerative or conservative?
-- How effectively is the productive labor available from farms used in the system?
-- What are the technological opportunities and potentials for change in each system?
-- What is the biological opportunity for changing each system?
-- What are the external requirements, i.e., what inputs are needed from outside each system?
-- What kinds of risks are present in each system?

Research on farming systems must be multidisciplinary. The leader of a farming systems research team is likely to be a "generalist" who has the ability to diagnose problems and coordinate the activities of specialists in several selected disciplines to solve identified problems. The disciplinary expertise needed will, of course, depend on the target problem and system.

Because of the broad range of factors that affect a farming system, the report of nearly every group of the World Food and Nutrition Study is related to farming systems. For example, in Profile 4 on plant adaptation to stress, Study Team 1 urges the selection of plants with improved tolerance to drought and calls for improvements in cropping and tillage practices. Likewise, the profile of Study Team 1 on interactions of species is pertinent to farming systems. Study Team 2 refers to the need for range and forage improvement for the maintenance and utilization of animals in farming systems. Numerous sections of the report of Study Team 4 are pertinent to farming systems, e.g.,

Profile 1, Subgroup B in this report, addresses the issue of soil and water management for crop production on oxisols and ultisols in tropical areas, and Profile 2 of the same subgroup proposes research on cropping systems for these important soil groups. Improving plant nutrient efficiency (Profile 2, Subgroup C), utilization of both human and animal power (Profile 1, Subgroup D), and machines (Profile 2, Subgroup D) are all areas of potential concern and application to the improvement of farming systems.

An Approach to Farming Systems Research

Conventional research approaches are not suitable for farming systems. Many techniques used presently are based on plot work at or near research stations. Most present research also is devoted to single crop systems. Such methods are limited in situations where (1) the natural resource base varies widely; (2) more than one crop or enterprise is involved, as in mixed farming, intercropping, sequential cropping, relay cropping, and other systems; (3) seasonal changes affect the crop and/or animal mix, crop growth, crop utilization, or other factors; (4) harvest periods are staggered or vary widely; (5) yields or returns for several crops or enterprises vary in types of measurement or values; (6) the availability of labor at certain seasons effectively limits the range of choices among crop-livestock combinations for typical farmers in the system; and (7) insufficient inputs or inaccessible markets effectively limit the choices farmers can make.

The research approach should deal with the dynamic nature of farming systems while at the same time identifying common elements among systems that are important to the farmer and the household. In many developing countries, women share in the decision making, management, and physical labor required in the farming system. By considering the farm and household as a unit it should be easier to measure the unmarketed as well as the marketable inputs and outputs that explain the workings of a particular farming system.

Ecological research techniques have been employed by range and pasture ecologists to understand the dynamics of the growth and behavior of a number of pasture plants, both desirable and undesirable, across a widely variable land and climatic resource base, and as influenced by the activities of people and grazing animals. This research is based on the belief that it is necessary first to delineate broad ecological zones which possess certain characteristics and then to

refine progressively the measurements and characterizations of identified subunits.

The Concept of the Transect

In order to understand the limits and parameters of a system, the ecologist lays out transects encompassing the principal variations within the system. Spot sampling along the transect is then used to obtain more detailed information on representative areas. Applied to farming systems, these variations may include cultural, social, and economic as well as biological and physical aspects.

The transect, or line, encompasses areas characterized by important differences in one or several significant variables. A line that cuts across areas of variable precipitation, altitude, or soil type, for example, would be a meaningful transect. Once the meaningful transect is located, observations are made along it. These observations can be made at long intervals for some purposes and at short intervals for others.

The transect approach has a number of strengths in farming systems research. Its greatest value is in identifying components of current systems that need attention, i.e., it should detect important problems (or limiting factors) and provide the context within which several problems can be compared. This transect approach also can indicate the representativeness of experimental plots and identify the technologies to be tested on farms.

No rules as to the number of transects or their directions can be given in advance. Professional judgments are needed on how to orient them in order to sample adequately the important macro- and microvariations. The scope of a transect is determined by macrovariations in climate, resources, population densities, and customs of the people of the region. The transect also must encompass such microenvironmental factors as field slope or drainage. The width of the transect will generally not be uniform throughout its length, but it must be sufficient to take in a whole farm. Where farms are small, several may have to be included at a given point along the transect. Transects may be laid out in parallel to cross the region, or they may cross at angles to sample the macrovariations. Sample farms must be representative and reflect the interactions among physical, biological, social, and economic forces. Criteria for selecting representative farms are extremely important because intensive farmer-researcher

interaction is necessary to achieve the benefits of the methodology.

Proposed Phases of the Research

We propose that research be done in four phases which will usually but not invariably be sequential. The first phase, the identification of target systems, involves collecting, collating, and synthesizing, largely from secondary sources, as much pertinent information as possible on the natural resource base for agriculture and the current degree of exploitation of these resources. This information should lead to the general identification of constraints and opportunities for betterment and the selection of farming systems that seem worthy of study. This is followed by "windshield" surveys of potential target areas and the selection of transects for the second phase.

In the second phase survey transects are conducted. Survey transects are established so that multidisciplinary teams can collect data on existing farming systems, the management of farms, the customs and traditions of the people, and the resource base. The farm, including the household, is the sampling unit. Survey transect studies will identify research priorities and representative farms which will undergo more detailed examination in phase three. Data on household behavior and human resources on the farm should be collected in this phase, as well as data on the physical and biological character of the farm.

In the third phase, detailed transect studies by specialists are used to evaluate important components of the farming system identified in phase two. Here the farm field is the sampling unit. Examples of a detailed transect are a study of disease incidence or spread in a field, or of competition between two crops in an intercrop system. Phase three will identify high priority research needs for phase four. The number and specificity of disciplines likely to be involved increases during the progression from phase one to phase four.

Most phase four studies can be termed "derived research" in that problems identified have been derived from a systematic study of the farming system and its needs. Some phase four studies might be stimulated by new biological or technological opportunities, but the results of these studies should be suitable for ready transfer to the farm, inasmuch as phases two and three have characterized and analyzed the farmer and the farmer's needs, as well as the biophysical and socioeconomic environment of the farm itself.

The study team's assessment of the activities, outputs, and potential benefits of each phase of this ecological approach to farming systems research is presented in outline form below.

Phase One: Identifying Target Systems

Purpose: To use what is known to make better selection of target systems and to improve research planning.

Outputs: Identification of (1) priority systems and initial transects, (2) additional data needed, (3) needs for information systems and capacity to gather information, and (4) a multidisciplinary team.

Examples of activities: (1) Selecting priority production systems; (2) establishing baselines through the use of existing information on soils, climate, agricultural, and socioeconomic status; and (3) conducting windshield surveys.

Follow-up: Interpretation, synthesis, and evaluation of information. The collected information is interpreted for use in planning farming systems research. Such information should identify (1) major topographical units and changes, (2) rainfall boundaries, (3) soil boundaries, (4) probable cropping system land units, (5) major economic factors, and (6) ethnic boundaries and customs.

Intervention, utilization, and transfer capability. Intervention possibilities will be small. Utilization and transfer are likely to be restricted to information gathered, which then will be available for wider use. A major benefit should be an increased awareness of the information available for a country or a region.

Phase Two: Conducting Survey Transects

Purpose: To understand the farm (the field and the household) as a unit (basically qualitatively), its characteristics, and the elements it shares with other systems.

Outputs: Description and characterization of representative farms and representative systems; identification of targets (both problems and opportunities) for detailed study in phase three; and state-of-the-art studies on existing practices.

Examples of activities: (1) Organizing a multidisciplinary team; (2) obtaining farm management data on sample farms, across the major sources of variations (macrorelief) which appear to affect the system; (3) sampling several farms in an area to assess variability in microrelief; and (4) collecting the

physical, biological, social, and economic measurements needed.

Follow-up: Interpretation, synthesis, and evaluation of information. Some of the following identifications should be made at the end of this phase: (1) an overall picture of the system; (2) apparent problems and those which appear to be the most limiting; (3) the problems most capable of solution; (4) the detailed transects or studies that should be conducted to obtain needed information; (5) the characteristics or elements shared with other systems; (6) the policy problems that can be resolved soon; (7) the innovative farming practices that can be transferred elsewhere; (8) changes most likely to be compatible with existing farming systems; and (9) the effects that recommended policies, programs, and projects would have on representative farms and farming systems.

Intervention, utilization, and transfer capability. Intervention possibilities will usually be small except in cases where policy issues are a major problem. Opportunities to transfer simple, innovative farming practices may be good. State-of-the-art publications should assist in the transfer and adoption of such information in other areas.

Phase Three: Conducting Detailed Transects

Purpose: To understand the factors affecting the farm where the field is the sampling unit for biological, physical, economic, and social measurements, (i.e., measuring the adaptation of the system to the environment and assessing the prospects for improvement).

Outputs: (1) Recommendations for high priority research to solve problems, (2) recommendations for change in the system, and (3) the appropriate tools to monitor and measure changes on the farm.

Examples of activities: Detailed measurement and analysis of (1) water management and irrigation use and potential; (2) crop competition and plant variety interactions; (3) role and use of livestock and sources of feed; (4) pest ecology; and (5) customs, population, labor supply, risk factors, infrastructure, and institutions.

Follow-up: Interpretation, synthesis, and evaluation of information. Multivariate analysis can be used in analyzing and interpreting socioeconomic as well as biophysical data. Detailed transects in farmers' fields will provide a basis for measuring and evaluating the stability of existing farming systems. Components of systems in the field, including

measurements of the impact of new technology and analysis of the different sources of risk, can be most effectively evaluated here.

Intervention, utilization, and transfer capability. Intervention possibilities are great. Detailed transects can be used to evaluate the dynamics and impact of the components of a farming system. Some intervention opportunities will be quite clear after these studies are completed. Utilization and transfer opportunities also will be great; research identified will be of high priority and specific to the farming system involved.

Phase Four: Derived Research Needs

Purpose: To conduct problem-oriented studies which will support improvements in existing farming systems or suggest the components of improved systems.

Outputs: (1) Suggestions for system interventions; (2) better-adapted plant varieties suitable for specific systems; (3) methodology for the testing and adaptation of new technology on the farm; (4) recommendations regarding fertilizer, pest control, water management, and so forth; and (5) suggestions for innovations or new systems.

Examples of activities: (1) Conducting research on system components; (2) developing specific technologies for existing or improved farming systems; (3) systematically studying major limiting physical, biological, and socioeconomic factors; (4) identifying and implementing institutional roles at the regional, national, and international levels; (5) training researchers, technicians, and farmers; (6) synthesizing and evaluating improved farming systems; and (7) conducting economic and social research on such problems as functioning of irrigation systems, input and output markets, market demand, credit, land tenure, transportation, educational institutions, and health institutions.

Follow-up: Interpretation, synthesis, and evaluation will be (1) focused on the farmer and system; (2) multidisciplinary; (3) action oriented; and (4) strongly focused on the implementation of ideas and the adoption of new technology, the need for which is identified in the preceding phases.

Intervention, utilization, and transfer capability. The intervention opportunity is great because research results have been focused on the farmer, and the action-oriented research approach involves the farmer in the system. Intervention can be monitored by continuing the detailed transects of phase three. Utilization and transfer will increase, especially if

the research is connected with existing soil and land classification systems and relates or is adapted to varying socioeconomic situations.

This proposed methodology for studying farming systems combines techniques from several disciplines and draws from the work of other scientists (Blaut 1959, Harwood 1976, and P. E. Hildebrand 1976, unpublished data, International Confederation of Technical Agriculturists [ICTA], Guatemala). An example of its application and potential usefulness is the millet-sorghum farming system in the Sahel presented in Appendix A. To give an indication of the data useful for phase one, secondary data on farming systems, regions, and countries of the world are included in Appendix B.

IMPACT

Research into farming systems will provide important inputs for other kinds of agricultural development activities, such as in the design and implementation of extension, credit, and land tenure development programs. These programs must be integrated, not only to make existing systems more effective but also to assure the effectiveness of new systems for land to be brought into cultivation and to make the distribution of income from agriculture more equitable as well as increase the aggregate income derived from agricultural operations. Much of the information to be obtained from the continuing research process can contribute to the agricultural statistics needed for short-run and long-run agricultural policy decisions by farmers, other private decision makers, and public agencies.

While the need for highly trained personnel to conduct the farming systems research described here should not be minimized, it is also true that local nationals will receive valuable in-service training while assisting in each phase of the work. Because many agricultural scientists today, both in the United States and the developing countries, were not reared on farms, the application of this methodology will hasten their perceptions of the problems faced by people in actual farming situations. In addition, phases two and three will help to spread the many improved practices devised and implemented by better farmers to their neighbors. In the United States, experiment stations rarely have resources and inputs not available to better farmers, but in the developing countries some experiment stations employ techniques and resources that will not be available to small farmers for many years. This situation often results in the making of

recommendations that cannot be adopted by small farmers. Again, phases two and three of the methodology will ensure more rapid generation of an appropriate and applicable technology.

Three examples of the benefits of combining a variety of complementary practices into a farming system are cited below. These examples involve cropping systems; similar examples could be developed for mixed farming systems. The first example concerns coffee production in Puerto Rico, where the use of improved coffee varieties increases yields 20 percent, proper pest control increases yields 40 percent, growing coffee in full sunlight increases yields 40 percent, proper liming and fertilization doubles yields, closer planting increases yields 50 percent, and harvesting with plastic nets reduces the loss of coffee berries by 30 percent. When all these practices are combined, yields are increased tenfold, from an average of 200 to about 2,000 kilograms per hectare (kg/ha).

An example of the benefits of intercropping also can be found in Puerto Rico. The creation of coffee plantations which do not use interplanting may cost over $5,000 per hectare over a three-year period before the first crop is harvested. But intercropping with plantains during the first two years and with taniers (Xanthosoma spp.) during the third year can bring the plantation a net profit of $3,750 per hectare before the first coffee crop is harvested. About 40,000 kg/ha of plantains and taniers containing 10,000 kg of dry matter with 6 percent protein are produced during this period.

Furthermore, the use of selected cultivars can increase plantain yields 40 percent, close planting can double yields, and proper fertilization can increase yields 40 percent. Use of a nematocide and borer control can increase first-crop yields 30 percent and make possible the harvest of two ratoons without replanting. Application of all of these practices simultaneously can increase total yield over a three-year period from 24,000 to 100,000 kg of fruit per hectare. The increase in profits resulting from decreased production costs together with increased yield is much greater than might be deduced from the crop yield figures just given.

A second example of the usefulness of intensive multiple cropping is illustrated by a system developed for small farms in El Salvador where soil, climate, and moisture are not limiting factors and where labor is abundant (Hildebrand, unpublished data). Corn is planted in double rows 1.5 meters apart, and radishes and beans are planted between rows simultaneously with the corn. Radishes are harvested as they begin to

compete with the beans for solar energy. The beans attain their mature green-pod stage before being shaded excessively by the corn and are harvested. Cucumbers are then planted, and the corn is doubled over for drying in the field with leaves removed so that the cucumbers receive full sunlight. The corn stalks are tied to form a tripod which serves as support for the cucumber vines. Before the last cucumber harvest, cabbage is transplanted in the rows, and four weeks later corn is again planted in double rows in the space where the beans had grown. After the cabbage is harvested, pole beans are planted. When the corn is harvested the corn stalks serve as supports for the bean plants. Thus the beans are the seventh crop grown on the same land within a one-year period.

This system is so labor-intensive that one family can manage no more than one-half hectare, but the resulting income is much more than could be obtained on several times that amount of land by traditional methods. Wooden plows, knapsack sprayers, and hand tools are the only equipment used.

The third example comes from South Korea (M. Cox 1976, personal communication, University of Arizona), where there is no longer land to feed the population using traditional agricultural methods. The population has grown from 19 million to over 31 million since World War II, and there is no more relatively flat land to develop. Thus it has become necessary to double-crop as much land as possible with barley in winter and rice in summer. Wheat was the first choice for a winter crop because demand for it was high, but barley was substituted because of its shorter maturity time.

Under the traditional system, rice and barley interfered with each other. When it came time to transplant rice into the fields from seedling beds the barley was not quite mature. If the barley was allowed to mature completely, the rice was planted too late and its maturity was delayed by low temperatures in the fall. This resulted in reduced rice yields and also interfered with the timely planting of winter barley. A system had to be developed to assure annual crops of both rice and barley.

The problem was further complicated by the fact that the peak labor demand in the agricultural sector occurred at rice transplanting time. There was not enough rural labor to fill this demand except at very high cost because more attractive industrial jobs in cities were drawing people from the countryside. If efforts were made to increase the availability of farm laborers for rice transplanting they would be largely underutilized for the rest of the year. While it was possible to eliminate this peak labor demand by seeding rice directly in the soil, the transplanting by hand of

seedlings ordinarily grown under plastic covers advanced rice maturity by about 20 days.

If labor demands were to be reduced and two crops per year made possible, the crop overlap time plus the 20 days lost by direct seeding would have to be eliminated. The solution can be summarized as follows:

OPERATION	GAINS IN DAYS
Shorter season rice	20
Shorter season barley	10
Quicker seed bed preparation (tractors)	7
Directly planting barley in rice stubble without tillage	7
	44
Direct seeding of rice	-20
NET GAIN	24

There were complications in carrying out this solution. Quicker seedbed preparation and puddling with tractors saved time, but only on fields large enough to use the machinery efficiently. Rice paddies have traditionally been very small (.2 to .8 ha), and each was hand-leveled separately so that each small field might be as much as 5 to 10 cm higher or lower than contiguous plots. Furthermore, many farmers owned several widely separated plots.

In order to extend the use of machinery, a program of paddy rearrangement was instituted to create level fields of about 2 to 5 hectares. After the rearrangement each farmer owned approximately as much land as before, but it was all in one block. Farmers who wound up with slightly smaller holdings were compensated in cash, while those with slightly larger holdings were required to pay for their new land on a long-term basis.

Larger fields were then graded to uniform levels. Farmers often had to work their paddy and irrigate together, with only small ridges demarcating the land owned by each. Irrigation and drainage ditches were rearranged, and this made a little more land available to be distributed among the farmers.

Seeding barley into rice stubble without tillage required a simple grain drill; this measure was only possible if the rice fields were kept weed-free without cultivation.

By combining all these methods, a new cropping system was developed that allows two crops per year about 90 percent of the time at South Korea's middle latitudes. Farther south, the percentage is close to 100. Farther north, in the Seoul area (about 37° latitude), varieties of rice and barley with even

shorter maturities were needed to achieve 90 percent reliability. Further refinements have been made since the original work was completed.

The foregoing examples illustrate that there is a tremendous potential for improving food production and the nutritional well-being of people by applying a systems research approach. However, no new methodology will be useful unless the social and economic climates of a given country are open to change. What is outlined here requires a commitment of will, capital, and patience on the part of governments to build the necessary capabilities, institutions, and infrastructures to accommodate change. One advantage of the methodology is that improvements can be initiated and tangible results demonstrated in phases instead of delayed until a long-term conventional research program is completed.

The testing and improvement of the methodology is a long-term process that proceeds from broad and aggregative studies to more detailed analyses of farm activities and from the early use of a limited number of disciplines to the later use of a wide range of relevant sciences.

To the extent that regions are reasonably homogeneous in terms of soil, climate, and institutions, data and observations collected in one location can be transferred to other locations. Consecutive phases of the research can be conducted simultaneously in some situations, yielding results in a short time (one to two years). The more detailed studies will require a longer time frame (two to five years) for completion.

IMPLEMENTATION

Even though the four phases of our ecological approach to research provide a valid framework for the improvement of farming systems, it will still be necessary to ensure that the results of the research will be applied through some form of "extension." Research capabilities in the developing countries vary greatly, but "extension" or "delivery" systems are even more variable in quality.

Some mechanism for transmitting new research information to farmers is needed, and this mechanism also should be reversible, transmitting needs and problems at the farm and field level back to researchers. Several important components of this mechanism can be identified.

Once new information is developed, it must be interpreted in light of existing situations and practices in order to become an integral part of the

synthesis of an improved farming system. After synthesis the new technology must be evaluated in the real-life farming system. These tasks of interpretation, synthesis, and evaluation can be conducted by the researcher or by the researcher and farmer working as a team. However, if the new technology is to receive broad application it must be transmitted by "technicians" or "change agents" who can assist in its implementation by a larger number of farmers. Thus the final step in the adaptation of the new technology can be termed intervention to stimulate change in present practices.

A diagram of the information flow system through the four phases of the research process is illustrated in Figure 1. There is connection and interaction among the last three research phases and the processes of intervention and utilization by the farmer. Information can and should flow in each direction.

Applying this methodology to a simple system in which there is little variation in soil and climate over the transect and in which secondary data are available, phases one, two, and three could be accomplished with as little as 2 to 6 scientist-years. In a more complex system, where a transect might cross wide variations in topography, soils, climate, and social customs, 20 to 30 scientist-years could easily

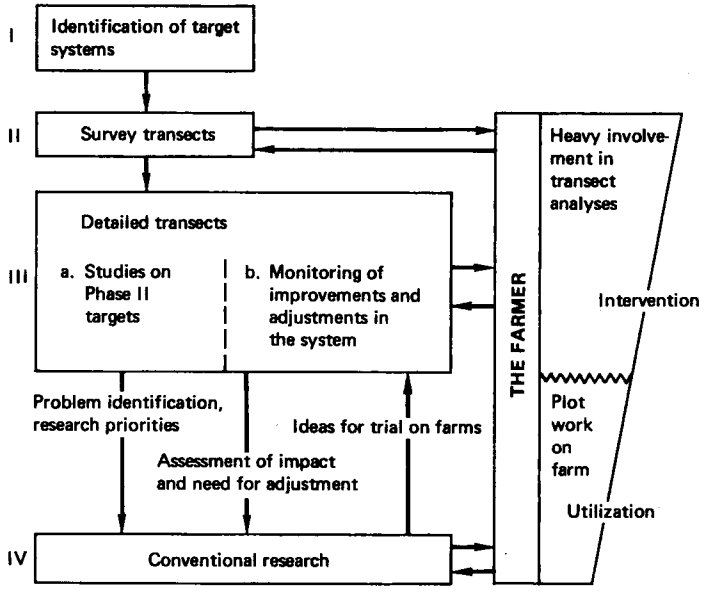


FIGURE 1 Basic operation of the ecological research approach.

be consumed in accomplishing phases one through three. Phase four is ongoing, and its extent will depend not only on the complexity of the problem but also on the need and resources available for its solution. Examples of the cost, time, and complexity of phase four can be taken from universities, national organizations, and international research centers whose mission is to conduct agricultural research.

Public agencies and educational institutions of a country would be involved in all phases of the methodology. The training of more researchers in an increasing number of disciplines will probably be necessary. Whether this is done in a university or government agency will depend on the country, but it should be emphasized that the approach is a participatory one. In a developing country, it will involve both foreign and local scientists. Some participants could come from universities in the high-income countries, some from international research centers, and some from local institutions. The expected result would be a self-sufficient farming systems research program in the developing country.

The time required to implement this research methodology will vary with a country's stage of development. If institutions to provide basic education must be created before implementation begins, at least a generation (20 years) will be required before a self-sufficient research effort comes into existence. Where educational and research infrastructures are already in place, the time required will be much shorter.

If the operational mode of much "modern" agricultural research is evaluated critically, it becomes quite obvious that phases two and three are often omitted; the researcher moves directly from phase one (literature review) to phase four (laboratory or field plot experiments). Farmers are not involved in the planning, execution, or evaluation of research done on their behalf. This may be the harmful result of our "modern," energy-intensive, monoculture agriculture. The derived research process is expensive, and experienced, highly trained specialists are needed at all levels of the research and intervention process.

Compare this situation with the one described earlier, where modest-sized multidisciplinary research teams function in each of the four phases, and two of the four phases employ the farmer as a partner and the individual farm as a laboratory. Even in phase four, where derived research is conducted, junior partners are being trained to make the entire cycle self-perpetuating and adapted to local institutions.

Appendix A

THE MILLET-SORGHUM FARMING SYSTEM IN THE SAHEL

Farming systems in the Sahel appear to be relatively simple land use systems that have evolved in response to the physical environment while accommodating themselves to political, cultural, and economic constraints and opportunities. Traditional Sahelian agronomic research for the most part has not focused on these farming systems but on specific crops, soils, production techniques, genetics, pest control, and related elements. Agricultural economists have generally studied the details of markets, transport, storage, processing income, credit, and government policies. Sociologists have studied customs, traditions, behavior patterns, political organization, and related areas. Such research has resulted in a large amount of useful information on the various aspects of the farming systems, but it has contributed only peripherally to the conceptualization and redesign of farming systems themselves.

Little is known about how the Sahelian farmer, perhaps through trial and error, has integrated the various aspects of farming into a production system--coordinating planting dates, harvesting dates, rainfall patterns, labor availability, storage facilities, transportation, and the like. Little also is known about how the natural environment has influenced the selection of crops and land use patterns, and few studies have been made that show how cultural and economic factors have imposed restrictions or provided opportunities for improvement. A research methodology designed to study multifaceted farming systems in a unified and multidisciplinary manner would not only provide an understanding of present Sahelian farming systems but also would indicate points where intervention in such systems could remove constraints or enhance opportunities. The intent here is to indicate how an ecological approach to farming systems research might be applied to a Sahelian millet-sorghum production system.

A research methodology designed to study multifaceted farming systems in a unified and interdependent manner must involve investigation of the ecology of the area being studied. It must include not only the physical environment (soil, water, topography, climate, vegetation, and so forth) but also the institutional artifacts that define the political, economic, and cultural milieu in which the people live. Such a research methodology should be broad enough to take into account the agronomic, economic, social, cultural, and physical factors, simple enough to be generally understood, efficiently managed, technically possible, and cheap enough to be economically feasible and operationally practical. This ecological approach to farming systems research requires expertise in a variety of disciplines, and the large mass of data involved will require the use of sophisticated multivariate analysis techniques. A major advantage of this approach is the increased probability that significant answers to some production problems may emerge prior to the development of the more expensive experimental station phase.

Range and pasture ecologists have long utilized transects as a sampling technique since they are often confronted with highly variable mixed vegetation occurring on different slopes and exposures, across many soil types, and under different grazing pressures. The transect method allows considerable accuracy in measuring these many variables, provided the transects are carefully laid out across as many variables as possible. In such a methodology the farmer is a dependent variable.

PHASE ONE

This phase is essentially a superficial examination of the country or region to be studied in order to gain a general idea of the "lay of the land."

The first step is the collection of as much published and unpublished material on the nature of the study area as possible, including any existing satellite imagery and/or aerial photographs. Low altitude flights over the area will provide a synoptic overview. Gross physiographic features, general topography, farming areas, livestock areas, towns and villages, roads, and so forth are noted. From the above the probable production areas to be studied are selected, and a preliminary selection is made of the areas through which the transects might pass. In the Sahel countries the transects should run north and south in order to cut across the most critical variable, the rainfall belts. The east and west

location of these north-south transects will depend upon the data collected.

Finally, the multidisciplinary team should travel through the area proposed as a transect site, observing the most obvious characteristics of the farming areas. The goal here is to gain general impressions and to avoid being diverted by details. If an unusual opportunity to collect detailed data presents itself, however, the team should take advantage of it.

At this point the agriculturist-ecologist should be collecting data on: (1) the quantity and quality of land available, (2) the topography, (3) general climatic factors, (4) vegetation, (5) erosion, and (6) livestock. The sociologist-anthropologist should be collecting data on: (1) local groupings and population, (2) tribal groupings, (3) areas of tribal occupancy, (4) villages and towns, and (5) rural compounds (housing). The economist should be collecting data on: (1) local markets, (2) transportation, (3) storage, (4) farm size, and (5) capital and labor availability.

In general, the team should try to determine the location and boundaries of major variables that should be taken into account when locating transects, and in what areas detailed transect studies may later be concentrated. At this point a preliminary decision should be made on the location of the transects, based upon what was learned in the above steps. Any or all of these transects may be moved or altered later. An initial decision also should be made on the width of the transects, making sure that they are wide enough to encompass whole farms. Note that these transects need not be uniform in width, they need not run directly north and south, and they need not be straight lines.

Outputs of Phase One

The outputs of phase one should include identification of the principal agronomic, economic, and sociological features of the study area that affect farming systems. They also should include identification of the variables that should be studied later in greater detail, as well as the establishment of the preliminary (and possibly the permanent) location of the transects.

PHASE TWO

This phase consists of a study of the transects to characterize the existing farming systems. The millet-sorghum farming systems now in use in the Sahel have

apparently evolved over long periods of time, with information and techniques being handed down from one generation of farmers to the next. Apparently there have been few innovators, or else there have been hidden factors limiting innovation. Neither plant spacing techniques nor thinning to reduce the number of plants per hill in order to better utilize scarce soil moisture are used. This is difficult to explain, since lack of moisture appears to be the principal factor limiting production. Furthermore, these systems do not appear to include crop rotation which would be an effective means of maintaining soil fertility and controlling weeds, insects, and plant diseases. It is common in the Sahel countries to see millet interplanted with cowpeas or peanuts year after year in the same fields. Witchweed, a parasite that greatly reduces millet yields, is widespread, but it has little parasitic effect on cowpeas or peanuts. The common headworm often reduces millet yields by 50 percent, but it too has little effect on cowpeas and peanuts. Thus rotating millet with cowpeas or peanuts in alternate years would interrupt pest reproduction cycles and greatly decrease losses, while the interplanting of these two crops perpetuates the pests of both.

Although these systems appear to be very primitive and seriously deficient in many respects, the team should be careful not to judge them until research is done to discover why things are done this way.

The multidisciplinary team should travel along the transects to collect more data on them, checking both length and direction to make sure the transects cover most of the macrovariations, and that they are wide enough to cover all significant microvariations.

Using one farm (or several small farms, if necessary) as the sampling unit, the team should obtain data on: (1) farming methods, (2) farm management, (3) physical and biological factors, in order to identify biological opportunities, (4) farm problems, (5) markets and prices, (6) transportation problems and storage facilities, (7) social systems, (8) social conflicts, (9) labor availability and utilization, (10) nutrition, (11) health, and (12) household practices and education.

Outputs of Phase Two

The outputs of phase two should include descriptions of the characteristics of the farming system, descriptions of farm management methods, descriptions of social systems, descriptions of farm households, better definition of the transects, and identification of targets for phase three.

PHASE THREE

This phase consists mainly of detailed studies along the transects, with attempts to quantify as many of the factors as possible, and to measure, or at least estimate, the interactions among agronomic technology, economic factors, social organization, and political policies. In this phase individual fields or parts of fields become the investigative unit, although in some cases individual farms may be studied in detail.

In the production area the team should concentrate on studying (1) water management, (2) irrigation and drainage, (3) salinity, (4) soil fertility management, (5) plant and insect pests, (6) the role of livestock, and (7) farm grain and forage storage techniques.

In the economic area the team should concentrate on: (1) price cycles (inputs and outputs), (2) commercial storage facilities, (3) production costs, (4) farm income and profits, (5) production risks, (6) capital availability and use, (7) labor costs, and (8) employment.

In the social area the focus should be on: (1) rural-urban relationships, (2) the role of women and children in agriculture, (3) relations between the nomadic grazer and the settled farmer, (4) attitudes toward outsiders, and (5) acceptance of new technology.

Some important and definitive findings should emerge from this phase, which should reveal whether the farming systems are fully or overutilizing the resource base, whether farmers' incomes are sufficient to promote production incentives, whether the systems are in harmony with the social order, and whether each country's institutions, laws, and policies tend to enhance or inhibit farm productivity. This phase should reveal any imbalances, inequities, or malfunctions in the socio-politico-technico-economic systems of the countries in the Sahelian study area. It should suggest possibilities for improvement without further research and the logical directions of new research. In short, this phase should define the effects of all the incentives and disincentives that bear on farmers, private agribusiness firms, and public agencies.

Outputs of Phase Three

Out of phase three should come a recognition of relevant problems and a set of recommendations for change accompanied by recommendations for research to guide future changes. An additional output should be an estimate of the likelihood of significant improvement in the overall system.

PHASE FOUR

The performance of conventional research will be facilitated by the researchers' experiences during phases one to three when, it is assumed, problem identification and analysis have been practiced and, hopefully, mastered. It is also assumed that the experienced scientists who supervised phases one to three were training junior partners during these exercises. While the experienced partner will continue to provide leadership during phase four, the junior partner can demonstrate the abilities of observation and analysis in the design, conduct, evaluation, interpretation, and reporting of experiments and experimental results. Targets of biological opportunity, for example, will have been identified in earlier phases. The result of proper and successful attack on the targets should be improvement in the farming system either quantitatively (yield) or qualitatively (improved nutrition). In other words, phase four should provide the means for continuing improvement of the system through research on specific problems.

In the Sahel it quickly becomes evident to the experienced observer that the natural vegetation has been nearly destroyed by a combination of misuse and drought. The ecosystem was resilient enough to tolerate either condition separately, at least for some years, but suffered severely when both conditions occurred at the same time. During a 10-year period of above-average annual rainfall prior to the drought, rowcrop farming of millet and sorghum was extended north into the grazing area. Vegetation was permanently removed by the combination of farming and overgrazing. The human population increased 25 percent, and livestock numbers went up 60 percent during this 10-year period. When the drought struck, the already heavily strained ecosystem virtually vanished. Rowcropping retreated beyond its normal position. Nomadic grazers then entered the abandoned farming areas but found little or no forage. Social conflict between nomadic grazers and sedentary farmers increased. Government officials spoke of "desertification" or "desert encroachment" as though the desert was aggressively forcing its way south, when in reality the desert was being pulled into the vacuum created by overuse of the resource base.

Overgrazing also resulted in changes in the botanical composition of pastures from perennial to annual grasses, which produce more seed. This, in turn, caused an increase in the quelea bird population. Normally this small and prolific seed-eater feeds on native grass seed, but overgrazing on the annual

grasses during the drought resulted in diminished grass seed crops. The birds then turned their attention to cultivated millet and sorghum.

Thus it is clear that systems devised by means of the procedures suggested in phases one to four should be better adapted to this fragile zone and less susceptible to the predictable variation in rainfall. This means that phase four activities should be devoted to problem-oriented research at experimental stations and in farmers' fields aimed at solving existing problems or improving the present system. Conventional research can determine whether changing natural conditions or human interventions will improve or degrade the existing system. New or complementary systems can be designed and tested.

Phase four research also should identify the appropriate role of existing institutions within each Sahelian country and region in supporting research activities as well as providing both formal and informal training. Depending on the adequacy of local and regional resources, connections with international institutions may be desirable to provide essential assistance in research and training.

Outputs of Phase Four

The outputs of phase four should include agronomic, ecologic, economic, social, institutional, and political recommendations, the synthesis and evaluation of improved millet-sorghum production systems, and the conceptualization of new millet-sorghum systems. Research organizations should investigate (1) new plant varieties, (2) cultural practices, (3) inputs and costs, (4) markets, (5) social impact, and (6) needed infrastructures. Feedback mechanisms should be created.

Appendix B

SECONDARY DATA ON FARMING SYSTEMS

It may be asked which farming systems should be studied. The answer will vary according to country, region, and problems faced. Using data obtained by the Food and Agriculture Organization (FAO) of the United Nations, Martin[1] has shown that several types of systems can be identified from existing worldwide data (Tables 1 and 2). Officials in individual countries may wish to compare one system with another.

WORLD FARMING REGIONS

What follows is an attempt to classify countries of the world into farming regions. The list does not include Europe, China, India, the U.S.S.R., Oceania, or the United States and Canada, which are considered to be six distinct regions. No country is listed in more than one region, although it would have been desirable to do so.

I

North African Wheat Region
Algeria, Libya, Morocco, and Tunisia.

II

African Millet-Sorghum Region
Botswana, Cameroon, Central African Federation, Chad, Ethiopia, Gambia, Mali, Mauritania, Niger, Nigeria, Rwanda, Senegal, Somalia, Sudan, Uganda, Upper Volta, and Zambia.

III

Guinean Zone of Africa--Rice, Corn, Roots, and Tubers
Congo, Dahomey, Equatorial Guinea, Gabon, Ghana, Guinea, Guinea-Bissau, Ivory Coast, Liberia, Madagascar, Mozambique, Sierra Leone, Togo, and Zaire.

IV

African Corn Region
Angola, Burundi, Kenya, Lesotho, Malawi, Namibia, Rhodesia, South Africa, Swaziland, and Tanzania.

V

Asian and Near Eastern Wheat Region
Afghanistan, Cyprus, Egypt, Iran, Iraq, Israel, Jordan, Lebanon, Mongolia, Pakistan, Syria, and Turkey.

VI

Asian Millet-Sorghum Region
North Korea, Saudi Arabia, Yemen Arab Republic, and Yemen Democratic Republic.

VII

Asian Rice Region
Bangladesh, Bjutan, Brunei, Burma, Indonesia, Japan, Khmer, Laos, Malaysia, Nepal, North Vietnam, Philippines, Sikkim, South Korea, South Vietnam, Sri Lanka, and Thailand.

VIII

Latin American Wheat and Livestock Region
Argentina, Chile, and Uruguay.

IX

Latin American Corn, Beans, and Root Vegetable Region
Bolivia, Brazil, Colombia, Ecuador, Paraguay, Peru, and Venezuela; Cuba, Guadeloupe, Haiti, and Jamaica; Belize, Costa Rica, El Salvador, Guatemala, Honduras, Mexico, Nicaragua, and Panama.

X

Latin American Sugar, Rice, and Livestock Region
Dominican Republic, French Guiana, Guyana, Martinique, Puerto Rico, Surinam, and Trinidad and Tobago.

Table 1. Characteristics of sixteen farming regions, 1973

	Farming region[a] or continent							
	I	II	III	IV	Africa	Europe	V	VI
Rice (000 ha)	5	776	2,513	150	3,444	404	2,716	371
Wheat (000 ha)	5,447	1,212	17	1,809	8,485	26,545	25,969	269
Corn (000 ha)	470	4,602	2,279	10,020	17,371	12,289	2,103	481
Sorghum (000 ha)	86	10,165	308	673	11,232	120	542	1,400
Millet (000 ha)	8	12,173	503	1,067	13,751	29	975	651
ALL CEREALS[b] (000 ha)	9,128	32,331	6,073	13,041	60,573	71,218	39,916	3,568
Roots and tubers[c] (000 ha)	82	4,857	3,499	1,906	10,344	6,712	362	210
Pulses[d] (000 ha)	721	7,386	744	1,996	10,847	4,517	3,030	423
Oilseeds[e] (000 ha)	8	4,558	785	7,001	6,352	230	101	410
Commercial crops[f] (000 ha)	100	5,551	4,587	2,091	12,329	4,178	5,113	94
17 Vegetables[g] (000 ha)	641	118	48	477	1,284	8,413	2,664	102
14 Fruits[h] (000 tons)	1,900	593	1,744	4,390	8,627	33,095	7,843	197
Population (000)	40,287	158,449	79,364	70,573	348,673	469,730	221,747	31,181

Digitized by Google

Arable land, land in permanent crops (000 ha)	21,428	112,314	34,706	37,702	206,150	144,296	93,139	4,351
Permanent meadow and pasture (000 ha)	53,316	361,440	198,910	226,092	791,758	90,661	197,814	101,115
Forests and woodlands (000 ha)	8,812	285,446	193,513	117,119	604,889	140,500	57,991	13,561
Irrigated land (000 ha)	740	1,125	388	947	3,200	13,703	28,387	276
Horses (000)	682	2,483	12	541	3,718	6,770	4,597	34
Donkeys (000)	1,527	7,026	59	652	9,264	1,646	8,789	856
Cattle (000,000)	5.3	82.8	17.5	41.2	146.8	129.3	42.4	2.4
Buffalo (000,000)	0.1	3.4			3.5	0.4	2.4	0.7
Sheep (000,000)	30.1	55.4	6.1	42.7	134.3	125.5	246.3	6.9
Goats (000,000)	12.8	71.3	8.8	19.6	112.5	11.7	152.9	10.8
RUMINANT ANIMAL UNITS[i] (000,000)	14.0	111.5	20.5	53.7	199.7	157.1	124.6	6.6
Pigs (000)	27	1,850	2,886	2,311	7,074	150,085	394	1,450
Chickens (000,000)	35.9	219.1	56.6	72.2	383.8	1,184.1	219.1	31.0
Total agricultural tractors[j] (000)	73.4	18.0	17.2	275.0	383.6	6,626.0	260.1	24.0

Table 1. Continued

	Farming region[a] or continent							
	China	India	VII	Asia	USSR	Oceania	VIII	IX
Rice (000 ha)	34,755	37,000	47,556	122,398	462	60	131	6,075
Wheat (000 ha)	28,701	19,881	708	75,528	63,155	9,199	4,719	2,814
Corn (000 ha)	10,634	6,000	7,441	26,659	4,031	81	3,877	22,828
Sorghum (000 ha)	4	17,400	100	19,446	90	710	2,416	1,514
Millet (000 ha)	29,804	19,000	340	50,770	2,850	40	198	290
ALL CEREALS[b] (000 ha)	120,649	101,687	56,790	322,610	120,652	13,534	13,219	34,442
Roots and tubers[c] (000 ha)	15,585	1,116	3,975	21,248	8,017	187	291	4,019
Pulses[d] (000 ha)	8,823	20,625	2,217	35,118	6,083	110	222	6,851
Oilseeds[e] (000 ha)	16,509	7,360	2,311	26,691	851	66	536	4,306
Commercial crops[f] (000 ha)	6,320	11,814	4,210	27,551	6,551	832	917	14,876
17 Vegetables[g] (000 ha)	300	86	1,630	4,782	2,051	136	667	761
14 Fruits[h] (000 tons)	3,499	1,890	25,997	39,426	60	1,755	2,965	30,791
Population (000)	814,276	575,380	555,949	2,198,533	249,835	20,560	38,808	258,090

Digitized by Google

Arable land, land in permanent crops (000 ha)	127,000	165,680	94,323	484,493	232,431	46,807	32,511	90,617
Permanent meadow and pasture (000 ha)	200,000	13,000	23,237	535,167	375,300	468,123	169,758	292,911
Forests and woodlands (000 ha)	118,000	65,930	312,475	567,957	910,009	84,892	84,000	822,513
Irrigated land (000 ha)	76,000	31,290	18,074	154,027	11,100	1,784	2,698	5,778
Horses (000)	7,000	950	1,629	14,210	7,075	585	3,880	19,243
Donkeys (000)	11,600	980	21	21,746	540	5	106	8,297
Cattle (000,000)	63.3	176.9	65.0	350.0	104.0	38.7	67.9	176.4
Buffalo (000,000)		1.1	0.3	4.5	0.2			
Sheep (000,000)	72.0	43.3	6.3	374.8	139.1	209.8	63.9	45.6
Goats (000,000)	58.7	69.2	24.1	315.7	5.6	0.2	6.0	32.5
RUMINANT ANIMAL UNITS[i] (000,000)	89.4	200.5	71.4	492.6	133.1	80.7	81.9	192.0
Pigs (000)	235,831	4,780	43,264	285,719	66,593	4,058	5,880	61,229
Chickens (000,000)	1,230.3	117.7	646.4	2,244.5	671.1	37.5	55.6	554.2
Total agricultural tractors[j] (000)	160	67	329.1	840.2	2,112	435	248.5	440.8

Table 1. Continued

	Farming region[a] or continent			
	X	Latin America	Canada & U.S.	World
Rice (000 ha)	235	6,441	878	134,087
Wheat (000 ha)		7,533	31,822	222,267
Corn (000 ha)	31	26,736	25,513	112,680
Sorghum (000 ha)		3,930	5,451	40,979
Millet (000 ha)		488		67,928
ALL CEREALS[b] (000 ha)	266	47,927	83,921	720,435
Roots and tubers[c] (000 ha)	55	4,365	686	51,559
Pulses[d] (000 ha)	64	7,137	777	64,589
Oilseeds[e] (000 ha)	71	4,913	23,625	62,728
Commercial crops[f] (000 ha)	636	16,429	6,089	73,959
17 Vegetables[g] (000 ha)	8	1,436	1,243	19,345
14 Fruits[h] (000 tons)	767	34,523	19,577	137,063
Population (000)	10,299	307,197	233,060	3,827,588
Arable land, land in permanent crops (000 ha)	2,234	125,362	234,820	1,474,359
Permanent meadow and pasture (000 ha)	4,226	466,895	269,173	2,997,077
Forests and woodlands (000 ha)	39,608	946,121	735,551	3,989,919
Irrigated land (000 ha)	293	8,769	16,253	208,836
Horses (000)	197	23,320	8,845	64,523
Donkeys (000)	171	8,574	4	41,779

Cattle (000,000)	2.6	246.9	134.2	1,149.9
Buffalo (000,000)				8.6
Sheep (000,000)	0.3	109.8	18.3	1,111.6
Goats (000,000)	1.0	39.5	1.6	486.8
RUMINANT ANIMAL UNITS[i] (000,000)	2.8	276.7	138.2	1,478.1
Pigs (000)	1,736	68,845	66,483	648,887
Chickens (000,000)	27.5	637.3	500.9	5,659.2
Total agricultural tractors[j] (000)	17.3	706.6	5,034	16,137.4

[a]See Appendix B for the country makeup of the numbered regions.
[b]Besides those listed above, "All Cereals" includes rye, barley, oats, mixed grain, unspecified millet and sorghum, buckwheat, and miscellaneous cereals.
[c]Potatoes, sweet potatoes and yams, cassava, and other starchy roots.
[d]Dry beans, dry peas, dry broad beans, chickpeas, lentils, pigeon peas, cowpeas, vetch, lupine, and other pulses.
[e]Soybeans and groundnuts.
[f]Seed cotton, cocoa beans, green coffee, sugarcane, sugar beets, tea, coffee, and jute.
[g]Cabbage, tomatoes, cauliflower, squash and pumpkins, cucumbers, eggplant, chillies, green onions, dry onions, garlic, string beans, green beans, green peas, carrots, watermelons, other melons, and grapes (including wine grapes).
[h]Apples, pears, peaches, plums, apricots, cherries, oranges, tangerines, lemons and limes, grapefruit, figs, mangoes, pineapples, and bananas.
[i]Cattle and buffalo and one-fifth (sheep plus goats).
[j]Crawler, wheel, and garden tractors.

Source: FAO (1974b).

Digitized by Google

Table 2. Relative characteristics of sixteen farming regions, 1973

	Farming region[a] or continent							
	I	II	III	IV	Africa	Europe	V	VI
Rice (ha per 1,000 people)	0.1	4.9	31.7	2.1	9.9	0.9	12.2	11.9
Wheat (ha per 1,000 people)	133.4	7.6	0.2	25.6	24.3	56.6	117.1	8.6
Corn (ha per 1,000 people)	11.5	29.0	28.7	142.0	49.8	26.2	9.5	15.4
Sorghum (ha per 1,000 people)	2.1	64.2	3.9	9.7	32.2	0.3	2.4	44.9
Millet (ha per 1,000 people)	0.2	76.8	6.3	15.1	39.4	0.1	4.4	20.9
ALL CEREALS[b]	223.5	204.0	76.5	184.8	173.7	151.6	180.0	114.4
Roots and tubers[c]	2.0	30.6	44.1	27.0	29.7	14.3	1.6	6.7
Pulses[d]	17.6	46.6	9.4	28.3	31.1	9.6	13.7	13.6
Oilseeds[e]	0.2	28.8	9.9	99.2	18.2	0.5	0.5	13.2
Commercial crops[f]	2.4	35.0	57.8	29.6	35.4	8.9	23.1	3.0
Vegetables[g]	15.7	0.7	0.6	6.8	3.7	17.9	12.0	3.3
Arable land, land in permanent crops	524.6	708.8	437.3	534.2	591.2	307.2	420.0	139.5
Permanent meadow and pasture	1,305.3	2,281.1	1,876.3	3,203.6	2,270.8	193.0	892.1	3,242.8
Forests and woodlands	215.4	1,801.5	2,438.3	1,659.5	1,734.8	299.1	261.5	434.9
Irrigated land	18.2	7.1	4.9	13.4	9.2	29.2	128.0	8.9

Ruminant animal units[h] (per 1,000 people)	342.8	703.7	258.3	760.9	572.7	334.4	561.9	211.7
Pigs (per 1,000 people)	0.7	11.7	36.4	32.8	20.3	319.5	1.8	46.5
Chickens (per 1,000 people)	878.9	1,382.8	713.2	1,023.0	1,100.7	2,520.8	988.1	994.2
Kilograms of fruit[i] per 1,000 people	46,516	15,671	21,975	62,205	24,742	70,455	35,369	6,318
Horses (per 1,000 ha of arable land)	31.8	22.1	0.4	14.3	18.0	46.9	49.4	7.8
Donkeys (per 1,000 ha of arable land)	71.3	62.6	1.7	17.3	44.9	11.4	89.0	196.7
Tractors[j] (per 1,000 ha of arable land)	3.4	0.2	0.5	7.3	1.9	45.9	2.8	5.5
Number of ruminant animal units[h] per hectare of permanent meadow and pasture	0.3	0.3	0.1	0.2	0.3	1.7	0.6	0.1
Average size of holding in hectares[k]	8.9	4.2	1.8	18.5	5.0	11.0	3.6	n.a.

Table 2. Continued

	Farming region[a] or continent							
	China	India	VII	Asia	USSR	Oceania	VIII	IX
Rice (ha per 1,000 people)	42.7	64.3	85.5	55.7	1.8	2.9	3.4	23.5
Wheat (ha per 1,000 people)	35.2	34.6	1.3	34.4	252.8	447.4	121.6	10.9
Corn (ha per 1,000 people)	13.1	10.4	13.4	12.1	16.1	3.9	99.9	88.4
Sorghum (ha per 1,000 people)		30.2	0.2	8.8	0.4	34.5	62.3	5.9
Millet (ha per 1,000 people)	36.6	33.0	0.6	23.1	11.4	2.0	5.1	1.1
ALL CEREALS	148.2	176.7	102.2	146.7	482.9	658.3	340.6	133.4
Roots and tubers[c]	19.1	1.9	7.2	9.7	32.1	9.1	7.5	15.6
Pulses[d]	10.8	35.8	4.0	16.0	24.4	5.4	5.7	26.6
Oilseeds[e]	20.3	12.8	4.2	12.1	3.4	3.2	13.8	16.7
Commercial crops[f]	7.8	20.5	7.6	12.5	26.2	40.5	23.6	57.6
Vegetables[g]	0.4	0.2	2.9	2.2	8.2	6.6	17.2	3.0
Arable land, land in permanent crops	156.0	287.9	169.7	220.4	930.3	2,276.6	837.7	351.1
Permanent meadow and pasture	245.6	22.6	41.8	243.4	1,502.2	22,768.6	4,374.3	1,134.9
Forests and woodlands	144.9	114.6	562.1	258.3	3,642.4	4,129.0	2,164.0	3,186.9
Irrigated land	93.3	54.4	32.5	70.1	44.4	86.8	69.5	22.4

Digitized by Google

Ruminant animal units[h] (per 1,000 people)	109.8	348.5	128.4	224.1	532.8	3,925.1	2,110.4	743.9
Pigs (per 1,000 people)	289.6	8.3	77.8	130.0	266.6	197.4	156.5	237.2
Chickens (per 1,000 people)	1,510.9	204.6	1,162.7	1,020.9	2,686.2	1,823.9	1,432.7	2,147.3
Kilograms of fruit[i] per 1,000 people	4,297	3,785	46,761	17,933	240	85,360	76,402	119,303
Horses (per 1,000 ha of arable land)	55.1	5.7	17.3	29.3	30.4	12.5	119.3	212.4
Donkeys (per 1,000 ha of arable land)	91.3	5.9	0.2	44.9	2.3	0.1	3.3	91.6
Tractors[j] (per 1,000 ha of arable land)	1.2	0.4	3.5	1.7	9.1	9.3	7.6	4.9
Number of ruminant animal units[h] per hectare of permanent meadow and pasture	0.4	15.4	3.1	0.9	0.4	0.2	0.5	0.7
Average size of holding in hectares[k]	n.a.	2.5	1.4	2.4	n.a.	1,303	104	25.0

Table 2. Continued

	Farming region[a] or continent			
	X	Latin America	Canada & U.S.	World
Rice (ha per 1,000 people)	22.8	21.0	3.8	35.0
Wheat (ha per 1,000 people)		24.5	136.5	58.1
Corn (ha per 1,000 people)	3.0	87.0	109.5	29.4
Sorghum (ha per 1,000 people)		12.8	23.4	10.7
Millets (ha per 1,000 people)		1.6		17.8
ALL CEREALS	25.8	156.0	360.1	188.2
Roots and tubers[c]	5.3	14.2	2.9	13.5
Pulses[d]	6.2	23.2	3.3	16.9
Oilseeds[e]	6.9	16.0	101.4	16.4
Commercial crops[f]	61.8	53.5	26.1	19.3
Vegetables[g]	0.8	4.7	5.3	5.0
Arable land, land in permanent crops	216.9	408.1	1,007.6	385.2
Permanent meadow and pasture	410.3	1,519.9	1,154.9	783.0
Forests and woodlands	3,845.8	3,079.8	3,156.1	1,042.4
Irrigated land	28.4	28.6	69.7	54.6
Ruminant animal units[h] (per 1,000 people)	271.9	900.7	593.0	386.2
Pigs (per 1,000 people	168.6	224.1	285.3	169.5
Chickens (per 1,000 people)	2,671.2	2,074.6	2,149.2	1,485.5
Kilograms of fruit[i] per 1,000 people	74,473	112,381	84,000	35,809

Digitized by Google

Horses (per 1,000 ha of arable land)	88.2	186.0	37.7	43.8
Donkeys (per 1,000 ha of arable land)	76.5	68.4	0.2	28.3
Tractors[j] (per 1,000 ha of arable land)	7.7	5.6	21.4	10.9
Number of ruminant animal units[h] per hectare of permanent meadow and pasture	0.7	0.6	0.5	0.49
Average size of holding in hectares[k]	8.9	30.7	161	12.7

[a]See Appendix B for the country makeup of the numbered regions.
[b]Besides those listed above, "All Cereals" includes rye, barley, oats, mixed grain, unspecified millet and sorghum, buckwheat, and miscellaneous cereals.
[c]Potatoes, sweet potatoes and yams, cassava, and other starchy roots.
[d]Dry beans, dry peas, dry broad beans, chickpeas, lentils, pigeon peas, cowpeas, vetch, lupine, and other pulses.
[e]Soybeans and ground nuts.
[f]Seed cotton, cocoa beans, green coffee, sugarcane, sugar beets, tea, coffee, and jute.
[g]Cabbage, tomatoes, cauliflower, squash and pumpkins, cucumbers, eggplant, chillies, green onions, dry onions, garlic, string beans, green beans, green peas, carrots, watermelons and other melons, and grapes (including wine grapes).
[h]Cattle and buffalo plus one-fifth (sheep plus goats).
[i]Apples, pears, peaches, plums, apricots, cherries, oranges, tangerines, lemons and limes, grapefruit, figs, mangoes, pineapples, and bananas.
[j]Crawler, wheel, and garden tractors.
[k]Computed from Table 2, Production Yearbook, 1973 (FAO 1974b). The numbers of landless holdings were eliminated. Data were not available for nearly as many countries as for the production data. Presumably the areas of holdings shown in Table 2 include other than arable land, although this is not perfectly clear. Thus the average shown here is an overestimate of the average area of arable land available in each farming region and continent.

Source: FAO (1974b).

Digitized by Google

Profile 2

ECONOMIC AND SOCIOINSTITUTIONAL ENVIRONMENTS AFFECTING AGRICULTURAL PRODUCTION

All economic activity, whether for production or consumption, is channeled in one direction or another by the surrounding natural environment, the technological environment, the economic environment of prices and markets, and the socioinstitutional environment of family, class, and governmental structures. Thus in order to gain an understanding of production and consumption activities within a given geographical area for the purpose of suggesting methods to increase either production or consumption, the following procedure would be adopted. First, the present situation would be described in terms of each of the four environments listed above. Then one would project empirically what would happen in the future without purposeful change in any of the environments. (Evolutionary change also would be examined as part of the analysis.) Finally, purposeful (planned) changes in the various environments would be posited, and the effects of both the "natural" evolutionary system and the new planned system on production and consumption would be evaluated and compared.

Profile 1 recognized the environmental relationships and proposed a methodological procedure for: (1) developing a description of an existing agricultural system, (2) suggesting purposeful changes, (3) investigating the scientific and technological requirements for accomplishing these purposeful changes, (4) determining the expected impact of these changes, and (5) monitoring and assessing the actual effects of these changes after their adoption in farming areas.

Most of the research profiles that follow focus specifically on the possibilities of modifying the natural and technological environments. These profiles are the current "best guesses" on the prospects for productive purposeful change in individual components, given the present state of knowledge about current and

projected farming systems. These research profiles in general require extensive biological and/or physical research in order to ascertain whether changes are possible. In each case, however, it is recognized that in order for a physical or biological possibility to be adopted within a given farming system, and for this adoption to increase either agricultural production or household nutrition, the proper combination of the other environmental conditions must be present.

Thus for each of the profiles that follows, research is implied on the economic and socioinstitutional environments that affect a particular farming system. This research profile concentrates on researchable issues that arise in the economic and socioinstitutional environments.

RESEARCH PROBLEM AREAS

The Price-Market Environment

This area of research overlaps that of Study Team 6 (Food Availability to Consumers) and Study Team 7 (Rural Institutions, Policies, and Social Science Research), and is actually a subdivision of the discussion on the socioinstitutional environment that follows. The prices of agricultural products are determined in the markets where these products are sold, rather than at the farm. Similarly, the prices of agricultural inputs are determined in the markets where inputs are sold, rather than at the farm. It is the socioinstitutional environment that establishes the preconditions within which input and output markets operate and prices fluctuate.

Yet scrutinizing the economics of resource use in agriculture is worthwhile. This area of economics chiefly concerns farm and ranch management, whose problems are much better understood than the problems of the socioinstitutional environment. The economics of resource use problems essentially take the socioinstitutional environment as given while asking the question: What is the effect of a given technological or management strategy on economic efficiency?

For example, in the context of the succeeding profiles on agricultural resources, there are several researchable questions. First, for each of the technological and scientific possibilities related to soil, water, machinery, and fertilizer, within the context of a given geographical area, and current production practices and output and input prices one should evaluate:

-- The net short-run economic advantage to the representative farm decision-making unit (in most cases the household) generated by the adoption of new knowledge or technology. (Net economic advantage includes household food consumption as well as the sale of products to others.)
-- The net long-run economic advantage to the representative farm decision-making unit stemming from the adoption of new knowledge or technology, assuming that farm size remains constant. That is, will positive changes in net income continue over the long run or be only transitory?
-- The implications of the adoption of new knowledge or technology on the long-term distribution of labor use and labor income on representative farms.
-- The implications of the adoption of new knowledge or technology on farm size in the long run.
-- The aggregate long-run effects on net farm income, labor use, labor income, and farm size.

Second, assuming that certain aggregative effects will be discovered through the above studies, the aggregative effects on various output and input prices, including the prices of land, water, fertilizer, machinery, and labor should be estimated. Since input and output price ratios are likely to change, the above analyses will have to be revised periodically. The advantages accruing from the adoption of new knowledge or technology may be either more or less than originally estimated if such changes in price ratios occur.

All of this suggested research ultimately focuses on improving production and consumption advantages at the household level. While other economic objectives could be suggested, improvement in food production and human nutrition at the household level is taken to be the ultimate objective of the World Food and Nutrition Study.

In fact, there are economic studies which should be evaluated under this same objective. For example, one research profile offered in this report, "Factors that Determine the Supply, Price, Distribution, and Use of Fertilizer," is essentially economic research, and the ultimate question is whether the development of a fertilizer forecasting system would help to increase on-farm production and/or household consumption within various geographic areas, and if so, by how much.

The Socioinstitutional Environment

This area of research is much wider than Study Team 7's "Rural Institutions, Policies, and Social Science

Research" where the word "institutions" is used in a narrower sense. As mentioned earlier, the socioinstitutional environment sets the preconditions that influence the economic environment. It is only within the socioinstitutional environment that the economic, or price-market, environment functions. All of the answers to the economic efficiency research questions posed earlier depend on the socioinstitutional environment. Thus a biological or technological development could prove economically advantageous in one socioinstitutional environment and not in another, even though the natural environments were identical. This concept suggests further areas of research.

What is the socioinstitutional environment? It is the whole spectrum of laws, administrative and management rules, and social customs that determine who can do what to whom and what defenses "whom" has against "who" in relation to agricultural resources and in sharing the products derived from use of those resources.

Within this spectrum of laws, rules, and customs, we may identify the concepts of "rights and duties," "liberties and exposures," "security of expectations," "flexibility and transferability," "reversibility," and "rigidity." Empirical documentation of these concepts as they affect water, land, and other agricultural resource use, as well as how these concepts affect the distribution of products, are the basic problems to be studied.

Rights and Duties; Liberties and Exposures

There are institutions that define agricultural property rights (e.g., water rights, land tenure rights), there are institutions that empower public agencies to administer the laws pertaining to these rights, and there are institutions that empower public organizations or private entities (corporations, businesses, individual producers, or consumers) to manage the resources themselves (extract, divert, store, use in production or consumption, and so forth) within the laws of property rights as administered by the empowering agencies. In addition, social custom at the household level may be as effective as formal law.

It is necessary, however, to know more than what the law or custom says are the rights, duties, liberties, and exposures of the relevant parties. It also is necessary to determine what administrative organizations and the courts have said they are. Thus administrative regulations and precedents and court decisions and precedents, in addition to legislation,

also must be determined and incorporated into any description of resource-related institutions.

Security of Expectations

In addition to describing empirically the rights, duties, liberties, and exposures defined by institutions and customs, the security of expectations felt by those affected as to the definability and constancy of their mutually reciprocal rights, duties, liberties, and exposures, their relations to agricultural resources and the products derived from these resources, and the "riskiness" with which people view future gains from currently legal acts due to feelings of institutional uncertainty, must be determined. It is quite usual to encounter more doubt about who really has power within a system than a cursory description of the system might suggest.

Flexibilities

The flexibility permitted by institutions and customs regarding the allocation and reallocation of agricultural resources and products among uses and users (i.e., the degree of transferability permitted in resource use and development) also must be determined.

Reversibility

As a corollary to flexibility, one must determine the degree to which institutions incorporate provisions for the reversal of allocations of resources among uses and/or users, particularly in regard to recovering their productivity or quality, reducing the capital intensity of their development, or permitting persons adversely affected by existing allocations to reattain a previous condition of well-being. Such reversibility, if found, will be in contrast to the usual condition, which is that allocations are flexible only in the direction of greater intensity.

Rigidity

One also must attempt to determine the rigidity, meaning the degree of "evolutionary fluidity," in the institutions themselves, that is, their susceptibility to change in form or conduct, with particular attention to the opportunities for and constraints on purposeful action to generate changes within these institutions.

Categorization of agricultural resource institutions must be empirical. What must be determined are the existing right-duty, liberty-exposure relations as defined in the institutions, together with their security, flexibility, reversibility, and rigidity.

These empirical findings should be incorporated in analytic economic models of agricultural systems in order to predict what will occur if nothing purposeful is done to change agricultural prices, markets, or socioinstitutional arrangements.

The final "payoff" step is to posit an array of practicable, purposeful changes in the socioinstitutional environment, that is, in institutions that supervise water, land, and related resources, in institutions that regulate markets and prices, and in institutions designed to increase the knowledge of resource users.

Each of these posited changes must, in turn, be incorporated into analytical models to determine its probable impact on future growth and well-being in the region. The purpose of this exercise is to provide policymakers with an array of options when they face the task of making choices in the light of political realities and the people's preferences.

A desirable addendum but one probably beyond our capabilities will be to assess the political feasibility for realistic change in institutions and practices as contained in each of our posited array of changes. Doing so would provide an additional dimension to our assistance to policymakers in their onerous task of making hard choices.

IMPACT

Two types of impact will flow from research into the problems listed above. The first is the positive impact measured in increased production and/or consumption. The second, equally important, is that of avoiding undesirable side effects (externalities) of resource development.

Increased Production and/or Consumption

The pervasiveness of economic and socioinstitutional issues suggests that analysis of problems in these areas is a prerequisite to improvements stemming from any application of new technology in the developing countries. In other words, virtually any new technology runs into barriers at the household and institutional levels. These barriers must be pushed aside if adoption of new

technology is to occur. In fact, the technolcgical, economic, and socioinstitutional environments are so closely intertwined that it is quite artificial to examine them separately. Both economic and socioinstitutional analysis form an integral part cf each prospective mechanism for increasing food and fiber production anywhere in the world.

Thus no specific monetary or index number estimates of impacts are made for these economic and socioinstitutional analyses. Wherever a monetary or index number estimate of increases in production and/or consumption is given in a technologically-oriented investigation, the chance of that estimate being realized is slight unless it is accompanied by economic and socioinstitutional research. But if technological progress can be made and combined with the elimination of economic and socioinstitutional barriers, nutritional improvements could be immense.

Avoiding Undesirable Impacts

As briefly discussed earlier, economic and socioinstitutional analyses should take place within the context of a given geographical area, and therefore within the farming system implied by the surrounding natural, technological, economic, and socioinstitutional environments. There is no such thing as the "economics of land" or the "economics of water." Economics does not begin to have relevance until the land or water is introduced into some production or consumption activity in combination with other inputs and within a socioinstitutional context. The same is true for the other social sciences, such as anthropology, sociology, or political science.

For example, the subgroup on land and water suggests research on agricultural soil fertility. Such research has no economic content until it is put into the context of a particular farming area where the possible benefits of that research may be evaluated relative to its cost and the economic and social costs that may be associated with its adoption. Such costs and benefits may be evaluated only within the context of alternative combinations of farming and socioinstitutional systems.

Similarly, hydrological studies may contribute to understanding the alternatives in the design and management of agricultural irrigation systems. But the economist has little to say about irrigation water until possible alternatives for the design and management of such systems have been determined within the context of a specific agricultural area. Once given alternative construction costs, economists and

other social scientists can then examine the trade-offs among all the economic and social costs and benefits.

Thus economists and other social scientists must play a role in the effort to relieve world hunger. Their most useful role will be to help evaluate whole systems rather than simply the costs and benefits of a single technological change related to a single resource, such as land or water, within a rigidly restrained system. In the past, economists and other social scientists usually have restricted their work much too narrowly. This narrow focus has contributed greatly to the agricultural and social problems of the United States up to the current time. Economists have generally concentrated on issues of economic efficiency while neglecting questions of welfare and distribution. The latter questions are more difficult to examine but are of crucial importance in making adjustments when a new technique is adopted.

For example, agricultural economists in the United States have concentrated on developing techniques that decrease the costs of production for a given level of output or, what is the same thing, techniques that raise output from a given level of resource use. With reference to water, agricultural economists have contributed toward the economically efficient management and use of irrigation techniques. The result has been increased food production for a given set of resources, the very goal of this study. At the same time, however, agricultural economists (and others) have failed to study the social implications of technological advances on the farm population. The unanticipated result (in the context of our free enterprise, economically competitive socioinstitutional environment) was tremendous growth in farm size, great diminution in farm numbers, and the movement of most of the farm population to the cities, an outcome that created enormous new social problems.

There is a good deal of evidence which suggests that U.S. efforts to increase food production in the developing countries are generally following the pattern described above. That is, great emphasis is being placed on research to increase the efficiency of food production, with not enough thought being given to the possible welfare and distributional effects on the total economy of the nations in question. In India, for example, some 90 percent of the total population lives in rural areas, most of them on farms. Virtually every analysis of the farm-food situation states that if Indian farmers were provided with adequate economic incentives--say, a large technological advance or a guarantee of stable net income--they would respond as American farmers have, and India would be on its way to a satisfactory level of development.

This may be so. But, after the United States was totally populated (that is, after there was no more undeveloped land on the frontier) further agricultural improvements relied on the coercive powers of pure economic competition in combination with economies of size. These developments forced marginal farmers out of business, and their lands were absorbed by their larger and more affluent neighbors. The fact that these forces would inevitably lead to this result within the context of the socioinstitutional environment was not well understood until the early 1960s when it was far too late to reverse the process even if the nation had wanted to.

As yet we have no real idea of what the long-run effects of technological development will be in India within its socioinstitutional environment. Will its population be forced off the farms? We do not know. But if such a move does occur, it would appear to be highly undesirable. Economic and socioinstitutional research is desperately needed to foresee and suggest ways to avoid such undesirable impacts.

It is natural for U.S. economists to undertake economic analysis of emerging agricultural resource problems in other nations by applying conventional economic wisdom. In so doing these economists would tend to presuppose (1) constant nonentropic natural systems (entropy = zero), (2) an existing superstructure of socio-political-economic institutional environments (or worse, an idealized socioinstitutional environment of perfect competition and perfect markets for resources in situ, for extracted resources, and for material goods and services produced from resources), (3) the present structure of relative preferences for resource-derived goods and services among consumers, as revealed by their reactions to current prices for such resource services, (4) some projected rate of technological change, (5) some projected rate of population growth, and (6) an objective function to maximize a unidimensional set of welfare goals, such as the gross national product.

But if economists were to follow this conventional economic analytic wisdom, they would be of little help to policymakers in foreign nations. They would have assumed the disappearance of the very conditions that generated requests for analytical help, and would in fact be adopting the implicit premise that whatever exists is both good and right, subject only to considerations about the adequacy of the knowledge possessed and applied by resource managers, public or private. Economists would in effect be saying to policymakers that all they need do is ensure that the channels of communication are open and functioning

adequately so that resource managers can be helped to "fine tune" their views as to the most appropriate input-output mix of the natural resources they control, and all would be well.

Many social scientists now recognize, however, that most resource policy problems are cases in which the resource manager does not generate the greatest possible public benefit even though the manager may be balancing inputs and outputs in the most productive way. This being the case, educating resource managers in new technologies will not solve a problem because its source does not lie in the manager's incomplete knowledge. The source of the problem will be found in (1) the socioinstitutional structure that surrounds resource decision making behavior, and (2) the public's goals and preferences in relation to the goods, services, and amenities that can be derived from the resource's use. Thus the problem shifts from the resource manager's shortcomings in knowledge to the inadequacies in the socioinstitutional environment and the preferences-goals structure of the surrounding society. The problem shifts from conventional economic wisdom to political economy, law, and social psychology.

While economists and other social scientists can be useful in evaluating microadjustment and microtechnological alternatives at the farm systems level, their greatest potential is in examining the implications and goal preferences of the socioinstitutional environment within which the farming systems operate. Such study is broader than, say, land and water policy. In relation to India, for example, social scientists might suggest that as long as the United States and other countries have a highly restricted immigration policy, as long as U.S. advisers, presupposing a competitive environment, recommend American-style agricultural development, and as long as the Indian government attempts to negate the effects of economic competition by supporting socialist measures, at least some of the standard preconditions for economic development do not hold in India. Major advances in technical knowledge on the use of land, water, and other agricultural resources will be "nice" and may be adopted in some areas, but they will not solve the overall population-food imbalance in India in the near future. In fact, where adoption does take place, the side effects could be so disruptive that adoption would be seen as undesirable. This profile proposes a way to understand the balance between desirable and undesirable effects. The benefits of avoiding undesirable effects could be very large.

IMPLEMENTATION

Multidisciplinary Teams

It is recommended that the United States establish highly skilled multidisciplinary teams to study and model the systems made up of the natural, technological, economic, and socioinstitutional environments in which agricultural development must take place. Such teams would of necessity work with and train their disciplinary counterparts from the areas under study. These teams would work with, and with the approval of, the governments in question.

Modeling such systems is a demanding job. The policy implications of such models will not always be immediately acceptable to the decision makers, and may never be. Or they may become acceptable over a long period of study, through revision of policy recommendations and interactions between analysts and decision makers.

The cost of making significant research contributions to the economic-socioinstitutional environment is estimated at $3 to $5 million per year per study region. An expenditure of this magnitude can be expected to provide significant impetus toward desired changes. The time required for such accomplishment is seen as two to five years per region, with some acceleration occurring due to accumulated experience and research results.

Preconditions for Multidisciplinary Teams

Action on the part of U.S. agencies requires the involvement of American universities. Universities have the needed labor pool. Yet, just as the socioinstitutional preconditions for agricultural development may not be present in some of the developing countries, the socioinstitutional preconditions for forming highly skilled multidisciplinary teams are not present at most universities.

While increased funding is obviously implied, such funding is not the crucial issue. The issue is the reward system of the universities, that is, how excellence is to be defined. Excellence currently is widely defined in terms of disciplinary analysis, the results of which are published in the leading disciplinary journals. Multidisciplinary work in problem solving, on the other hand, is not always highly regarded, and the results of such work do not appear in the journals. Further, members of problem-oriented type institutes (e.g., a "water center" or

"food institute") often are not as highly recognized as members of disciplinary departments, even when the work of institute members clearly is useful for problem solving. Promotion and tenure committees at the universities are seldom impressed by multiple authorship.

Thus young professors may not be working in their own best interests if they devote themselves to multidisciplinary research to solve problems. Frequently, it is rational to wait until they are promoted to a professorial positon with tenure before undertaking multidisciplinary work. This absence of early encouragement is present both at prestigious privately funded universities, where promotion and tenure policies are combined with the institute problem, and at land-grant and other state universities, where the above problems are compounded by the belief that state-supported educational and research institutions should work on state-related problems.

These problems can only be solved by modification of the preferences-goals-reward structure of the universities. U.S. government agencies and private foundations dealing with international agricultural development must help convince the public that state institutions should participate in such work, and university employees at every level, from president to undergraduate, must learn to recognize the value of such work and reward such work accordingly.

Until educational and research institutions within the United States truly believe in supporting multidisciplinary research and solving the problems of the developing countries, they will contribute relatively little toward the world's need for increased food production and better nutrition. We must change our socioinstitutions as necessary preconditions for effective research before we can expect the developing countries to modify their socioinstitutions and preferences-goals structures as necessary preconditions for agricultural development.

Report of Subgroup B, Study Team 4

LAND AND WATER

INTRODUCTION

Water covers about 71 percent of the earth, and land 29 percent. Slightly more than one-tenth of the land is covered with permanent snow and ice, leaving an ice-free surface of 13 to 14 billion hectares (J. M. Goldberg et al. 1966, unpublished data, U.S. Geological Survey Open File Report; Buringh et al. 1975).

LAND

In 1967, as part of a world food study, a fairly comprehensive attempt was made to determine the extent of potentially arable soils in the world (President's Science Advisory Committee [PSAC] 1967). The then somewhat surprising result was that 24 percent of the ice-free land surface was physically suitable for cropping. This was notably higher than earlier estimates. Kovda (1971), a Soviet authority, states that the proportion of the earth's land under cultivation could be expanded relatively easily from about 10 percent to 16 to 17 percent, and adds that "if marshlands were drained and arid land irrigated the percentage could be raised to 25-30 percent." In 1974, perhaps the most comprehensive study to date was done by a team of scientists in the Netherlands (Buringh et al. 1975). Their conclusion was that 25 percent of the ice-free land surface is potentially arable. In short, as more has been learned about the soils of the world and how to use them, estimates of potentially arable land have moved upward.

Between 10 and 11 percent of the ice-free land surface is now under cultivation (FAO 1974b), about 41 percent of the total available as estimated by Buringh and associates. The potentially arable land is not evenly distributed. There is none in the Arctic, not much in the Subarctic, and the deserts would provide less than 10 percent even if it were assumed (quite unrealistically) that water could be brought to all desert areas containing potentially useful soils (Orvedal and Ackerson 1972). The temperate and

tropical regions contain the largest blocks of potentially arable land. Unfortunately, the distribution of this land does not match the distribution of people very well (Table 3). Asia, with 58 percent of the world's population, has only 20 percent of the world's arable land, of which 77 percent is cultivated already. In contrast, South America, with 5.4 percent of the world's population, has 17.5 percent of the arable land, only 13 percent of which is now cultivated.

North and Central America and the Caribbean Islands are estimated to have 353 million hectares physically suited for cropping that are not cropped today (Buringh et al. 1975). (This is 126 million more hectares than estimated in the FSAC study.) Most of this uncultivated land is used for forestry or grazing, or a combination of the two. While not wasted, it is used less intensively than it could be. The same is true of most uncropped but potentially arable land on other continents.

In the southeastern United States, much of the uncropped land was at one time cleared and used for growing cotton and corn but is now again in forest. Yields on these "abandoned" soils were low under the farming system common until 30 or 40 years ago, a kind of shifting cultivation. Accelerated soil erosion, not limited to the Southeast, was so widespread that it was called a national menace (Bennett and Chapline 1928). Today many, perhaps most, of the sloping soils of the Southeast are producing wood for pulp and lumber and forage for grazing, important but less intensive uses than cropping. In the meantime, they are being conserved.

The truly huge uncropped reserves of land are in South America and Africa, each of which has more than 500 million potentially arable hectares. Some of this, particularly in Africa, is cropped in a system of shifting cultivation, but most of the time the land is in forest, brush, or savanna. The potentially arable land in the Amazon Basin of South America and the Congo Basin in Africa equals in aggregate area somewhat more than one-half of the coterminous United States (Buringh et al. 1975). These basins are sparsely populated, with less than 3 percent of the land under cultivation. The dominant soils are oxisols and ultisols. Both classes of soils are low in fertility by temperate region standards, and much remains to be learned about how to establish sustained, high-level production on them, but enough is known to justify optimism. Oxisols and most ultisols will grow plants every day of the year if water and nutrients are available. Fertilizers and lime are needed, but knowledge about the proper

Table 3. Potentially arable land and cultivated land, by continents

Continent	Total area (million ha)	Potentially arable land (percent)	Cultivated land (percent)	Cultivated land (million ha)	Uncultivated but potentially arable land (million ha)	Total of cultivated and potentially arable land (million ha)	Total arable land per capita[d] (ha)
North America[a]	2,420[b]	25.9	11.3	273.4	353.3[c]	626.7	1.84
South America	1,780	33.5	4.4	78.3	518.0	596.3	2.81
Africa	3,030	23.5	5.2	157.5	554.5	712.0	1.85
Europe	1,050	37.9	20.2	212.1	185.8	397.9	0.60
Asia	4,390	20.2	15.6	684.8	201.9	886.7	0.39
Australia and New Zealand	860	23.2	3.9	33.5	166.0	199.5	9.97
WORLD	13,530	25.3	10.6	1,439.6	1,979.5	3,419.1	0.88

[a]Includes Central America and the Caribbean Islands.
[b]This is 312 million hectares larger than the comparable figure in the U.S. study (PSAC 1967) and 174 million hectares larger than the comparable figure in FAO's Production Yearbook, 1974.
[c]This is 126 million hectares larger than the comparable figure in the U.S. study (PSAC 1967).
[d]Based on population data in FAO's Production Yearbook, 1974.

Source: Buringh, et al. (1975).

kinds, amounts, and placements of these for cropping systems, including multiple cropping, is limited.

Full use of oxisols and ultisols for food production is also complicated by two other problems. One is that these huge areas are sparsely populated and lack the infrastructure that exists in the high-income countries. The other is that these areas are thousands of kilometers from the densely populated regions of southeastern Asia and western Europe.

In the 1967 PSAC study, nonarable but potentially grazeable land was estimated at about 28 percent of the ice-free land surface (PSAC 1967). About 22 percent (FAO 1974b) is grazed now, most of it in arid and semiarid regions, and including some that is potentially arable. In a substantial portion of it, forage production is far below what would be possible with improved range management, including controlled grazing. More information is needed, not only about soil and range management and farming systems but also about social and cultural systems in the little known parts of the world where nomadic grazing has been practiced for hundreds or thousands of years. Even if a severalfold increase in the production of forage and livestock was to be brought about, its effect would be relatively small in comparison to the worldwide production of grains and other food crops. However, increased meat production could improve diets substantially.

WATER

Between 97 and 98 percent of the total water on the earth is in the oceans. Only about 2.5 percent of the total is fresh, and 97 percent of that is stored in glaciers or as groundwater (Nace 1969). The rain and snow that fall on the continents each year, only 106,000 km^3 or .007 percent of all water, provide almost all the precipitation used by plants. About 65 percent of the land precipitation is evaporated or transpired, and nearly all of the remaining 35 percent (0.0025 percent of the total) supplies the annual flow of the rivers (Mather 1974).

Rivers are the principal source of water for irrigation, although as much as one-third of the annual supply of irrigation water may be supplied by groundwater (Peterson 1971). Most of the groundwater used for irrigation is associated hydrologically with river flow and accordingly is renewable. The amount of nonrenewable groundwater in relatively long-term or semipermanent storage that can be used economically for irrigation is conjectural, in view of its depth and frequent salinity. The total amount in storage is

relatively large, 0.615 percent of the global total (Nace 1969).

Although only about 6.5 percent of the annual precipitation on the land surface is actually used for evapotranspiration to produce crops, precipitation is highly variable both as to time and geographic distribution. Much of the world gets too little, while some gets too much, at least at certain times. Damaging floods due to excessive precipitaton or inadequate surface drainage often have ranked along with drought as major causes of shortfalls in food production in China, India, Bangladesh, and Pakistan. Precipitation and irrigation often mean too much water in millions of hectares of soil, and artificial underground drainage is often needed to grow most crops other than rice. Too little precipitation is much more widespread than too much, but time variances in precipitation constitute perhaps the most serious threat to stable agriculture. Short-term droughts during the growing season, as well as wide annual fluctuations in rainfall, are the principal causes of shortfalls in agricultural production (NRC 1976a).

Irrigation to supplement precipitation has been critical to agriculture in much of the world for four or five millenia, and has now become critical on a global scale. The latest estimate (Buringh et al. 1975) of total land under irrigation is 201 million hectares, or 14.3 percent of the land under cultivation.[2] This is higher than the estimate of 11 percent made nearly a decade ago (PSAC 1967) and indicates that irrigation continues to increase. In the People's Republic of China, for example, irrigated land has been expanded (U.S. Department of Agriculture [USDA] 1975a) and now amounts to 40 percent of the cultivated land (Peterson 1974).

If all water resources available for irrigation were to be used, an estimated 470 million hectares could be irrigated (Buringh et al. 1975). This is slightly lower than an estimate of 500 million hectares published in 1969 (Framji and Mahajan).

Kovda (1971) estimates that in "value terms" irrigated farming produces almost as much food as nonirrigated farming. He further estimates that roughly two-thirds of the irrigated land is in rice and one-third in cotton, sugar beets, sugarcane, fruits, and grains. Much of the irrigated rice is not in arid areas but in subhumid and humid regions of the world, mainly in the Indian subcontinent, Southeast Asia, and the Far East. Double or even triple cropping is commonplace where climate permits, although this practice is not as widespread as will be necessary to meet world food needs.

Nearly everywhere irrigation is expensive, and the return is not always high. The water in an irrigation system does not remain under individual control, and social institutions are generally not effective in governing its distribution. This human factor may be the principal difficulty in improving the effectiveness of irrigation. In the arid and semiarid regions, where irrigation is used mainly to grow dryland crops, soil deterioration due to salt accumulation is common. Drainage to flush out excess salts usually is needed but often not provided. One estimate is that 50 tc 60 percent of the previously nonsaline soils in the arid zone have now become saline due to irrigation (Kovda 1971). In addition, water use efficiency in terms of evapotranspiration compared to the amount applied or diverted for irrigation is still low. Even in the western United States, irrigation efficiency is only about 40 percent (Brown et al. 1975). The problems inherent in irrigation are serious, but through research and the application of known technology the prospects are good that food production on irrigated land can be increased substantially.

Important as irrigation is, the management of rainwater is no less important. About 85 percent of the land cultivated today is dependent on rainwater. Hence even a small but widespread improvement in the conservation and use of rain and snow can result in a substantial increase in food production worldwide. In India, where moisture conservation is poor, a conservative estimate is that a 50 to 100 percent increase in yield would be possible with improved moisture conservation, fertilization, and responsive crop varieties. Without better moisture conservation, however, fertilization and improved crop varieties will mean only insignificant gains, or even losses. In fact, improved moisture conservation sometimes makes the difference between a crop and no crop (USDA 1966 and D. A. Williams 1976, personal communication, The Ford Foundation). Moisture management on the world's vast grazing lands, primarily by varying grazing practices but also by changing plant species, is important also.

Adequate water control is essential to improved control of soil erosion, which remains a serious problem. Substantial increases in food production are possible from widespread application of known technology and the development of still better ways to use fully the water resources that come as rain and snow.

TRANSFER OF SCIENCE AND TECHNOLOGY

The disparity between the high-income and developing countries in yields of cereal crops is severalfold. The disparity between yields obtained by researchers and innovative farmers and those shown as national averages are even greater. This disparity suggests that a substantial increase in food production could be brought about if ways and means could be found to transfer and apply relevant technology to the small farms of the developing countries.

While there is no absolute assurance that technology developed in one area will be applicable in another, the new soil taxonomy (USDA 1975c) provides an improved vehicle for the transfer of soil and water management technology. Although soil scientists developed this taxonomy for the United States, they had much help from foreign soil scientists, and the resulting soil classification is worldwide in scope. It is a versatile classification that can be very useful in the transfer of technology.

The probability is high, for example, that a soil and water management system successful on a specified kind of soil in the Congo Basin in Africa also will work on similarly classified soils in the Amazon Basin of South America if farming systems permit. Full use of the soil taxonomy will require classification of soils in both the source-information localities and the target-application areas. This will entail soil surveys, at least in the target areas.

The fact that yields have been increasing worldwide in the last decade or two shows that improvement is possible, but the increases (in terms of kilograms per hectare) generally have been greater in the high-income countries, indicating that technology transfer has been slower in the developing countries. Speeding this transfer to the millions of small farms in South and Southeast Asia, where additional arable hectares are scarce, could have a tremendous short-run impact.

YIELDS

For centuries grain yields in western Europe were low, about 800 kg/ha (DeWit 1968). Today the average grain yield in the Netherlands exceeds 5 tons/ha, and in several other west European countries it exceeds 4 tons/ha (FAO 1974b). Research discoveries and innovative farming, along with compatible social and economic conditions, have made this phenomenal increase possible. As long ago as the early 1950s in India, paddy yields of 15 to 17 tons/ha were obtained in production contests (Simonson 1955); today the average

paddy yields are only 1.6 to 1.7 tons/ha (FAO 1974b). Rice production of 26 tons/ha has been obtained by means of experimental triple cropping in the Philippines (DeVries et al. 1967, cited by Buringh et al. 1975). Yet that country's average is only about 1.6 tons/ha (FAO 1974b). In much of the world, particularly in the developing countries where yields are low, the prospects for improvement are high if optimum combinations of interacting inputs can be discovered and adjusted to the kinds of soils available.

MAXIMUM POSSIBLE PRODUCTION

Buringh, et al. (1975) computed the "absolute maximum food production" capacity of the world assuming full use of all soil and water resources, including all possible irrigation, and also assuming full use of modern production technology, including adequate fertilization and improved and responsive plant varieties. The researchers took into account constraints imposed by climate and permanently unfavorable soil characteristics. According to their computation, "... the absolute maximum production expressed in grain equivalents of a standard cereal crop...is almost 40 times the present crop production."

Some may find this figure unrealistically high, particularly in view of the implicit assumption that all inputs--including fertilizers; seeds of improved and adapted crop varieties; pesticides; machinery and fuel for timely plowing, planting, and harvesting; and irrigation water--would be available in adequate amounts at the right time wherever needed. The estimate illustrates, however, that food production will continue to be hampered more by shortages of other needed inputs than of soil and water resources, and by persistent traditions, social customs, unfavorable economic conditions, and inadequate institutions. To bring about substantial increases in food production will be neither easy nor instant, but globally, at least, the soil and water resources needed to accomplish the task exist.

Profile 1

SOIL AND WATER MANAGEMENT SYSTEMS FOR THE TROPICAL OXISOLS AND ULTISOLS

The world is currently using about 40 percent of its potentially arable land resources. It has been estimated (Buringh et al. 1975) that 1.4 billion of a total 3.4 billion hectares of potential agricultural land in the world are being cultivated at present, and that 201 million of a total 470 million hectares of irrigable agricultural land are currently irrigated. From the standpoint of land and water utilization there are two main strategies for increasing world food production: increasing yields through more intensive use of land already under cultivation, and increasing the total amount of cultivated area in the world. A priori judgments about the value of either strategy as compared to the other ignore important geographic and demographic considerations. This profile focuses on the prospects for increasing world food production by expanding the cultivated area in regions with the greatest potential; other profiles emphasize ways to increase the productivity of land already in use.

The largest proportion (about 40 percent) of the 2 billion hectares of potentially arable but virtually unused land in the world lies in the vast tropical forest and savanna areas that have acid, highly weathered soils classified as oxisols and ultisols (Kellogg and Orvedal 1969). These regions comprise approximately 1,660 million hectares, of which 822 million are potentially arable (Table 4). The largest contiguous regions are in the interior of South America and in Africa, with smaller but important areas in Indonesia and Malaysia. Most of the remaining potentially arable land lies in northern portions of North America and Asia which have severe temperature limitations.

The bulk of these oxisols and ultisols are in their natural state or are devoted to shifting cultivation and extensive cattle grazing. The climate in the tropical forest and savanna areas is ideal for year-

Table 4. Approximate extension of ultisols and oxisols in the tropics (million hectares)*

Soil order	Dry season (months)	America	Tropical Africa	Tropical Asia	Total area	Potentially arable
Oxisols	<3	390	380	0	770	460
	3-6	170	170	0	340	190
Ultisols	<	165	75	190	430	122
	3-6	35	25	60	120	50
TOTAL		760	650	250	1,660	822

*Calculated from a "Soils of the World" map (1972) and Orvedal and Ackerson (1972).

round crop production since there are virtually no temperature limitations; about 70 percent of these areas have high annual rainfall with no prolonged dry season, while the remaining 30 percent have a 3- to 6-month dry season (Table 4).

Many of these soils have excellent physical properties with gentle topography suitable for intensive crop production. Although these areas lack a well-developed social and economic infrastructure, the main reason these favorable lands are not being cultivated extensively is the general lack of native soil fertility. The luxuriant vegetation of oxisols and ultisols in tropical forests and savannas is deceptive, since there is a tight nutrient cycle between vegetation and soil. When these areas are cleared fertility declines rapidly and weed control becomes difficult (Nye and Greenland 1960). Farmers without access to fertilizers or manures normally shift to other areas.

The terms "low soil fertility" and "low base status soils" encompass a general deficiency of several macro- and micronutrients, low cation exchange capacity of the clay fraction, high soil acidity, aluminum or manganese toxicity, and, in many loamy or clayey textured topsoils, high phosphorus fixation capacity. People

have traditionally settled first around soils with high native fertility. To a large extent, population density and high base status soils are still highly correlated except in arid regions, where water is the main limiting factor (Sanchez and Buol 1975).

The tropics do not have a monopoly on low base status soils. Large areas of very similar ultisols are found throughout the southeastern United States and southeastern China, where they are now under intensive crop production. These regions were first settled where pockets of high base status soils existed. When population pressures increased, the low base status soils were made productive through the intensive use of fertilizers and amendments. This approach, unfortunately, is not directly transferable to most tropical regions because of a totally different economic and social environment and recent increases in the prices of fertilizer and related inputs. The basic principles for improving low base status soils are the same anywhere, but appropriate technology suitable to different environments needs to be developed (see Profile 1, Subgroup A).

The conquest of the tropical low base status oxisols and ultisols has been attempted for decades. Following a classic example of failure in the attempt to transplant temperate region high-energy technology to what is now Zaire in the 1930s (Jurion and Henry 1969), progress generally has been limited to small areas with ample capital available for export crops, of which Hawaii and the state of Sao Paulo in Brazil are perhaps the best examples. Systematic research aimed at developing realistic soil management practices for food crop production on tropical ultisols and oxisols is underway in a number of tropical countries and is supported by both U.S. and international organizations. Where population densities are low, national governments have not felt an immediate need to develop their "empty" lands, but the need to put them into production is rapidly approaching. Programs to disperse populations into ultisol and oxisol regions without a sound agronomic base predictably fail. Major commitments are now being made for rapid agricultural development in these areas. Research and development on the most appropriate ways to manage these tropical soils is a prerequisite for the success of these efforts.

The emphasis here on acid, low base status soils should not be taken to imply that the management of relatively fertile, high base status tropical soils is unimportant. That subject is addressed in most of the other profiles of this study team. Much more is known about high base status soils than about the oxisols and ultisols. For example, dark colored, heavy clay

vertisols are important in many regions and cause severe problems due to their tendency to shrink or swell under variable moisture. Dudal (1965) summarized these problems and their solutions: what is needed, to a large extent, is the application of known technology.

RESEARCH NEEDS

In order to put the vast areas of oxisols and ultisols into full agricultural production, the following research and development activities should be carried out. This research should be conducted at representative sites so that the results will have utility over the widest possible area. The following components must be included in a proper time sequence.

Survey of Possible Impact Areas

Available information on soils, climate, water resources, and present cultivation practices must be compiled. Soil surveys should identify the suitability of soils for crop production, pasture, or forest. In essence, the identification of potentially arable land is done at the microlevel. Determining local variability is extremely important because potentially irrigable areas and areas with high base status soils exist and can be utilized more intensively with the technology already available. Soils with very limited potential, on the other hand, should remain under forest or savanna. Wise evaluation of soil and water resources will prevent ecological catastrophies and guarantee a positive rather than a negative environmental impact. Although forest areas have been described by the media as the "lungs of the world," it should be remembered that a hectare of productive cropland produces as much oxygen as a hectare of tropical forest (H. Leith 1975, personal communication, Department of Botany, University of North Carolina, Chapel Hill).

Soils must be classified according to the newly developed soil taxonomy and interpreted according to various technical classification systems in order to allow the extrapolation of research results to a large area (see Profile 4). Research stations must be located on soils that represent the range of local variability, usually along a topographical or drainage gradient. These rather obvious principles are emphasized here because the majority of field stations are established without such preliminary studies. As a result, they frequently are placed at unrepresentative

locations where research produces results with limited extrapolation value.

Land Clearing and Preparation

The method used to clear forested areas is one of the most critical parameters affecting the productivity of new agricultural projects. Experiences in the Amazon jungle of Peru (Seubert et al. 1977) and in Africa (R. Lal 1976, personal communication, International Institute for Tropical Agriculture, Ibadan, Nigeria) show that mechanized land clearing methods often accelerate the decline of fertility and the deterioration of the physical properties of land, particularly in forested areas. Improper land clearing methods are considered by knowledgable persons to be probably the single most important reason for the failure of large-scale settlements. The dissemination of knowledge on proper land clearing is needed tc assure that proper use is made of the ash that remains after trees have been burned. This ash is a valuable source of fertilizer and lime. Dissemination of such knowledge also is necessary to ensure that soil erosion problems are not created in areas with frequent rainstorms.

Correction of Soil Fertility

After the first one cr two crops in tropical forested areas, and immediately in savanna regions, severe fertility limitations are encountered that should be corrected in an economical fashion. The principal research problems are outlined below.

Soil Acidity and Liming

Aluminum and manganese toxicity is a common problem of ultisols and oxisols. Although the basic chemistry of soil acidity is well established, much needs to be learned about its application in tropical ultisol and oxisol areas (Pearson 1975). The residual effects of lime are believed to last for several years, but the exact duration is known in very few areas. Deep lime application (down to 30 cm) has proven extremely beneficial in the oxisols of Brazil, where the subsoil is so acid that root development is restricted and plants suffer from water stress during the temporary droughts which commonly occur in the rainy season, even though the subsoil has ample available moisture (North Carolina State University 1973:14-23). More efficient

ways to use lime should be fully developed and coupled with the use of plant varieties more tolerant cf scil acidity.

Phosphorus Deficiency and Fixation

Most loamy or clayey ultisols and oxisols have a high capacity to "fix" phosphorus into slowly available forms. Phosphorus is almost universally deficient in these areas, but adding phosphorus fertilizers may not be economically feasible because of the large amounts needed. Overcoming this problem under present energy constraints involves the following strategies (Sanchez and Uehara 1977): (1) improve the efficiency of phosphorus fertilization through better placement methods and the use of cheaper, low solubility rock phosphates; (2) decrease the soil's capacity to fix phosphorus through liming and silicate applicaticns; and (3) develop crop varieties tolerant of phosphorus stress and maximize the utilization of mycorrhiza or micrcbial populations that increase phosphorus absorption by plant roots (see Profiles 5 and 6). Field studies also are needed to evaluate the long-term residual effect of phosphorus applications.

Correcting Other Nutrient Deficiencies

Most tropical oxisols and ultisols are also deficient in other nutrients essential for plant growth. These include nitrogen, potassium, calcium, magnesium, sulfur, zinc, iron, boron, copper, and molybdenum. Nutrient deficiencies must be identified by rapid, inexpensive, and accurate soil and plant analysis so that deficiencies can be corrected at economically optimum rates. The capacity of soil to supply nutrients is not static; after land is cleared, nutrient capacity changes with time until a new equilibrium is reached. Fertilizer requirements, therefore, vary during the first few crucial years. The use of leguminous crops to provide nitrogen fixation must be a part of the program (see Profile 6). Although the basic principles of soil fertility are well established, the application of such knowledge to ultisol and oxisol regions from a systematic viewpoint needs to be strengthened (see Profile 2, Subgroup C).

Maintenance of Soil Physical Properties

After new lands are cleared the soil is exposed for the first time to increased temperatures and the direct

impact of rainstorms. These factors make newly cleared land susceptible to compaction and erosion. This is particularly important in forested ultisols with a sandy topsoil texture, but it is also important in the well-structured oxisols. A large portion of the Piedmont region of the southeastern United States became unsuitable for agriculture when ultisols were cleared and severe soil erosion occurred. Technology for preventing the deterioration of the soil's physical properties needs to be developed for specific environments. Low-energy technologies based on zero tillage, mulching, and multiple cropping appear to be promising (Lal 1975). Short-term fallows with leguminous crops also should be included.

Many well-structured oxisols have a low water-holding capacity due to the strong aggregation of clay particles, which are cemented by iron and aluminum oxides (Wolf 1975). Such practices as mulching and deep liming are needed to overcome this problem. Practices aimed at conserving the physical properties of soil also are likely to improve certain chemical properties.

Development of Suitable Cropping Systems

Long-term studies are needed to establish the most productive cropping systems for newly cleared ultisols and oxisols and for different levels of capital investment and farm size. The best combination of food crops with or without pastures and/or fruit crops and/or forest needs to be established at the local level. These studies also should devote attention to intercropping and other forms of multiple cropping (see Profile 5). The use of crop species and varieties tolerant of soil-related stresses, such as aluminum toxicity, should play an important role (see Profile 5). Many important tropical crops, including most root crops and plantains, are poorly understood in terms of their fertility and moisture requirements, which need to be established. The question of long-term crop rotation also needs investigation. Continuous food crop production may not be as advisable as the use of short-term fallow periods to improve the soil. Fallow land, in some cases, can be used for grazing animals.

Reduction to Practice

After research has established adequate soil and water management systems, the information should be utilized on a wider scale in development projects, whether public or private. Close contact between

research and development is needed in order for them to operate as a logical continuum.

IMPACT

On World Food Production

A successful, tropics-wide research and development program would make it possible to increase the world's cultivated area by 50 percent. The probability of success in soil and water research is high. Ultisols are extensive and well managed in the southeastern United States, and oxisols are extensive and well managed in Hawaii. Because of year-round production possibilities in the tropics, increases in world food production are likely to be substantially higher than the increase in area, perhaps on the order of 70 to 100 percent. Because of the vast areas involved and the need for parallel infrastructure development, the proposed research is a necessary but not a sufficient condition to achieve these increases in cultivated area and food production. The management of these tropical soils, however, is one of the least understood aspects of the food-population imbalance, and the success of research in this area is a key assumption in long-term estimates of the world's ability to feed itself (Kahn et al. 1976:123-124).

The potential for major increases in food production in these oxisol and ultisol areas within the physical, economic, and social constraints of the tropical environment has been demonstrated in several areas. Ongoing research in the jungle areas of Peru (North Carolina State University 1974:27-41) and Nigeria (International Institute for Tropical Agriculture 1974) shows that two or three grain crops can be harvested every year from these soils if they are well managed, with each crop yielding about 80 percent as much as in the United States. With two or three crops a year these lands may produce about 150 to 200 percent as much grain as temperate regions, where only one crop can be grown each year. Similar indications of the potential productivity of the savanna oxisol areas have been documented in Colombia (Paladines 1975) and Brazil (Anonymous 1975b). Due to the severe dry season in most savanna areas, however, no more than one crop a year can be expected without irrigation. A great potential for increasing beef production in these areas also exists, despite the long dry season (Paladines 1975).

Timing and Geographical Implications

Because of the vast areas involved, the full impact of the proposed research is expected to come after 1985. However, there are certain tropical forests and savanna regions which are rapidly being developed where a pre-1985 impact is expected. The time scale of such a development is envisioned as follows:

Short Run (0 to 15 Years)

The most immediate impact is likely to occur in parts of South America where the agricultural frontier has moved to the oxisol and ultisol areas. The opening of new highways in the South American interior has resulted in a significant migration of people away from the densely populated Andean valleys and northeast Brazil. The governments of Brazil, Peru, Colombia, and Venezuela have made major commitments to occupying and developing their savanna and jungle regions and to developing the necessary physical and economic infrastructure. Brazil is currently committed to opening 3 million hectares of its cerrado (oxisol savannas) by 1979 and 50 million hectares by the end of the century (Anonymous 1975b). Peru, which has no cerrado, is committed to developing 2 million hectares of its Amazon jungle for crop production in order to feed its expected 1990 population. Similar short-run efforts are expected in other regions. FAO estimates that developing countries can expect to increase their cultivated land from 737 million hectares in 1970 to 890 million hectares in 1985, an average increase of about 10 million hectares per year (FAO 1974a). Most of this increase is expected to take place in oxisol and ultisol areas of Africa and South America. FAO also assumes that 75 percent of these areas will be occupied spontaneously and 25 percent through organized settlements sponsored mostly by governments. Much of this rapid development will take place without the benefit of solid information on how to manage these soils profitably for continuous agricultural production. This problem is likely to be particularly acute along newly opened roads in jungle areas. After the native soil fertility is exhausted during the first few years, settlers will return to their crowded hometowns, often leaving severe ecological damage behind. The question is not whether these areas should be settled, since they will be in any case. The issue is how to provide the proper technology so that settlements remain productive and the environment is not damaged.

Long Run (Post-1985)

In the years after 1985 the agricultural frontier will continue to advance into other oxisol and ultisol areas of Africa, Southeast Asia, and tropical South America. The previously cited FAO study suggests that the rate of opening new arable land after 1985 will be higher than the present rate of 10 million hectares per year. The problems discussed in the previous paragraph also will apply to areas opened up after 1985. The pressure to increase food production will be greater at that time, however, as the rate of increase in crop yields from high base status soils begins to level off following the achievement of major research breakthroughs in the fertile areas.

Other Impacts

The conquest of the low base status soils of the tropical forests and savannas is likely to result in population shifts similar to those that followed the opening of the U.S. West about a century ago. While the main centers of population are likely to remain where they are (on the coast or in mountain areas of the tropics), pioneering families will migrate to the more isolated areas, relieving some of the pressures in older population centers and creating new ones as well. For example, the city of Brasilia, located in the heart of the oxisol savannas, already has a metropolitan population of 1 million although it is only 18 years old. Whether the newly opened rural areas are occupied by large or small farming operations will depend on the dominant social ideology of the countries involved. There is room for both approaches. In any event, these sparsely settled areas will be net exporters of food to crowded urban centers of their own countries and to other nations as well.

Research on the management of tropical ultisols and oxisols is also likely to have a beneficial effect on U.S. agriculture. Ultisols are the dominant soils of the United States from New Jersey southward through Texas. Oxisols are present only in small areas of Hawaii and Puerto Rico, but many oxidic soils in the Piedmont region of the southeastern United States have similar management problems. In the past most of the transfer of knowledge have been from the temperate regions to the tropics. Implementation of the proposed research in the tropics should result in feedback applicable to the "red" soils of the United States, particularly in the development of energy-saving management systems.

IMPLEMENTATION

A number of U.S. and U.S.-supported institutions are deeply involved in the development of the ultisol and oxisol regions in cooperation with foreign governments. These include North Carolina State University, Cornell University, the International Institute for Tropical Agriculture (IITA) in Africa, and the Centro Internacional de Agricultura Tropical (CIAT) in Latin America. FAO is increasingly involved in agricultural research around the world, and the Institut de Recherches Agronomiques Tropicales et de Cultures Vivrieres (IRAT), the French agronomic research organization, conducts research on agricultural objectives in francophone African countries. Because of the site-specificity of this research, no single location can produce all the necessary information, and major regional centers need to be developed. The IITA facility at Ibadan serves this purpose well for West Africa, but research centers for the jungles and the savannas of Central Africa also are needed. Similarly, a major center for the Amazon jungle is needed urgently, while the Cerrado Research Center of the Empresa Brasileira de Pesquisa Agropecuaria (EMBRAPA) in Brasilia can serve as the regional center for the South American savannas. Even more important than these regional centers is the establishment of a dynamic, well-coordinated, tropics-wide network of field stations to develop specific practices for specific environments.

The annual cost of developing new regional centers for the Amazon, the Central African jungles, and the Central African savannas, as well as strengthening the existing centers in Africa and Latin America and developing a cooperative field station network, is likely to amount to $30 to $40 million per year for an initial three-year period, followed by an estimated annual operating cost of $15 million.

Development projects financed by the private sector are in operation in the Amazon of Brazil and other areas and are generally needed and encouraged. Massive government-sponsored colonization programs also are in operation. The estimated costs will vary with the number and size of these projects.

Profile 2

SOIL AND WATER MANAGEMENT PRACTICES FOR MULTIPLE CROPPING SYSTEMS

Most of the scientific knowledge about soil-plant-water relationships is based on crops grown in single stands, usually once a year, in temperate countries. For centuries, however, farmers in the tropics have taken advantage of adequate year-round temperatures and sunlight to grow not one but several crops a year when water is available.

Multiple cropping can be defined as growing two or more crops on the same field during one year. Its various forms can be divided into two main categories: sequential cropping, where two or more crops are grown one after the other in the same field during one year; and intercropping, where two or more crops are grown simultaneously in the same field (Andrews and Kassam 1976).

Growing a variety of crops in a seemingly random arrangement has long been scorned by many agricultural scientists as primitive and disorderly, notwithstanding substantial evidence to the contrary, and it was not until Bradfield did pioneering scientific work at the International Rice Research Institute (IRRI) that international attention was attracted to multiple cropping. Most scientists now agree that multiple cropping practices are scientifically sound and usually result in higher production per unit area than monoculture. Yields from intercropping normally range from 20 to 40 percent higher per hectare (Andrews and Kassam 1976).

Agronomists and social scientists also have discovered that the vast majority of food consumed in the developing countries is produced on small farms using a wide variety of complex multiple cropping systems. About 70 percent of the food consumed in Colombia and Central America, for example, is produced on small farms (Pinchinat et al. 1976). The aggregate importance of small farming systems in the tropics is enormous. In tropical Asia 75 percent of the farms are

smaller than 2 hectares (Harwood and Price 1976). Although many are devoted to paddy rice monoculture, a large number also produce several crops in multiple cropping systems. In Central America 69 percent of all farms are smaller than 5 hectares (Pinchinat et al. 1976). FAO's 1973 Production Yearbook (1974b) reports data indicating that the average farm size in 20 tropical African countries was 5.4 hectares.

The demand for increased production also has resulted in the extension of sequential multiple cropping practices to parts of the United States, where two crops per year are now harvested on land that previously produced one crop. Approximately 25 percent of the soybeans produced in North Carolina during 1975, for example, were double cropped with wheat (J. A. Phillips 1976, personal communication, North Carclina Agricultural Extension Service).

In 1975, the American Society of Agronomy sponsored a symposium at which worldwide knowledge of the ongoing practices, basic principles, and research needs of multiple cropping was presented by an interdisciplinary group. This symposium showed that one of the main obstacles to increased food crop production is inadequate knowledge of soil-plant-water relationships where two or more crops are growing simultaneously in the same field.

RESEARCH NEEDS

Fundamental Aspects

Most of the principles of plant competition have been developed through experiments with varieties of the same species or through experiments using both grass and legume pasture species. How do combinations such as corn and beans interact when planted together? Notwithstanding some valuable experiments, agronomists simply do not know. Fundamental research on intercrop and intracrop competition is needed in order to understand how two interacting crop canopies utilize available solar radiation, how two interacting root systems absorb water and nutrients from the soil, how the presence of the root of one species affects the growth and functioning of another, and whether the different root systems intermingle or tap different soil depths. While some intercropping systems are known to be pest suppressant, the mechanisms involved also need to be understood. Intercropping leguminous crops with cereals or root crops is commonly practiced. What is the fate of biologically fixed nitrogen under these conditions?

Logically, existing intercropping systems should be studied first in order to find out how they work. The basic understanding developed from such studies would, it is hoped, provide the knowledge needed to improve these systems and develop new ones.

Nutrient Requirements for Intercropped Systems

Preliminary work in Trinidad (Dalal 1974) and the Philippines (IRRI 1974b:339-342) shows that certain intercropped mixtures take up more nutrients from the soil than monoculture fields of the same plant varieties. This suggests that intercropped plants may use fertilizers more efficiently. This possibility needs to be explored systematically in view of the potential for utilizing expensive fertilizer inputs more efficiently. The most economical sources of nutrients, as well as the most suitable rates of application and timing and placement practices, must be developed for the different intercropped systems. The role of crop residues, slow release fertilizers, animal manures, and biological nitrogen fixation of legumes needs to be established. In a corn-soybean intercropped system, for example, fertilizer nitrogen should be added only to the corn rows in order not to disturb nitrogen fixation by the legume. But in order to take advantage of this free nitrogen, the next cereal crop should be planted in the rows previously occupied by soybeans. Studies need to be conducted on all aspects of soil fertility, including the use of soil amendments.

Selection of Varieties and Plant Types

Plant breeding traditionally has been aimed at developing improved varieties for monoculture, but a plant variety good for monoculture may not be the most efficient one for intercropping. It is already known that the most efficient intercropped combinations are those that combine a tall-statured erect plant with a short-statured plant whose leaves are horizontal. Specific breeding objectives for intercropped systems are needed (Francis et al. 1976a). Likewise, new varieties adapted to the different day length, temperature, and moisture regimes encountered by sequential cropping systems in the United States are needed. What happens below the ground is not known, although it can be speculated that an aluminum-tolerant plant variety will permit its roots to extend into an acid subsoil while the accompanying plant may restrict its rooting volume to limed topsoil (see Profile 5).

Consequently, well-focused programs on varietal improvement are needed as part of the overall interdisciplinary effort.

Water Management and Soil Physical Properties

Some intercropped systems are intensively irrigated throughout the year, but little is known about their water requirements, such as the effect of frequency of water application and the efficiency of water use as compared with monocultures. Other systems are completely rainfall-dependent. The possibilities for irrigating rainfall-dependent systems or for improving their drought tolerance need to be established.

In many clayey soils of the deltas and valleys of Southeast Asia, the growing of rainfed crops after the rainy season-flooded rice crop may result in dramatic increases in food production and in the nutritional quality of the farm household diets. This practice is known to be possible in sandy or loamy soils because the destruction of soil structure caused by puddling for the rice crop can be reversed rather quickly. In heavy clay soils, particularly those with montmorillonitic mineralogy, changes in soil structure are slow and depend on precise water management. Research on how to overcome this problem is needed.

The effects of intensive intercropping on the physical properties of soil need to be determined. In general, protecting the soil by maintaining a crop canopy throughout the year is likely to retard erosion, but this needs to be quantified for specific situations.

Pest Control

Certain multiple cropping systems are considered pest suppressant (Litsinger and Moody 1976). There is less opportunity for weed growth when land is cropped for a longer period of time, especially where two or more crop canopies are present. There are fewer insect and disease attacks in certain intercropped systems than in their respective monocultures. The amount of actual data on this subject, however, is restricted to a few systems. A broad study of the conditions under which multiple cropping favors pest control may have major worldwide implications.

Equipment Research

A common limitation to the spread of multiple cropping systems is shortage of labor for planting and harvesting. Even on small farms the introduction of small specialized tractors can permit the farmer to utilize labor more effectively. Although excellent research on this subject is being conducted at IITA and the International Rice Research Institute (IRRI), it should be extended to cover other multiple cropping systems (see Profile 2, Subgroup D).

The main limitation to the spread of intercropping systems into capital-intensive large farms in temperate and tropical regions is the need for specialized equipment. The genius of the agricultural engineers who developed machines to harvest grapes and tobacco leaves should be put to use to design planting, cultivating, and harvesting equipment for intensive multiple cropping systems. Both new designs and adaptations of present designs are needed.

IMPACT

On Increasing Food Production

A worldwide interdisciplinary effort to improve the productivity of multiple cropping systems is likely to have a major impact on food production in areas already under intensive cultivation. Since multiple cropping is still a largely unexplored scientific area, it seems reasonable to expect major breakthroughs and widespread adaptation of new systems within the first 15 years of a major research effort. The preliminary results already obtained (Indian Agricultural Research Institute 1972, Bradfield 1972, Papendick et al. 1976) fully support this assumption. These publications show numerous instances where annual food production was doubled or even quadrupled with improved sequential or intercropped systems. The wide variety of multiple cropping systems in different physical and social environments makes it impossible to give a quantitative estimate of expected increases in food production. Although the major impact is believed to be short run, continuing long-run improvement (post-1985) also can be expected.

On Farmers and Consumers in Developing Countries

The proposed research is aimed directly at improving the yields of small farms in tropical

countries. Farmers in these countries are likely to enjoy more stable production and more stable income when multiple cropping systems are improved, lessening their dependence on single crops. The proposed research also can be expected to improve human nutrition through the production of protein-rich and vitamin-rich crops in addition to carbohydrate-rich crops.

Since most of the food consumed in tropical developing countries is produced on small farms relatively close to urban areas, the proposed program also should have a positive impact on these areas. Greater and more stable food production on the farm will result in more stable supplies and market prices to local consumers.

On U.S. and Other Large-scale Farming Systems

Multiple cropping research also can be expected to have a favorable impact in the United States, since some of these ancient tropical practices can be adapted to large-scale mechanized farming systems. Some innovative concepts tested under limited conditions show the potential impact. In western Minnesota, for example, strip intercropping of corn and soybeans modified the microclimate in such a way that total yield was increased by 25 percent (Radke and Hagstrom 1976). Intercropped systems of corn and soybeans in North Carolina have produced 40 percent more yield per hectare than crops of each grown in separate fields of one-half hectare each (R. E. McCollum and A. Cordero 1976, unpublished data, Soil Science Department, North Carolina State University).

In many areas of the United States the possibilities for further increasing monoculture yields per hectare appear quite limited. Monoculture also has led to specialized, capital-intensive (and sometimes counterproductive) pest control. Multiple cropping concepts of intensification in time and space may spearhead the next breakthrough by providing opportunities for better soil and water management and decreased dependence on high-cost pesticides.

A similar potential exists for large-scale, mechanized farming in the tropics as well as in other high-income countries. The absence of temperature limitations in the former should increase the advantages of multiple cropping even more significantly.

On the Environment

Improved multiple cropping systems are likely to have a positive impact on the environment. Multiple cropping is essentially a form of low-energy technology that should make for better utilization of human and animal labor in small farming. Better use of available solar energy, water, and nutrients by multicanopies above and below the ground, a different approach to pest control, and improved soil conservation are examples of positive environmental impacts.

IMPLEMENTATION

The site-specific nature of different systems requires that agronomic-economic research into multiple cropping be widely scattered in regions where such systems are already practiced. Whereas basic work can be done anywhere, the research is probably best done at regional research centers in order to focus more specifically on social, soil, and climatic constraints. Ongoing research efforts by IRRI, IITA, and the Centro Agronomico Tropical de Investigacion y Ensenanza (CATIE) in Costa Rica, and at North Carolina State University in the United States, should be strengthened. More importantly, a coordinated network of field stations throughout the tropics should be established. In most cases the physical and social infrastructure is already in place, and all that is required is a change in research program orientation. The estimated additional operating budget for a functioning multiple cropping network including both fundamental and applied research is likely to be on the order of $10 to $20 million a year for at least 15 years.

Profile 3

WATER MANAGEMENT AND UTILIZATION IN RAINFED AGRICULTURE

Much of the world's food is produced by rainfed agriculture. Success in this type of agriculture is highly dependent on careful water management. Unfortunately, rainfall is extremely variable in vast regions of the world, including Central Africa, Southern Asia, and eastern South America, all of which have annual rainfall variations of ±40 percent or more (Landsberg 1975). Our basic concern, however, is not with variations in rainfall but with their effects on crop production.

In view of their importance, grains and pulses are probably the most logical crops for concentrated research in water management. Small upland grains are grown primarily on temperate and subtropical steppes, but upland grains and pulses are increasingly being grown as dry season crops in tropical and subtropical wet-dry climates. Rice flourishes in tropical and subtropical monsoon areas. Maize can thrive in both temperate and tropical regions.

The water requirements for these crops are almost never in phase with the water available from natural rainfall. The vagaries of precipitation are somewhat modified by the ability of soils to store moisture, but usually not enough. Of 3.4 billion hectares of potentially arable land (less than half of this is now cultivated), more than 1.6 billion hectares are in the tropics. On three-fourths of this tropical land, however, the productivity is limited because of insufficient moisture all or part of the year.

RESEARCH NEEDS

There are two ways in which advances can be made: (1) altering physical conditions to make more water available to the crop and to remove excess water, and (2) improving management of the entire crop-soil-weather system.

Improved Physical Conditions

The options available for water management include surface and internal drainage, supplemental irrigation, changes in cropping patterns, tillage, and engineering structures as well as a host of others. Irrigation per se is covered in Profile 7, but its use as one part of a total water management plan is discussed here.

Primary objectives are to increase the amount of captured precipitation (rainfall and snowmelt) and the amount stored in the soil (Goetze 1973). Past research has led to successful methods adapted to specific circumstances. Both infiltration rates and soil water storage can be enhanced by tillage practices, such as zero or minimal till, thus reducing evaporation losses from low intensity rains and runoff from high intensity rains. Crop rotations, use of crop residues, land surface modifications, and conservation structures have been used effectively. Chemical amendments to increase surface infiltration or reduce evaporation have been tried, with but limited success (American Academy of Arts and Sciences and Hebrew University 1975).

Soil erosion and sediment control continue to be serious threats to increased agricultural production. Too often, gains made by clearing and developing new lands are short-lived and may be counterproductive, even in the short run. Increased summer fallow designed to conserve moisture may increase the erosion hazard. Despite extensive erosion control efforts in the United States, no more than 25 percent of the farmlands are under approved conservation practices, and gross annual losses are estimated at an average of 8.3 tons of soil per hectare of cropped land. The situation is even worse in developing tropical areas, where increases in population are resulting in the expansion of subsistence agriculture to steeper slopes, shallower soils, and areas with more erratic rainfall patterns. Under these conditions crop failures are more frequent, and landslides may occur without good soil conservation practices. Even if it were available to farmers in these areas, knowledge of soil and water conservation techniques would not be enough. Such knowledge will be ignored unless it is associated with profits to the farmer (Pereira 1975). Thus if soil

conservation practices are to succeed, it is essential to develop improved cultivation practices and cropping systems which are profitable under widely varying soil conditions.

Water-holding capacity and depth of soil govern how much water the soil can store for later plant use. Thus the development or selection of cultivars having more adventurous and deeper rooting systems is an important research goal. Microclimatic influences on evaporation rates, improved summer fallow techniques, intercropping, and reduction of weed growth can all help increase soil moisture storage.

Where and when water is not a limiting factor, drainage often is. Both surface and subsurface drainage technology is well advanced in the high-income countries. Recent innovations, such as the substitution of corrugated plastic tubing for clay or concrete drain tile and the use of laser-controlled plow-in machines to replace trenchers, have substantially reduced labor requirements and unit costs. But these innovations are not necessarily well suited to less-developed areas.

Many interesting concepts for concentrating and conserving rainwater, especially on the more arid lands, are presented in a recent publication of the National Academy of Sciences (NRC 1974b). Such techniques as rainwater harvesting and runoff agriculture, as well as more conventional approaches, are discussed.

Supplemental irrigation is often possible and has been extensively applied; however, its potential to augment natural rainfall, even in humid areas, has been widely neglected, even though it has been particularly effective in some rice-growing areas. Supplemental irrigation may be provided by the relatively uncontrolled spreading of water from its natural channel during high runoff, or it may be done more precisely, such as through a sprinkler system using surface storage or groundwater.

Both fundamental and adaptive research is needed to improve on the arsenal of tools available, to adapt them to specific circumstances, and to learn their usefulness beyond the area for which they were developed.

The need is for total water management plans tailored to specific farming systems in the developing countries. In Montana, for example, a few rows of tall wheatgrass across a wheat field increase snow catch and thus grain yields; in Minnesota two rows of maize alternating with 16 rows of soybeans modify the microclimate so that 25 percent more grain is harvested; in South Carolina a subsurface system of tubes that either removes or supplies water, as needed,

compensates for the vagaries of rainfall. Improvements like these raise interesting questions. Can crop geometry in the cerrado of Brazil or the jungles of Peru be manipulated to enhance water storage? Can the high-cost concept developed in South Carolina be modified for use in Nigeria?

Grass backslope terraces with rate-controlled drainage are extremely well adapted to the loess hills of western Iowa; they conserve water, reduce erosion, reduce effective landslope, and accommodate six-row machinery conveniently. Is it possible to design a terrace system where land holdings are just a few hectares in size and property lines run up- and downhill?

In the eastern United States, reasonable estimates are readily made of the expected rate of erosion due to land, crop, and cultural factors. But these estimates are not valid even in the western United States, not to mention other regions. With even the best of conservation technology, hydrologic changes and accelerated erosion became severe when a forest was converted to a tea plantation in Kenya (Pereira 1975).

In short, there is a wide array of practices that have proven useful under certain circumstances. Some of these can be transferred confidently to other regions, while others can be adapted. In many cases, however, not enough is known to evaluate their transferability. Furthermore, there are opportunities not yet developed or, perhaps, even conceived. Often overlooked is the possibility of augmenting soil moisture storage by irrigation during the noncrop season. This practice could increase soil moisture storage for a single crop, mitigate short-term drought during the growing season, and extend the cropping season in order to produce more than one crop.

Research should lead to the creation of overall water management plans that will assure better utilization of limited or poorly distributed precipitation. Greater specificity at this point serves little purpose, in view of the wide range of conditions found on the 1.5 billion hectares of rainfed agricultural land. Nonetheless, the all-pervasive problem of water excesses and shortages demands top priority for research on water use efficiency.

Improved Management of Crop-Soil-Weather Systems

Management strategy involves selecting the best cultivar and planting date at an acceptable level of risk in a highly stochastic environment, especially with regard to climate and water. To extract limited water supplies in a way that produces the greatest

return, account must be taken of nutrient availability, desirable planting, harvesting, and nutrient application practices, and pest control.

Cropping systems are discussed elsewhere in this report, but in this section particular emphasis is given to them as a water conserving technology. Traditionally, field trials have been used to determine the plant varieties and agricultural practices most desirable for local situations, and this technique will continue to be used extensively. Nonetheless, field trials have disadvantages. Since a large number are required, they are expensive both in terms of financial and manpower resources, and they are not easy to control, especially under conditions in developing countries. Furthermore, unless they are conducted for relatively long periods, they will cover insufficient samples of the local climate. But when they are conducted over long periods, new varieties usually will have been developed in the meantime.

Soil indicators and climatic analogues have been found to be very useful tools. On the other hand, one of the most significant recent technological developments, automatic data processing coupled with simulation and/or optimization techniques (widely used in nonagricultural management), has not been utilized.

A number of crop production models are in existence or under development which can relate production to environmental variables. By linking these models to stochastic models of weather (climate), simulation models should be developed to predict production levels for various cultivars under sequences of equally likely climatic events. Such combined models have been worked out in a rather crude fashion for a few cultivars of soybeans and corn (Hill et al. 1974).

Crop production models describe the growth of selected plants as affected by site environmental conditions. The environmental model requires a measurement or estimate of the soil water in storage at the beginning of the season, as well as total soil water storage capacity in the root zone, daily values of rainfall, maximum and minimum temperatures, day length, and specific parameters for each cultivar that relate phenology to weather data. The combined model would utilize production and weather data from a number of sites as well as a single site, thus decreasing the length of the test period. For any preselected planting date, the models could predict yield as a function of relative transpiration during each of the several important growth periods. For soybeans, for example, the periods would be emergence to start of flowering, start of flowering to start of podfill, start of podfill to end of flowering, and end of flowering to maturity. Proper attention would have to

be given to such constraints as soil characteristics and freezing dates.

Management may be improved by the selection of a set of crops or cultivars and planting dates such that production under the dynamic weather variables of a specific site is maximized. Tillage, application of nutrients, and other physical modifications can be applied in the most timely fashion. It seems likely that plant and insect pest dynamics eventually could be related, at least in part, to the sequence of environmental and phenological events.

Even the crude programs available now have proven useful for estimating, from historical weather data, the probable effects of management on the yield of specific cultivars. Such programs do not eliminate the need for field trials, but they can be used to identify the most promising cultivars, planting dates, plant populations, and water management practices.

IMPACT

The approaches suggested here are important to nearly all rainfed agriculture in varying degrees. They are particularly applicable to grain, oil, and pulse crops, which (see Table 5) occupy nearly 700 million hectares, almost one-half the currently cultivated land of the world (Gray 1975). Only about 70 million hectares of this land (Doorenbos 1975) are irrigated. Because of their importance and the availability of data about them, these crops are used here as the major, but by no means the only, indicators of impact.

Yields in the developing countries are low. Wheat yields, for example, are only 1.2 tons/ha compared to 3.25 tons/ha in western Europe; current rice yields are only 1.3 tons/ha (milled); sorghum yields in the grain-deficient countries of Asia and Africa are only 0.65 tons/ha compared to 3 tons/ha in the United States and Argentina; the average maize yield during 1969-1971 in India was 1.05 tons/ha as against 5.15 in the United States (USDA 1975b:122-143).

By planting new high yielding dwarf wheat varieties and using a statistical management strategy that considers climate, stored soil water, and rate of nitrogen fertilization, wheat farmers in northwestern rainfed areas of the United States have almost doubled their yields to over 2.3 tons/ha in recent years. Similar systems using summer fallow have been successfully introduced in Turkey and Jordan. The proposed research could do much toward improving the prospects for higher yields in other developing countries. Even raising yields in the developing

Table 5. Worldwide areas of major grain crops

Crop	Millions of hectares	Responsible international institute
Wheat	217.2	CIMMYT*
Rice (paddy)	134.9	IRRI
Oilseeds	120.8	
Millet and sorghum	113.4	ICRISAT**
Maize	112.9	CIMMYT
Barley	82.2	CIMMYT
Pulses	62.5	CIAT, IITA, ICRISAT
Oats	31.2	
Rye	19.7	CIMMYT
TOTAL	894.8	

*International Maize and Wheat Improvement Center.
**International Crops Research Institute for the Semi-Arid Tropics.

Source: Gray (1975).

countries to a point midway between their current level and the level in the high-income countries would eliminate the current food deficit.

IMPLEMENTATION

Water conservation techniques for rainfed agriculture range from basic general principles having wide application to innovative practices that apply to rather specific crop, cultural, and ecological conditions. What is the best way to stimulate the development of total water management plans tailored to specific farming systems? How does one draw on the steadily increasing base of technical knowledge and enhance the transfer of innovative practices? The existing international crop research centers, particularly the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) and the International Center for Agricultural Research in Dry Areas (ICARDA), can contribute both in identifying physical processes and in developing information needed to design improved crop-soil-weather systems. In-country organizations must be involved in systematically collecting and contributing information, particularly about crop-soil-weather interactions; and in extending pertinent information and adapting it innovatively. Much about the physical processes and their application to upland grains and rice is already known and can be applied through technical assistance programs. These efforts need to be multiplied, especially for upland cereals. For grain legumes and other crops the list of cultivar species is a long one, and transferable knowledge is much more limited. Emphasis needs to be given to (1) facilitating the development of in-country research and extension capability, and (2) improving the network for transfer of technology. These aspects of the programs of the U.S. Agency for International Development (AID) and FAO, and the combined efforts of international centers with related U.S. research institutions, such as the wheat research at Oregon State University, should be strengthened. Studies should be implemented on how to improve network capability, utilizing special teams or workshops, perhaps sponsored by the Consultative Group on International Agricultural Research (CGIAR) or one or more of its members. This would be a long-term program, probably requiring an increased investment of $1 million the first year and $12 to $15 million in about four years. The United States should move aggressively to get the program started and could probably interest other donors in providing part (say 50 percent) of the funding.

Crop-water-soil systems research is viewed as an important part of this task as a means of designing research, transferring results, and stimulating indigenous innovations. The requirements for such a program from a water management point of view (as described in Profile 8) must be integrated with the systems approach outlined by Study Team 8. A multidisciplinary group should be assembled to organize and implement this phase of the program. Leadership and coordination might be provided through contractual arrangements with an appropriate institution or group of institutions, following one or more planning workshops organized by NAS, AID, or USDA. By selecting one or two main crops as a starting point (say maize or soybeans), significant results could be expected in three years at a cost of $3 to $5 million per year. Following this first effort, depending on the experience gained, the program should be expanded at an appropriate level of funding.

The management approach described here has the advantage of involving private enterprise, particularly the seed industry, which already conducts many field trials on literally thousands of seed varieties. Any seed production enterprise will want its products included in the information bank and will be interested in reducing the costs of field testing. In previous work on corn seed, private companies have cooperated enthusiastically and have agreed to utilize open pedigree and indicator strains in place of closed strains. They would be expected to share some of the costs.

The optimal strategy for mobilizing the research program would be to select research sites at existing facilities according to the following criteria: (1) technical competence and support already available, and the prospects for upgrading it; (2) location within an important "reference climate"; and (3) location on a "benchmark soil."

Only one or two research sites are needed for each climate and soil of major importance. By selecting existing facilities where there is a reasonable expectation of competent performance, maximum advantage can be gained from the field research. From this base, the experiments could be broadened to more extensive field trials and demonstrations. The probability of success is high if an adequate commitment is made, but is low or negligible otherwise. The target--closing the gap to a point midway between average yields in the developing countries and yields achieved under similar ecological conditions in the high-income countries--should be achievable in 6 to 10 years for major cereal crops.

Profile 4

TECHNICAL LAND CLASSIFICATION SYSTEMS

The 1.4 billion cultivated hectares and the 2.0 billion uncultivated but potentially arable hectares of land in the world today include thousands of kinds of soils. Each kind has a unique set of characteristics, and the importance of these characteristics varies according to the use made of the soils. In detailed soil surveys of counties in the United States, the different kinds of soils recognized and mapped commonly range between 100 and 200. Dealing with soil information, especially for large areas, is difficult because of the large number of soil types and the scientific nomenclature applied to them. It is so difficult that persons unfamiliar with soil science usually give up in frustration when they attempt it.

But a way has been found to bridge the gap between makers and users of soil surveys. Those who make soil surveys can group soils according to pragmatic objectives, i.e., make technical land classifications. Many such classifications (groupings) can be made from a modern soil survey, and technology exists for using computers to assist in the preparation of land classification maps based on soil surveys.

The Land Capability Classification System (Klingebiel and Montgomery 1961), used widely for many years in the United States, is one example of a technical land classification system. Even with its shortcomings, it has proved useful, especially for providing a quantitative evaluation of the soil resources of counties, states, and drainage basins, as well as the entire country. Through interpretation of detailed soil surveys (which now cover about half of the United States) and sampling in unsurveyed areas, soil scientists have determined how many hectares make up classes I to VIII. The U.S. land inventory is unmatched by that of any other large country.

The Land Capability Classification System, however, is not helpful in determining alternative uses of the same tract of land. There is a great need for this

kind of comparative evaluation, particularly in the developing countries. Most tracts of land can be put to several kinds of use. A technical land classification is needed for each kind of reasonable alternative so that government officials and others concerned with land use decisions can evaluate comparative suitability.

While land use classification systems should be applicable globally, coverage by every system need not be worldwide. A land classification system for wetland rice, for example, needs to be applicable only to those parts of the world for which such use of the land is one of the reasonable alternatives. The areas of the world to which a classification is applicable, therefore, will vary according to the nature and objective of the classification.

Basic to the development of technical land classifications, including the making of maps, is the system of soil classification set forth in a new publication, Soil Taxonomy: A Basic System of Soil Classification for Making and Interpreting Soil Surveys (USDA 1975c). This soil taxonomy is the result of many years of development and testing in the United States with substantial help from foreign soil scientists. Although developed for use in this country, it is international in scope and is being used in many countries. While it is a natural classificaticn in the same sense that plant taxonomy is a natural classification, it also is a practical classification; the soil taxa (classes) are well differentiated in relation to alternative soil and water management systems. This means that soils classified according to the U.S. soil taxonomy also are classified so as to be usable directly for technical land classification. Other information, such as climatic data, may be needed too, but good information on soils always can serve as a base for making the kinds of land classifications envisaged in this research profile.

Another important potential advantage of the U.S. soil taxonomy is that it can serve as a better basis for the transfer of soil and water management technology. Such use of the taxonomy goes hand in hand with its use for making technical land classifications. Therefore, use of this taxonomy ought to be encouraged, or even required, where the United States supports soil and water management research.

RESEARCH NEEDS

Modern soil surveys provide the best known inventories of soil resources. Many technical land classifications, including maps, can be derived from

such surveys. Every modern detailed soil survey in the United States contains a dozen or more land classifications which can be translated into maps as needed. Although significant progress has been made worldwide in soil surveying at medium and small scales (1:100,000 to 1:1,000,000), important areas remain unmapped even at these scales. Hence, soil surveying--classification, mapping, and interpretation--should be encouraged, especially in the developing countries, in order to improve recorded knowledge of the nature, quality, and geography of soil resources. Since the technology for making soil surveys is well advanced, however, augmentation of ongoing research in this field is not suggested.

What is suggested is substantially increased research and development of technical land classification systems--what kinds to make, how to make them, and how to use them--which would be adequate for global use and relevant to food production. The technology for such systems, while reasonably advanced in the United States and a few other countries, is lagging elsewhere. Moreover, most of the technology that exists is provincial rather than global in application. More research and development usable internationally for land use planning is needed at all levels--farm, community, provincial, national, and international. They are needed so that, as soil surveys are completed, appropriate and full use can be made of them by government officials and others who make land use decisions, thereby reducing or preventing costly mistakes and human suffering.

Technical land classifications, including maps, are derivable primarily, although not exclusively, from soil surveys. The classification of land to show which soils are naturally wet and which are not is a kind of technical land classification derivable entirely from soil surveys. A classification of land according to suitability for wetland rice production is an example of a land classification for which information on climate and hydrology needs to be combined with that derivable from soil surveys.

Three Research Phases

The first research phase is the determination of the family of technical land classification systems most urgently needed. A classification made for one purpose seldom, if ever, can serve another. Hence the list of possible classifications can be much too long for all to be designed at once. A selection of a dozen, or preferably less, of those most needed should be made first, and the purpose of each objectively set

forth. Among these might be land suitability for wetland rice production, for production of dryland grain crops, and for irrigation.

The second phase is the development of the technical land classification systems themselves. This entails specification of map scale, definition of categories if more than one, definitions of classes within each category, and the setting forth of the land use significance of each class in language understandable to the intended users. Supportive texts and the kinds of data that can be helpful should be set forth too.

The third phase is the specification, for soil scientists, of criteria for placing (grouping) kinds of soils in each class of each technical land classification system. The Guide for Interpreting Engineering Uses of Soils (USDA 1971) is an example of the kind of guidance that needs to be developed. A start has been made (Brinkman and Smyth 1973) for developing international guidance for technical land classifications in general, but additional guides are needed which are specific to each classification system. The guides also should contain specifications for handling climatic data and data from other disciplines. Moreover, the guidance should be such that the resulting information is amenable to storage in a computer system so that land classification data easily can be used with other kinds of information (data banks) as one of several tools to achieve the best possible land use decisions. For example, a land classification system for cropping should be relevant to and usable with data about crop tolerances of stress caused by unfavorable climatic and soil characteristics.

Research and development on technical land classification systems must go on indefinitely because such systems, and the maps made according to them, are temporary. Both the systems and the maps become obsolete due to advances made in soil and water management, changes in social or economic conditions, or other reasons. The capability for updating old systems and designing new ones therefore needs to be maintained.

IMPACT

The use and management of soils should be compatible with the characteristics of the soils and should be such that the soils are maintained or improved as resources for growing plants. Many soils are not physically suited to cultivation, many have limited suitability and then only under exacting

management, and some have a wide range of suitability and are not exacting in management. Land use or land management not in harmony with what the soil requires for its maintenance results in loss or other deterioration (Eckholm 1976). Perhaps the most common, and certainly the most visible, kind of loss is accelerated erosion, which is a hazard on most sloping land. Less visible but nevertheless important is salt accumulation in irrigated soils in arid lands. There also are other kinds of deterioration, such as waterlogging, loss of fertility, bad tilth from cultivation when too wet, and compaction from heavy machinery. This list could be extended.

More commonly than not, decisions about the use and management of soils are made without due regard to the characteristics of the soils themselves. Consequently, many unwise decisions are made unwittingly, resulting in uneconomic production or crop failures, wastage of precious soil resources, and untold misery for people trapped in unprofitable or failing land development projects. Good, relevant technical land classification maps with supporting texts and data will not guarantee an end to unwise land use decisions, but they would make possible a drastic reduction in such decisions.

The impact, therefore, of research and development in bringing about the production and use of relevant land classification maps and supporting data lies mainly in a drastic reduction in unwise land use decisions unwittingly made by well-intentioned people. The monetary savings easily can amount to hundreds of millions of dollars, and the reductions in human misery also can be substantial.

The major use, or at least an important use, of technical land classification maps and data is by local, provincial, and national governments in formulating land use policy. Such maps and data will be a relatively new kind of input in most countries. Integrating new information into the formulation and execution of policy takes time. The impact, therefore, will take time too. While the impact should be significant in the short run (before 1985), the major impact is likely to manifest itself after 1985.

IMPLEMENTATION

Implementation of this research requires interdisciplinary cooperation between (1) soil scientists familiar with soil surveys, (2) climatologists and scientists in other fields, and (3) potential users of land classification maps and data.

The third group may, and probably should, include government officials who may not be scientists but who

have responsibilities in administering programs affecting land use. Experience in the United States indicates that involving the third group is the only way to be sure that the land classification systems are in fact usable, and likely to be used, by the intended users. The participation of the third group is especially needed for the first two phases, the determination of the initial family of classification systems and definitions of the categories and classes of each system. Their participation is less urgent in the third phase, the specification of criteria regarding soils and the determination of climatological or other data needed to execute the classification systems. Economists, however, may be needed in the third step.

The nature of the research is such as to suggest, as a focus for implementation, consideration of the National Cooperative Soil Survey (NCSS), the national leadership of which is vested in the Soil Conservation Service. Several other federal agencies, all state agricultural experiment stations, and many local units of government cooperate in NCSS work. NCSS scientists have participated in FAO-sponsored international conferences on soil surveying and land classification. A major portion of the U.S. expertise in land classification resides in NCSS and the institutions, especially the state agricultural experiment stations, participating in it.

At least several countries in addition to the United States have an interest and some competence in land classification. FAO is definitely interested and other international establishments including IRRI may be also. As the areas most in need of technical land classification are outside the United States, international participation is essential. International research centers, such as IRRI, could be quite helpful, but participation by the relevant agencies of at least some of the target countries is essential.

An international institution with access to computers certainly would be helpful, and eventually might be essential, in helping to process raw data into land classification maps and to take on the responsibility for leadership in the updating of the first family of land classification systems and the designing of new ones when necessary.

It is quite likely that the three research phases outlined in this profile could be completed in 5 to 10 years at a probable cost of $5 to $10 million. This estimate does not include the actual classifying of land tracts nor the preparation of relevant maps. Neither does it include the cost of continuing the

research and development beyond the initial family of technical land classification systems.

Profile 5

PLANT ADAPTATION TO SOIL AND WATER STRESSES

Agricultural research has largely been geared toward overcoming various soil, climatic, and water limitations and breeding plant varieties that yield well when grown in well-fertilized and appropriately watered soils. Although the world owes much of its present food supply to this approach, limitations in energy-related inputs and the need to bring vast new areas of arable land into cultivation suggest that an alternative strategy be pursued. This strategy is to adapt the plant to the soil's limitations rather than building up soil to compensate for plant limitations.

With the exception of breeding for drought tolerance in semiarid areas, however, this approach has largely been ignored until recently. The advent of the energy crisis has spurred the interest of plant breeders and soil scientists in breeding new varieties tolerant of such soil stresses as short-term drought in humid climates, aluminum toxicity, phosphorus deficiency, salinity, iron deficiency, iron toxicity, and zinc deficiency. Moreover, adapted plant varieties can be used by small farmers without the high levels of investment and skill necessary for environmental modifications. Successful breeding for aluminum tolerance (DaSilva 1976) suggests that it is no longer invariably necessary to breed for optimum soil conditions. In addition, there is also the possibility that species not now considered worthwhile in a commercial sense can be effectively integrated in a total resource management system.

This profile proposes that this approach be exploited fully. The word "tolerance" is used to mean adequate plant growth in the presence of higher levels of stress than previously used in selection programs. "Tolerant" varieties would need smaller inputs of fertilizer, lime, irrigation, or drainage than "sensitive" varieties to produce acceptable yields. Using tolerant plants, therefore, will make better use of already scarce agricultural inputs.

RESEARCH NEEDS

Tolerance to Soil Acidity Stresses

Most plants suffer when grown at soil pH values lower than 5.5 because of aluminum and/or manganese toxicity, and/or calcium, magnesium, or phosphorus deficiency. These factors do not always occur simultaneously, but tolerance to aluminum, and to phosphorus and calcium deficiency, seem to be related (Foy 1974). Crop and pasture species that have evolved or been selected by breeding programs in acid soils have developed certain degrees of tolerance, while those developed in neutral or alkaline soils are usually very susceptible to soil acidity stress. Farmers in acid tropical areas have selected the most tolerant species, such as cassava, rice, and many pasture species, and the most tolerant varieties within these species. Australian scientists adapted this concept to their country's acid tropical grasslands and have made profitable ventures using tolerant species, such as Stylosanthes guyanensis, instead of the more demanding alfalfa (Andrew et al. 1973). Work at Beltsville, Maryland and Oregon State University has shown that inheritance of susceptibility to aluminum toxicity in barley is controlled by one or two genes (Foy 1974). The physiological mechanisms, however, are very complex and need to be understood. Recent research on the acid tropical savannas of Colombia (Spain et al. 1975) and Brazil (North Carolina State University 1974:128-142) show that tolerant varieties of several tropical crops can be identified. Mass screening methods have been developed for testing tolerance of aluminum toxicity and phosphorus deficiency.

These results show the possibilities for adapting crop plants to acid soil conditions and underline the need for a systematic effort (1) to determine the differential tolerance of most varieties of major food and pasture crops, and (2) to cross tolerant but usually low yielding varieties with sensitive high yielding varieties to produce, in effect, a new "Green Revolution" for acid infertile soils.

Liming does not solve aluminum toxicity problems in many instances. In many developing countries lime is either extremely expensive or not available at all. Even where lime is cheap and available it can only eliminate aluminum toxicity to the depth applied, which is commonly the top 15 cm, or at most, 30 cm. Lime does not move down and therefore cannot affect aluminum toxicity in the subsoil. Subsoil aluminum toxicity is very common in many acid soils of the tropics and the southeastern United States. The development of

aluminum-tolerant varieties will stimulate deeper root development which, in turn, will alleviate temporary drought stresses by allowing plants to utilize stored subsoil moisture from depths where aluminum toxicity previously impeded root growth (North Carolina State University 1974:128-142).

Drought Tolerance

Similar varietal differences in drought tolerance have been identified in semiarid areas. Temporary droughts also affect yields in humid regions, and varietal differences in drought tolerance have been identified in rice, one of the crops most susceptible to water stress. Work by IRAT in Africa and a comprehensive program at IRRI (International Rice Research Institute 1972-74) show that certain early maturing varieties with deeper and more aggressive root systems can avoid and/or tolerate the frequent water stresses that occur during the rainy season. The physiological mechanisms are not well understood nor the genetics established. The IRRI program shows the tremendous potential rewards from a systematic search for drought tolerance. Plant breeders in humid regions have unintentionally selected varieties more tolerant to water stress simply because they performed best in plots when the stresses occurred. In most cases trials of plant varieties are conducted with adequate water availability, and drought-tolerant varieties cannot be identified.

Other Stresses

It is known that there are varietal differences in tolerating soil salinity, high sodium saturation, temporary flooding, extreme temperatures, phosphorus deficiency in nonacid soils, zinc deficiency, iron deficiency, and iron toxicity (International Rice Research Institute 1972-74). Similar arguments made in the previous sections are applicable to the whole spectrum of environmental stresses.

Search for and Adapt Species Not Now Considered "Commercial"

In addition to genetic selection and/or breeding of commonly grown crops for stress tolerance, the opportunity now exists to identify species growing naturally under stress conditions for which economic uses can be developed. Examples in the recent

literature include the wing bean and jojoba bean (NRC 1975c). The first apparently has a significant potential for enhancing food production, the second for increasing cash income.

With respect to salt tolerance, for example, such a search may lead to plants that can be grown where no currently available crops can. It also may permit the development of integrated systems of water and land utilization that make better use of the total resources at hand. For example, rather than dispose of drainage water too saline to grow conventional food crops by means of expensive conveyance systems or evaporation ponds, it could be used to grow biomass for methane production, wildlife habitats, or direct use as fuel. Success could lead to alternate solutions to water quality problems of significance in the United States (e.g., in California and the Colorado River Basin) as well as abroad.

Need for a Systematic Effort

A major, worldwide research effort of this kind should be considered a matter of high priority in an energy-short world. It would have the following components:

- -- identification of the physiological mechanisms and genetics of various plant tolerances of soil-related stresses and the interaction among stresses;
- -- cataloging of species and varieties according to their tolerance of critical stress levels (percent aluminum saturation, bars of soil moisture tension, etc.) in order to permit worldwide extrapolation of this information;
- -- incorporation of tolerances into breeding objectives;
- -- development of mass screening selection methods;
- -- tests of different varieties in the field at several stress levels at a sufficiently large number of sites to firmly establish tolerances and yield potential;
- -- incorporation of the results in technical soil classification data banks.

IMPACT

The systematic research and development effort just outlined is likely to have three major impacts: (1) increased plant yields under stress conditions; (2) more efficient use of high energy inputs, such as

fertilizer, lime, irrigation, and drainage; and (3) economic cultivation of lands where previous varieties or species failed to survive. These impacts are related and what follows is an attempt to discuss them within the context DOCUMENT of the different stress factors under consideration.

On Food Production and on Using Inputs Efficiently

The development of high yield crop varieties tolerant of relatively high aluminum saturation levels (50 to 80 percent) is likely to have a profound impact on food production in acid soils in both temperate and tropical regions (see Profile 1). Success in this endeavor is virtually assured.

It would not be unreasonable to estimate an annual increase in cereal grain production of 1 ton per hectare from varieties able to tolerate high levels of aluminum. There may be as many as 500 million hectares of land in the tropics of South America, Africa, and Asia where such an outcome could be obtained. Thus success from this line of investigation could be expected to produce an additional 500 million tons of cereal grains annually.

The use of aluminum-tolerant varieties is likely to decrease but not eliminate lime requirements. Properly determined liming rates will neutralize the exchangeable aluminum to the depth at which it is incorporated. In many acid soils of the southeastern United States and the tropics, the subsoil has toxic levels of aluminum that impede the root development of sensitive varieties just as if they were physical barriers. The use of aluminum-tolerant varieties, therefore, will permit deeper exploitation of the soil and the use of available water and nutrients in the subscil.

A recent example from the oxisols of the cerrado of Brazil illustrates the concept of increasing the efficient use of liming. In this acid soil a conventional grain sorghum variety required 6 tons/ha of lime to produce a grain yield of 5 tons/ha, but an aluminum-tolerant variety required only 1 ton/ha of lime to produce the same grain yield (North Carolina State University 1975). The low lime rate supplied needed amounts of calcium and magnesium as fertilizer. The tolerant variety also suffered less drought stress because of its deeper root system. Similar impacts can be expected for grain varieties that can utilize low levels of available soil phosphorus more efficiently (Salinas and Sanchez 1976).

The benefits that would accrue from the development of crop varieties better able to avoid or withstand drought periods is obviously very great. It is extremely difficult, however, to put any precise measurement on what the current economic losses are, partly because of the difficulty in defining what drought is. Furthermore, the probability of success is considerably less than in the case of aluminum tolerance. For present purposes a useful, if rough, estimate can be obtained from a look at what happened in India during the drought period from 1965 to 1967. Other factors were at work that normally would have tended to increase production over this period, but yields during these two years appear to have fallen below the trend line by perhaps 15 percent. This can be taken as a rough measure of what the aggregate losses might be over a large area with a broad range of farming systems in climatic conditions ranging from very humid to semiarid. If the effects of drought can be reduced, farmers can utilize fertilizer and other inputs with less fear of loss. Thus the net gain may be greater than avoidance of loss alone would suggest (see Profile 4 of Study Team 1).

The improvement of crop tolerance of soil salinity and the use of new crop species better adapted to saline soils is likely to have a large impact, both in increasing food production and allowing the cultivation of land currently considered unsuitable for cultivation. For the western United States alone, it has been calculated that increasing the salt tolerance of a range of crops (so that the same yield would be obtained if the salinity of the irrigation water were increased by 350 milligrams per liter) would result in a potential benefit of $80 million a year (U.S. Salinity Laboratory 1975). Very preliminary results suggest that this approach will succeed, but substantial results should not be expected in less than 15 years.

Timing Considerations

The preliminary results already in hand suggest that a major portion of the impact can be achieved in the short run (0 to 15 years). Like other aspects of crop improvement, improved tolerance requires sustained efforts through the years, but unlike breeding for insect and disease tolerance, researchers will not have to contend with pathogen mutations.

Geographic and Scale Considerations

The proposed research will have global applications since the stresses under discussion occur throughout the world. Drought problems are worldwide; aluminum toxicity problems occur in high rainfall areas of both tropical and temperate regions, while salinity problems occur globally in low rainfall areas. This research will be of particular significance to the United States and other countries as a means of conserving high energy-related inputs and nonrenewable resources like phosphate deposits. Although the technology is scale-neutral, some of the main beneficiaries will be farmers who lack the funds to invest in high energy inputs.

IMPLEMENTATION

A systematic, worldwide research and development effort to adapt plants to soil and water stresses means a new approach to agricultural research. The interdisciplinary nature of this research also should be recognized. A joint effort by plant breeders, plant physiologists, and soil scientists is needed throughout the world. The major international centers (IRRI, IITA, CIAT, the International Potato Institute [CIP], CIMMYT, and so forth) and national centers for the various crop and pasture species should take the lead for the appropriate commodities, with connections to basic research centers in U.S. universities and the Agricultural Research Service, such as the Plant Stress Laboratory in Beltsville, Maryland. These efforts should be linked to an international network of field experiment sites similar to those developed to study plant tolerance to disease and insects. The ultimate test is in the field, and tests should be conducted at several stress levels instead of under optimum conditions, as traditionally has been done. U.S. institutions and U.S.-supported institutions overseas, along with Australia, have been the leaders in tolerance research and are expected to continue to provide such leadership. Commercial seed companies should be involved at the developmental stage.

The yearly cost of a full research network is difficult to estimate, but the magnitude of the task suggests that a total of $20 million per year is required to cover all the major important food and pasture crops on a systematic basis for the next 15 years. Required funding thereafter can only be estimated after more experience is gained, but is likely to be less.

Profile 6

BIOLOGICAL UTILIZATION OF THE RHIZOSPHERE

The region of the soil in which micrcflora are affected by the presence of plant roots is called the rhizosphere. It is an incredibly complex and ever-changing region. As the roots of plants grow they secrete a mucilagenous material that coats their outer surfaces and eases their passage through the soil. A number of other substances also are excreted from the roots, including sugars, amino acids, organic acids, nucleotides, flavones, and phenolic compounds. Moreover, portions of the root tissue are ruptured by abrasion, releasing additional organic material. Recent estimates indicate that up to 18 percent of photosynthetically fixed carbon may be released by the roots of plants growing in unsterilized soil. This array of materials provides a rich substrate, and the various microflora of the soil respond differently to this material, the nature of which varies greatly with species, with distance along the roots, and with different stages in plant development. The ensuing microbial populations in this region thus differ enormously, both quantitatively and qualitatively, from those in bulk soil. Furthermore, these populaticns may change dramatically with time, with plant species, and with the soil and aerial environment.

In addition to the biotic components, the chemical attributes of the rhizosphere differ greatly from those in bulk soil. This results from (1) the incorporation or metabolic transformations of organic material by the microflora, (2) the variable secretion of hydrogen and hydroxyl ions from roots and microbes, and (3) the movement via diffusion and convection of soil- and fertilizer-derived nutrients. The physical state of the rhizosphere also differs from that of bulk soil due to (1) the compression of mineral and organic components as root apices penetrate the soil, (2) the release of organic materials from the roots which affect the physical state of the soil, and (3) the marked changes in gaseous composition as root

respiration and intense microbial activity exert their effects on the consumption of oxygen, release of carbon dioxide, and generation of volatile metabolites.

It is the complex region of the rhizosphere, not the bulk soil medium, in which the roots of plants exist and to which they respond. The microflora that flourish in the rhizosphere, some of which subsequently infect the root tissue, can either markedly promote or markedly restrict the development of plants. But our understanding of this crucial region is quite limited. This is because of the difficulty of gaining access to it without simultaneously altering it severely, and because of the complexity and dynamic nature of its biotic, chemical, and physical attributes. It has been documented that microflora can influence plant growth greatly; what remains to be discovered is how to manipulate and control the ecology of the microflora in a predictable way. The quantities and proportions of organic materials excreted, the apparent marked differences among species given the stage of plant development and environmental conditions, together with the varying nutritional requirements of the soil microflora and the beneficial effects caused by promoting the proliferation of specific microorganisms in the rhizosphere, all combine to indicate that selective manipulation of the rhizosphere ecology to promote plant growth should be possible.

There are two general processes of crucial importance in crop production, the optimal functioning of which is directly a consequence of the ecology of the rhizosphere. First, extremely important are the associative symbioses in which microorganisms (diazotrophs) capable of reducing atmospheric nitrogen to ammonia infect the roots of a host plant which itself is not capable of nitrogen fixation. In these symbioses the host provides a favorable environment and energy for the diazotroph, and receives at least part of the reduced nitrogen from it. There are two kinds of associative symbioses. One is the *Rhizobium*-legume association in which the bacteria invade the roots, nodules are developed, and the bacteria receive photosynthate and other substances from the shoots and carry out their nitrogen-fixing activity in highly organized, close association with the host's root cells. Some nonlegume nodule symbioses also occur, although the diazotrophs have not yet been positively identified and seem not to be important with crop plants.

The other associative symbioses are not so closely organized as those involving nodules. They range from nonnodular, intracellular root-microbe associations, such as *Spirillum* with *Digitaria* and *Zea* roots, to rhizosphere associations in which the nitrogen-fixing

microbes exist in the rhizosphere and receive their nourishment from excreted products of the roots. All associative symbioses are affected by the characteristics of the rhizosphere, the nodular systems and the nonnodular intracellular one because they are established by means of infection from the diazotrophs which proliferate in the rhizosphere, and the others because the diazotrophs must be sustained by, and carry out their functions, in the rhizosphere. For the former, rhizosphere conditions must be such as to maximize infection of the host until the diazotrcphs reach the more sheltered environment within the root tissue. For the latter, the rhizosphere conditions obviously must be maintained in a state that encourages the growth and nitrogen-fixing activities of the diazotrophs to the relative exclusion of other microflora, if need be.

Numerous studies have recently been made which document the critical importance of enhancing biological nitrogen fixation to the maximum extent possible. The considerations here involve (1) the necessity for high rates of crop production for world food supplies, and (2) the increasing cost of the production and transport of fertilizer nitrogen, the consumption of fossil fuels in its manufacture, and the pollution hazards that may result from its inefficient use. These intensive studies also emphasize that enough is now known to generate optimism about the prospects for enhancing efficiency of Rhizobium-legume symbioses and to expand the other associations to a considerable extent. It should be emphasized, however, that these advances require the optimization of specific events in the rhizosphere for specific purposes, i.e., an optimal development of specific microorganisms at specific times, either to effectively infect the host or for the organism to flourish and function in the rhizosphere.

The Rhizobium-legume symbioses afford the largest opportunity for a substantial increase in nitrogen fixation in the near future. This is because of the intimate relationship between the diazotroph and the host, which restricts competition with other microorganisms and creates an efficient pathway for the flow of materials to and from the nodules after infection takes place. The major research efforts should include (1) optimizing the effectiveness with which the Rhizobium strains are able to infect major food grain and forage legumes; (2) optimizing the efficiency with which nitrogen is fixed per unit of energy consumed in the process; and (3) prolonging the capability for nitrogen fixation longer into the fruit-filling period.

Some doubt currently exists as to the magnitude of nitrogen fixation in the nonnodular extracellular root associative symbioses and in the rhizosphere symbioses. The uncertainty concerns the oxygen tension within the roots' cells where the diazotrophs exist and the apparent absence of a mechanism to keep this oxygen concentration sufficiently low. Recent assays for nitrogenase activity have not resolved the matter, and there is an urgent need to demonstrate unequivocally by $^{15}N_2$ methods the actual amount of nitrogen fixed by these processes. If such studies clearly show significant nitrogen fixation, a substantial effort should be made immediately to delineate the diazotrophs involved, to extend the range of host species and environmental conditions under which effective inoculation by the diazotrophs can take place, and to enhance the efficiency of the process in terms of the amount fixed and the energy expended.

The second general rhizosphere process that may have considerable impact in crop production is the preferential development of fungal associations with roots, i.e., the mycorrhizae. Many plants, including most of the major food crops, are capable of developing these associations. Marked increases in crop growth have resulted in soils of low or moderate fertility when roots have been induced to form endomycorrhizae. The oxisols and ultisols of the tropical savannas are generally deficient in phosphorus and other relatively immobile nutrients, and transporting fertilizers to these regions is expensive. Some dramatic increases in crop yield on these and other phosphorus-deficient soils have been observed when root systems were infected with the appropriate fungus. Most mycorrhizae on crop species are of the endotrophytic type with extensive hyphal development, which extends from the host and effectively permeates the soil. Because the transfer of nutrients from the fungus to the host is quite efficient, the association results in an enormous increase in the surface area of the root system at relatively low cost to the host. In consequence, a much more extensive exploitation of the soil takes place, and the total phosphorus requirement of the plant can be met at very low levels of soil supply. Evidence is also available to suggest distinct advantages from mycorrhizae in obtaining the required amounts of other relatively immobile nutrients. Potential advantages are not limited to monoculture; enabling low root density species to compete with high root density species in mixed culture also seems feasible. Immediate attention should be focused on selection and development of fungal strains able to sustain themselves in the rhizosphere and able to

infect the roots of important crop species under specific soil and environmental conditions.

RESEARCH NEEDS

Optimize Nitrogen-fixing Associative Symbioses

Increase the efficiency of nitrogen fixation (quantity of nitrogen fixed per unit of carbohydrate consumed) by Rhizobium-legume symbioses of the major food and feed legumes. Current evidence with soybeans and peas indicates that the capacity for nitrogen fixation in Rhizobium-legume symbioses is limited by the supply of photosynthate. This is especially crucial after the onset of reproductive growth, when diversion of the photosynthate from the root system to the developing fruit occurs. Furthermore, recent evidence suggests that 40 to 60 percent of the energy supplied to the nodules and consumed in nitrogen fixation may be used for the reduction of protons and the evolution of hydrogen at the expense of nitrogen reduction. A restricted photosynthate supply and inefficient use of the energy that is available for nitrogen fixation combine to impose a severe limitation on the total nitrogen input; seed yields are thereby restricted even under optimal environmental conditions. The reasons for these limitations in efficiency of the nitrogen fixation process need to be established, and the extent of the limitations needs to be thoroughly documented under soil and climatic conditions where legumes are grown. It must be emphasized that uncritical reliance on the currently employed acetylene reduction method for estimating nitrogen fixation in these studies will be a mistake. Rigorous verification of the applicability of the method using $^{15}N_2$ procedures is mandatory in all studies. Attempts should be initiated to develop legume cultivars capable of effectively proportioning photosynthate between the developing seed and nodules so as to sustain substantial nitrogen fixation longer into reproductive growth. Intensive searches in the tropics for cultivars of legumes having this attribute may be rewarding.

Optimize the matching Rhizobia and legume genotypes for effective inoculation and efficiency of nitrogen fixation by the major food and feed legumes. Rhizobia survive saprophytically in the soil, and the subsequent infection and development of effective nodules are extremely complex processes that require a remarkable adaptation on the part of both the bacteria and the host. The development of superior Rhizobium strains for specific soil conditions for each major legume

should be advanced. Such strains must be able to compete effectively with the native Rhizobia and other microflora of the rhizosphere so as to be able to establish the dominant symbioses with the host. Such strains also should be able to continue to fix nitrogen efficiently in the presence of significant quantities of soil nitrogen so that legumes can profit from both nitrogen sources, i.e., they must be able to prevent repression of nitrogenase. Extending the promiscuity of Rhizobia to a number of legumes would be highly advantageous.

Characterizing the genetic and molecular determinates of infection and efficiency in fixation of both the diazotroph and the host will be fundamental to significant progress in all of these endeavors. Rhizobia can now be cultured so as to synthesize nitrogenase in the absence of the host, offering promise of the opportunity to select mutants that are highly efficient in nitrogen fixation.

Optimize nonnodular root-microbe and rhizosphere associative symbioses in the major nonlegume food crops. That associative symbioses other than those of nodule systems can result in nitrogen fixation is clearly established; it is not so clear how much nitrogen is fixed by these processes. It is necessary to establish unequivocally by the use of extensive $^{15}N_2$ experiments how much nitrogen these processes contribute under conditions where the symbioses currently are found. Intensive efforts should then be made to increase the efficiency of the processes by developing effective diazotrophs and by developing host cultivars which optimize the associations under specific soil and aerial environmental conditions. Transfer of nitrogen-fixing genes from one bacterial species to another has been accomplished, making it possible to introduce nitrogen-fixing capability into organisms which may more effectively infect higher plants. Development of host cultivars whose root metabolism results in the synthesis and/or excretion of compounds specific for the desired diazotroph appears to be a promising approach. Optimization of these processes also will depend upon diazotroph strains that are capable of proliferation in the rhizosphere and root cells of the host against the intense competition of endogenous microorganisms and under a wide range of soil and environmental conditions. It also will depend upon the host being able to partition photosynthate in such a way as to provide continual sustenance to the diazotrophs during a large part of the growing period.

Increase the Efficiency of Nutrient Absorption and Crop Growth

Develop methods for introducing and maintaining highly efficient mycorrhizal fungi for particular plants and soils. Marked crop responses on soils of low and medium fertility have been obtained by inoculation with endomycorrhizal fungi. Many of the naturally-occurring fungi are not effective, and marked differences have been noted among selected fungi. An intensive effort is needed to define fungal strains for particular soils and crops. Methods for producing appropriate inoculum are well advanced, but methods for application need to be worked out for areas where large-scale production with little labor input occurs. Although there is good reason to be optimistic, uncertainty exists about how consistently the beneficial strains can be introduced, maintained in the rhizosphere, and develop effective infections. Soils vary greatly in selective suppression of certain microorganisms and in enhancement of others. The mycorrhizal fungi are no exception, and the controlling forces are not evident. A concentrated effort to develop sound principles of rhizosphere ecology is imperative for sustained advance in this promising field.

Establish how roots and rhizosphere microorganisms interact in moderating nutrient absorption and plant growth. Periodically there are reports which indicate that a definite advantage in crop yield results from soil inoculation with various bacteria and actinomycetes which do not fix nitrogen nor develop mycorrhizae. The experiments have been difficult to reproduce experimentally, but their occurrence in specific instances is unquestioned. Similarly, significant increases in plant growth following fumigation have occurred under conditions where the effects apparently could not be accounted for by the control of known pests or pathogens. The presence of subclinical pathogenicity is inferred. Some effects may be exerted by subtle alterations in root morphology or function, or by metabolic products of the rhizosphere microorganisms. Compounds formed in the presence of rhizosphere microorganisms have been shown to give a marked enhancement in manganese absorption by plants, and other similar effects are likely. Degradation or alteration of potential allelopathic compounds also are likely to occur in the rhizosphere. All of these events are poorly understood; when, where, and how they occur cannot be predicted. Yet their cumulative effects on crop growth are likely to be extremely important for sustained, predictable production at high levels, especially in mixed culture.

IMPACT

Two recent studies (Evans 1975, Hardy et al. 1975) have emphasized the crucial importance of increasing biological nitrogen fixation immediately. Briefly, forecasts indicate that 160 x 10^6 tons of fertilizer nitrogen will be required for crop production within 25 years, compared with about 40 x 10^6 tons used in 1974, unless a substantial change in the proportion of nitrogen supplied by nitrogen fixation relative to that supplied by industrial fixation occurs. The progressively increasing expenditure of energy and the cost and consumption of fossil fuels in the current manufacture of fertilizer nitrogen make it imperative to alter this proportion as soon as possible. It has been suggested that about 90 x 10^6 tons of nitrogen are currently fixed by agricultural soils, of which about 35 x 10^6 tons occur with legume crops. Increasing the acreage planted in legumes and pulses, and increasing their efficiency of nitrogen fixation from the approximate present value of 50 percent, could at least double the amount of nitrogen fixed and correspondingly decrease fertilizer nitrogen requirements while increasing yields.

More dramatic improvement would result if nitrogen-fixing capability could be fostered even partially in nonleguminous plants, since these constitute a much greater proportion of current world food crops. It seems reasonable to expect that up to 50 percent, and eventually 100 percent, of the nitrogen requirements of all crops could be supplied by nitrogen fixation. It is necessary to strive vigorously for this independence from industrial nitrogen fixation.

About 50 percent of the tropics are occupied by highly leached, low base oxisols and ultisols. Rainfall patterns and soil physical conditions permit good crop growth in these soils when their nutrient status is enhanced. Given the immense area involved, an enormous increase in world food production would result if the nutritional limitation can be met economically.

The development of efficient mycorrhizal associations, together with the development of cultivars more capable of utilizing low base soil nutrient supplies under acidic conditions, offers excellent promise for sharply decreasing the cost of bringing these areas into efficient production (Jackson et al. 1975).

IMPLEMENTATION

A substantial number of public and private organizations are deeply involved in studies of biological nitrogen fixation. The scope of the work ranges from fundamental to applied research. What is most needed is that the good laboratories be identified and be assured of solid financial support over a prolonged period of time in order to develop into centers of excellence. Perhaps a doubling of present financial support could be effectively used, with expanded attention being focused on food and feed legumes other than soybeans and on the potential for nonlegume nitrogen-fixing associations with major food crops. The present organizational framework is adequate, although a national center that emphasized nonlegume studies would be desirable.

Investigations on mycorrhizal associations are not as well funded as those on nitrogen fixation, although there is a solid core of scientists active in the area who realize the potential worth of these approaches in enhancing food production. These groups also need to be assured of sustained support in critically examining and developing methodologies for promoting effective and efficient infections in the major food crops on nutrient-poor soils. Since the greatest immediate benefit of this approach will be in the low base tropical savannas, an international center with specific responsibility for those studies should be established in one of the countries where the potential is greatest.

Profile 7

UTILIZATION OF IRRIGATION WATER

In most countries, irrigation of some type is desirable or necessary to improve and stabilize food production and nutrition levels. However, there have been (and continue to be) notable failures in the implementation of irrigation projects. Serious problems of water control occur on existing projects and can be expected on many under construction or planned. While these problems have much in common, each situation has its own characteristics which must be taken into account. Although the proper selection and management of irrigation methods is only one part of any complex agricultural system, it plays an important role in the activities and success of the family farm.

Irrigation efforts are undertaken both by individual farmers and by groups of farmers, usually as a result of government-sponsored programs. In most organized projects, emphasis is given to the construction of storage facilities, water diversion facilities, and main canals. Too often, little or no attention is given to the proper selection and subsequent management of conveyance systems for the distribution of water from main canals to farms, to irrigation systems on the farms themselves, and to onfarm management practices. The water delivery system is often operated without proper regard to farm needs (Reidinger 1974). These difficulties result in water use efficiencies which are seldom as high as 50 percent and more typically nearer 25 percent (Doorenbos 1975). More importantly, however, they result in low yields, sometimes as low as one-tenth that expected by planners.

U.S. and Israeli experience has shown that farm irrigation efficiencies of 70 percent (the current U.S. Bureau of Reclamation design assumption) to 95 percent are possible, and that 80 percent is not difficult to obtain technically. Experience also has shown that the actual efficiencies have increased very little in some

30 years. The reasons are complex, but they are more often institutional and economic than technical.

In view of the large investment expected in irrigation and drainage development in the future, growing limitations on resources (water, energy, fertilizer), and the often irreversible consequences of "wrong" decisions, finding solutions to irrigation problems is an urgent matter.

Kovda's estimate (see Introduction, report of this subgroup) may be excessive, but waterlogging and salination are likely problems for new systems as well as older ones. Attempts to meet the objectives of U.S. water quality legislation (P.L. 92-500) have revealed the high costs both of maintaining old habits and meeting new standards (e.g., the Central Valley drain in California and the Colorado River Salinity Control Plan, P.L. 93-320).

Even if a system is reasonably adequate in concept and construction quality, poor management can be expected to decrease the efficiency of water use by 50 percent (Doorenbos 1975) and result in waterlogging, salinization, erosion, reduced crop yields, plant pests, and human diseases.

Any attempt to choose or improve an irrigation design and management program must consider all of the relevant social, economic, political, and technical conditions. Too often, important considerations are overlooked, ignored, or their consequences undervalued. Irrigation system failures have been, to a large degree, the result of the improper matching of irrigation methods with the social, political, economic, and technical management capabilities of those responsible for the irrigation and farming system as well as the physical characteristics of the land and water for crop production. Such mismatches are bound to result in partial or total failure of the system.

Proper consideration of the important components of the various irrigation methods in terms of measurable site conditions can lead to greater success for both old and new systems. This process would be facilitated by systems approaches that take into account the full range of various irrigation methods, so that on-site recommendations or decisions can be made rationally and with due consideration of the many alternatives.

RESEARCH NEEDS

Our general objective is to increase the effectiveness of irrigation within acceptable environmental standards, an objective described in the following statement issued by the international

conference on crop productivity (Brown et al. 1975:222):

> Food production is the first demand one must place on the use of the soil and water resources, and irrigation plays a major role in this regard. Optimization, however, should be towards maximum food production while protecting the environment: minimum use of renewable resources, minimum degradation of environmental quality (if not enhancement), and minimum adverse impact on societal values. Achieving or approaching such a goal will require the application of present technology and development of new technology; as important, however, may be a redirection of the criteria for success used by scientists in their research and by all in implementation.

In the end, knowledge has to be applied innovatively to each site. Success will require a diagnostic approach based on as much relevant site data as can be obtained, including the views of farmers. Finally, results must be translated into action by human beings, individually and collectively. The chief principle to be kept in mind is that water supply and weather need to interact properly with cropping systems, which themselves may be modified to improve the utilization of weather and water supply.

Much of the technology that is being promoted to improve the effectiveness of irrigation throughout the world was instigated in the high-income nations. This technology reflects the economic, social, and political predilections of the countries where it originated, and it may be ill-suited to the developing nations. This general concept has been well illustrated by Schumacher (1975), who pleads for more emphasis on "intermediate technology" programs. Often, alternate technologies better suited to the developing nations have been bypassed, rejected, or have remained largely unknown. The development and promotion of alternative irrigation methods could form an important part of the solution to the world food problem. For example, simple hose fed, low pressure sprinkler systems may be ideal for many developing areas, but they are seldom recommended.

Sometimes, too, it may be advantageous to mix advanced with simpler technologies. For example, surveying for land leveling might be done by a service organization using laser planes, while the farmer uses a bullock to move the earth.

Whatever the system, the primary function of irrigation is to provide water of adequate quality to the root zone of plants in proper quantities at

appropriate times. The cruxes of this deceptively simple statement are the words "proper" and "appropriate."

While irrigation principles are reasonably well understood and universally applicable, their application is strongly site-specific. Generally, the implementation of these principles must match physical, social, and economic factors on the farm and includes finding a compromise among varying social objectives. Sometimes technological and management options are readily at hand; at other times, innovative or new methods must be devised.

The main goal of irrigation research is to get a complex system to work better. Because of the interactions and linkages in irrigation systems, separating the problem into discrete research projects actually may retard progress toward the goal. Thus the systems research and the research on components, physical and human, should be conducted interactively. We suggest three interactive efforts.

First, classify and evaluate irrigation methods and develop an information system about them. To ensure steady progress in agricultural production, a constant flow of practical information on new techniques and improved irrigation practices is required (Doorenbos 1975). In a world where published work increases almost exponentially, there seems an obvious need, first of all, to assess all that is new and then to adapt and improve it so that it can be used in conditions very different from those under which it was developed.

In a general sense, much is known about the physical and economic aspects of various irrigation methods, and a number of texts are available. Unfortunately, however, most of the available information is not presented in a manner that is helpful in selecting the "best" method for a given physical site.

An analysis of the physical and economic problems related to the selection of irrigation methods points toward the value of developing a meaningful taxonomy of irrigation methods. This would result in a more rapid and efficient matching of irrigation method and site. At present, matching is often done either inefficiently, with a personal bias, or not at all.

Since it is people who must administer, manage, use, and operate the irrigation system, the taxonomy of irrigation methods must devote some attention to social interactions and customs. This is neither trivial nor easy, and it must be done if there is to be adequate matching of irrigation methods to indigenous cultural sites. An example of a general taxonomic approach to a complex sociotechnical system is contained in the study

made by the Technical Committee of the Water Resource Centers of the Thirteen Western States (U.S. Department of the Interior 1974).

Second, develop methods of diagnosis and analysis for that part of the irrigation system that includes water distribution to farmers' fields and management of water on the farm. One of the more difficult problems of irrigation is the physical extension of water networks so that every field is adequately served in areas where land ownership is fragmented and owners take steps opposing any common use, e.g., through ditch rights-of-way. Okita and Takase (1976) point out that no other single action could have as much impact on rice production in Southeast Asia as this one. Alternate technologies, such as the use of inexpensive closed conduits underground, make solution of this problem easier. But even if the physical system can be constructed difficult problems of management still remain.

Irrigation systems can be classified broadly into those that deliver water to the edge of a field and depend on gravitational flow for distribution, and those that deliver water at prescribed rates to the point of use. The former (including furrow, basin, and flood irrigation) depend for uniformity of distribution on the time water is ponded on the soil surface and the rate at which the soil will absorb water. The latter, in contrast, depends on engineering appurtenances for uniformity. Both types have inherent, but different, limitations.

For optimal management, water should be delivered to the field on demand, meaning that the farmer should have the option of applying a prescribed amount of water when needed. Delivery on demand, however, is only possible if the conveyance system is designed with sufficient capacity to accommodate peak demand and is operated with that objective in mind, and if adequate surface reservoir or groundwater storage is part of the supply system.

Proper irrigation scheduling requires careful control of both the amount and time of irrigation, and a rational determination of the amount required at a given time. In many instances, neither the knowledge nor the equipment is readily available to make these determinations.

Both uniformity of distribution and proper scheduling are needed for efficient irrigation. Efficient irrigation, in turn, is generally important, and sometimes crucial, for effective resource use. Efficient irrigation, for example, reduces the drainage requirement, and it reduces energy requirements since much of the water is pumped either for irrigation or for drainage. It also reduces fertilizer requirements

by reducing leaching losses. Reuse pits can greatly improve the efficiency of some irrigation systems.

All irrigation water contains salts, and its use by crops increases the salt concentration in the water that remains. Provision must be made to remove this salt through leaching. The resulting drainage water will have a higher salt concentration that will degrade water in the receiving water course.

All of these concepts are well established, but their use to develop effective irrigation systems that optimize water use for crop production, the management of the salt, and the subsequent use or disposal of the drainage water remain major problems. While general principles can be quoted readily, details are often in contention or unknown. For example, the amount of leaching required to maintain a vigorous crop and not adversely affect soil properties has not been established satisfactorily. Specific application of general principles also depends heavily on local circumstances. For example, uniformity in a furrow irrigation system can be increased by reducing the length of run; short runs can interfere with mechanized operations, but they will affect farmers with smaller farms and less equipment differently than those with large farms and ample equipment.

The need is for careful determination of optimum management procedures and for innovative solutions to achieve such management levels within local socioeconomic and physical restraints. These studies must be interdisciplinary in nature and should take into account human and socioeconomic factors as well as physical and technological ones. Institutional relationships must be understood thoroughly, and technical changes must be evaluated as to impact and feasibility. Such studies can be conducted best as case studies on existing projects. Comparisons among cases should help identify common criteria of success or failure.

Third, provide a steady flow of research and technology on basic and applied problems. The types of research needed for irrigation and drainage problems have been treated in detail by several recent studies (USAID 1974, 1975; NRC 1975a; USDA 1975b:101-113; Brown et al. 1975). Below are some research needs we found important.

First, evaluate existing systems and develop new ones for applying water to meet crop needs exactly. For example, dead-level irrigation has an important place in eliminating tail water and increasing efficiency, but where is it applicable? Does it need laser leveling and automation to come to full fruition? Pump-back systems become economical if water prices are high enough, or if penalties are applied for excessive

drainage. In the Imperial Valley of California the price of water is now tripled if the irrigation district judges its application to be excessive. Tree crops can be irrigated at 98 percent uniformity, using simple plastic tubing distribution systems, if water is continuously available at the field with only 1 meter of hydraulic head.

Second, provide practical means of implementing the scheduling of time and amount of water. Required are on-site water measuring devices; computer scheduling is one approach--with limitations; soil-water monitoring with a limited number of neutron access tubes and individual field calibration from experience may be an alternative.

Third, utilize field irrigation scheduling data to operate delivery/distribution systems. Computerization and feedback control are probably needed, but as yet are not available in workable form for open channel conveyance systems.

Fourth, through systems analysis, develop hydraulic and economic parameters for total system designs that will distribute water from source to field in correct quantities when needed. This may involve increased dependence on intermediate storage facilities and delivery through closed conduits.

Fifth, evaluate the concept (Rawlins and Raats 1975) of frequent irrigation. In some cases soil properties may restrict the infiltration rate when the soil is kept wet from too frequent irrigation, resulting in low oxygen or other stresses. Feedback systems are needed to control application rates. Current drip systems, for example, are frequently supplied with water by means of guesses as to need.

Sixth, evaluate leaching requirements. Limiting leaching to a minimum can have high benefits in total water use in some circumstances, but it has no value in others. However, the management skills and techniques needed for low leaching are different from those needed for high leaching, and theoretical limits are not yet well established physiologically.

Seventh, develop low-cost closed conduits, using indigenous materials if possible. These would help solve many problems, such as right-of-way space and excessive seepage, as well as providing controlled delivery with low expenditures of energy.

IMPACT

Improved irrigation could increase (1) yields on present and future irrigated lands, (2) farmer income, (3) amount of productive land in the world, (4) returns on capital investments in irrigation, and (5) stability

of agricultural production at all levels, from individual farmer to international market. Furthermore, it would decrease harmful environmental impacts and increase conservation of resources.

How rapidly these benefits could be obtained, and what size they would be, is difficult to estimate. Some period of time, say five years, will be needed to gain momentum. At the end of 10 years, improved irrigation could reasonably be expected to contribute 200 to 400 million tons of equivalent food grains, and three or four times that amount by 2000.

The critical importance of irrigation in increasing crop yields cannot be overemphasized; water, in shortage or excess, is the most common limiting factor in food production. Areas of ample annual precipitation often suffer from periodic droughts, as well as periods of excess water. The efficiency with which irrigation water is used is highly management-dependent. Where total supplies of water are short relative to available land, maximum production may result from limited irrigation in extended areas.

According to the Asian Development Bank (1969:516-561), rice production (which occupies over half of the world's irrigated area) can be raised from typical average yields of less than 1 ton/ha to over 2 tons/ha through better water management practices alone. By also using better cultivating methods and higher yielding varieties with more fertilizer and pest control, average yields in Japan have been raised above 5 tons/ha. This is still well below half of what could be achieved with multiple cropping under optimum conditions.

There are an estimated 201 million hectares of irrigated land in the world, and the prospects are for at least a doubling of this area. Under optimum production the area now irrigated could feed the entire world population, at least at subsistence level.

According to Okita and Takase (1976):

> As of 1975, the total harvested paddy in sixteen Asian countries or areas (excluding China and Japan) was approximately 80 million hectares. Of this, 65% depends on rainfall, 33% is inadequately irrigated, and only 2% is adequately irrigated.
>
> Unlike other areas there is a close correlation between food production and irrigation rate only in Asia. In the case of paddy, water is an absolute prerequisite and varieties and fertilizers have effects only when there is sufficient water.

In order to meet the requirements of this area, which had a 1975 population of 1.13 billion and produced only 150 million tons of rice in 1970, rice production should be doubled by 1990. This will require 26.4 million hectares of rainfed and 21.5 million hectares of inadequately irrigated areas to be adequately irrigated by 1990 at a cost of $48.2 billion. This is a cost of $1,000 per hectare on the average for land that is already under cultivation.

The investment cost of new irrigation works and land preparation has been estimated to range from about $500 per hectare for small river projects to $1,500 for sprinkler irrigation using groundwater, $2,500 for complete trickle systems for intensive horticulture, and up to $3,000 for large schemes requiring water storage and long distance conveyance, using 1974 prices (Doorenbos 1975). Using an average figure of $2,000 projected to 201 million hectares, a worldwide investment of $400 billion in current prices would be required. For the 40 million hectares for which irrigation systems are currently on the drawing boards, the required investment would be $80 billion. If yields could be increased by 1 ton/ha on lands now irrigated, the increased gross annual return would be $50 billion at $250 per ton, or 12.5 percent on the capital value.

Weather, particularly water supply, is the major cause of fluctuation in food production. Irrigation greatly reduces or eliminates this risk. At the farm level, risk is a major deterrent to innovation. Improvement of water courses in experimental areas in Pakistan has caused some small farmers to move from subsistence to market agriculture (R. Ernst 1976, personal communication, AID).

Furthermore, as noted in the Introduction, Kovda (1971) estimates that 50 to 60 percent of the irrigated lands in arid areas have become saline. Soil erosion, increased sediment load and salinity in rivers, and increased breeding areas for disease organisms are among the environmental hazards of current irrigation methods. These hazards can be greatly reduced or even eliminated by improved irrigation and drainage practices.

IMPLEMENTATION

Estimated funding needs for research and technological advances to improve farm irrigation are from $10 to $20 million per year over the next 20 years. While these figures may seem large, they are small when compared to the investments that have already been made in poorly functioning irrigation

facilities and the capital investments expected in the future. The probability for success is high, but success will require an effort of critical size, a continuing commitment, and a large measure of international cooperation. Also needed is a substantial education and training program to increase the number of qualified personnel, who should be given opportunities for international experience and the promise of career advancement, a factor that has been missing both at academic institutions and in government agencies.

The usefulness of an international center for irrigation and water management has been explored for the past 10 years or more (Peterson 1972:17-21). The central problem of "making the system work" requires a broad clinical approach. It would be difficult to assess case histories, however, unless the center were organized and managed with the blessing of responsible national officials of ministerial rank, since most countries are sensitive to the shortcomings of projects under their jurisdiction. This, and the existence of rather definite regional ecological areas, makes regional centers more appealing than international ones. The issue deserves more careful study, including discussions with the officials of concerned countries.

Meanwhile, some of the necessary organizational elements may be converging. AID has encouraged the formation of a university consortium and has financed at several universities the development of personnel to work on agricultural water management. This effort could be extended further under the Title XII amendment to the Foreign Assistance Act. AID also has fostered research dealing with the systems aspects of irrigation projects in some of the developing countries (e.g., Eckert et al. 1975). There are substantial personnel resources at USDA, and FAO needs to be involved, as do the international banks and other donors.

Perhaps the first step should be to organize effectively the various U.S. resources, i.e., those universities having significant expertise in irrigation, USDA, and AID, and to frame the general outlines of a proposed program that could then be discussed with the International Bank for Reconstruction and Development (IBRD) and its international counterparts. Title XII could provide the framework for fostering and strengthening the leadership effort, which should include the creation of a multidisciplinary technical advisory group and a consolidated technical staff drawn from AID, USDA, and the universities. The National Academy of Sciences might take the initiative by sponsoring a workshop to discuss and make recommendations on a specific organizational approach. Needless to say, such an

organization should develop close ties with organizations in other disciplines, particularly soils, crops, and economics.

The development of much of the physical and economic taxonomy of irrigation systems, the creation of alternate technologies, and the training of technical, management, and institutional personnel can be carried out at selected sites in the high-income countries; however, the incorporation of cultural factors into system design and management will require new and ingenious approaches involving combined teams of social scientists, engineers, and economists working directly with indigenous groups in the rural areas of developing nations.

Ultimately, pilot and demonstration farms must be operated throughout the world, and used as training grounds and as examples of better irrigation practices at the local level. Fortunately, many of the needed research and demonstration farms are already in existence, and the major requirement will be reorganization and coordination of efforts.

Doorenbos (1975) has summarized the effort required to take better advantage of the potential of irrigation:

> Cooperative research programmes are needed, involving research centres of the high-income countries, international and regional research and suitable national institutions, particularly on problems relating to the development of an appropriate technology for improved yields of land and water resources and increased productivity. With the present food crisis, if research is to be effective, it should be primarily goal-oriented and the results at least believed to be achievable in practice. Since generated knowledge is too often applied independently by the user, provision, where feasible, should be included for the research organization to extend its work to the implementation phase. More emphasis is needed on the so-called intermediate technology programmes which must be concerned particularly with applied research. But gathered knowledge needs to be broadbase. Different levels and methods can be identified which may range from scientific seminars for the specialists, to a meeting on a demonstration plot for the farmer. Each situation is site specific and must be treated accordingly. It remains dangerous to dictate precisely the type and level of research required. Whether basic or applied, whether

at the level of policymakers, specialists or users, experience shows that each step must be carefully planned; it is a long and also costly process; it must be a continuous programme and it must produce results.

Profile 8

USE AND PREDICTABILITY OF WEATHER INFORMATION FOR FARM MANAGEMENT DECISION MAKING

Variations in weather are the largest single factor causing shortfalls in food production (NRC 1976a). (See Study Team 5's report.) Since relatively little can be done to change the weather, the best way to reduce weather risks is to improve the transfer of advanced agricultural technology, taking into account weather variables. If the risks involving both rainfed and irrigated agriculture are to be reduced, much more attention needs to be given to weather data in terms of their specific significance to crop systems. The importance of improved transfer of agricultural technology has been stressed in recent extensive agricultural research studies (USDA 1975b:113, NRC 1975a).

By and large, attempts to transfer agricultural technology directly from the temperate to the tropical zone have been less than successful; the tropical environment, including pests and fertility as well as climatic factors, has proved to be extremely hostile to temperate plant varieties and hybrids, although many aspects of irrigation, drainage, cultivation, and chemical application technology have been found to be readily transferable. Logically, better plant varieties need to be found to utilize available technology effectively, and strategies are needed to adapt transferable technology to different environments. If this is to be done, those environmental indicators most directly related to the growth of crops must be better characterized. Soil is an important environmental indicator, and its use as one is discussed in Profiles 1 and 4. Weather constitutes another, although weather and soil are related. Practical weather indicators related to crop growth need to be devised and improved crop-weather relationships established (NRC 1976a).

The results of specific variety tests involving soil, water, fertilizer, and other variables relate to

limited geographical areas and only to the weather of the year in which they are conducted. The only logical bases for their transfer to other sites are environmental characteristics common to both the test and transfer sites. Small changes in latitude or altitude can significantly alter the performance of most varieties. Thus most U.S. states are subdivided for crop yield testing into many small areas that are relatively homogenous in climate, soil, and water availability, and even then a three-year or four-year test at a site gives a relatively small sample of the weather at that site.

Yield experiments conducted around the world have provided some positive additions to knowledge about food production in tropical and subtropical areas, but progress has been slow. The need for increased food production, on the other hand, is large and immediate. This suggests the need for a more systematic approach to collecting and disseminating information useful for predicting the response of cropping systems as weather conditions vary, and information on how crop systems might be altered to increase production, given weather variances. Such an approach also must characterize other environmental and management variables. A comprehensive, computerized information system considering all cultivars, all environmental variables, and all management inputs will probably never be feasible or desirable. However, efforts to organize data into a classification system so that production data collected at one site can be used to predict production at other sites with similar environments would have substantial benefits. This effort is particularly needed in the case of weather.

RESEARCH NEEDS

The one thing that can be transferred without variance to a new site is the genetic makeup of a cultivar. This is all contained in the seed, which may be carried from one place to another. Production data, therefore, are commonly referenced to specific cultivars tested, i.e., organized in terms of specific cultivars. There are, however, literally thousands of varieties within even one crop species. Practically speaking, many of these are closely related, and testing is continually conducted to identify the more promising varieties, of which relatively few are used. In 1970 in the United States, for example, 77 percent of the corn grown was dependent upon six major cultivars, 65 percent of the rice on four, 50 percent of the wheat on two, and 56 percent of the soybeans on six (NRC 1972a).

A very general model concept is presented here. It is recognized that such a model never will be fully developed quantitatively. Even if possible, that would serve no purpose since there is a trade-off between level of resolution and utility. The reaction R of any cultivar V to any environment E is expressed by the equation R=f(VxE). The V x E interaction can be expanded as shown in Figure 2.

Each cultivar can have many responses (yields) in a given natural environment if that environment is modified through different husbandry practices, such as changing planting time, tillage, irrigation, drainage, fertilizer use, and pest control.

As mentioned earlier, soil characterization is a basic transfer element that implicitly contains climatic information. Similar cultivars on similar soils can be expected to produce similar yields. Soil would therefore be a primary environmental indicator, and the equation would take the form R=f(VxS) where S is the soil indicator.

Climatic approaches to transfer also have been used with considerable success. These include climatic analogs and crop geography-climatology techniques. In this case climate or climate and soil together constitute the environmental component in the

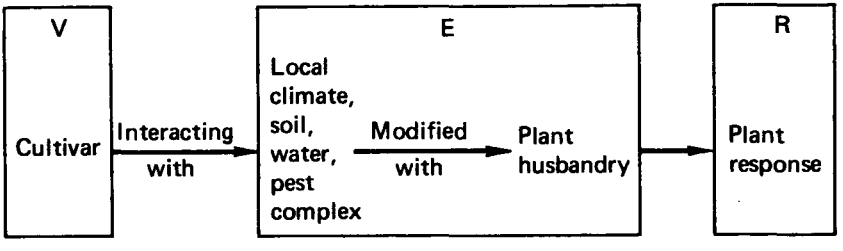


FIGURE 2 The interaction between cultivar and environment.

conceptual model. (See the report of Study Team 5, especially Profiles 5 and 6.)

The purpose of this profile is to explore the possibilities of adding weather as a transfer element. This brings the time dimension into play in dynamic terms. Weather is the most variable and least predictable of the resources used in crop production, and of the various weather factors water supply is the most variable. Since there is an infinite set of equally likely future weather sequences at any site, the impacts of weather need to be approached using stochastic processes and expressed in probabilistic terms.

If stochastic sequences of weather are to be examined, crop production prediction models responsive to time sequences of weather variables are needed. A number of these have been proposed and appear promising. One approach has been to combine such variables as temperature, humidity, radiation, etc. into potential evapotranspiration (ET). This is then moderated to predicted ET based on soil moisture and crop phenology. Crop phenology timing may be predicted using, say, cumulative heat units rather than calendar days. Such models should be solidly based on physical and biological principles. What is needed are field-level prediction models with a balanced level of resolution consistent with other information (Hill et al. 1974).

Research and development needs in the interactive system are listed below. These cannot each be approached separately. Each must be linked iteratively to the others as the research and development effort progresses (Keller and Leininger 1975).

-- Identify a set of weather indicators that are fairly easily measured and can be used in crop-weather models useful for yield prediction.
-- Develop and test field-level crop production models responding to various sets of weather sequences, testing and calibrating these for a selected number of varieties of important crop species. The Committee on Climate and Weather Fluctuation and Agricultural Production of the National Academy of Sciences recommends that "dynamic crop-growth-simulation models should be improved and developed for such crops as wheat, corn, soybeans, sorghum, and major forages (NRC 1976a)." The general form of such models seems reasonably constant within species. Hopefully, this will hold true to the needed level of resolution. (The proposed approach has been criticized on the grounds that it may lose sight of basic scientific principles by simply substituting sets of empirical correlations of

data. Certainly the approach should not violate known scientific principles and should draw on them to the fullest extent. The process works both ways, however. A successful field model, even though partly empirical, could point the way toward better understanding of fundamental scientific processes).

— Characterize site weather indicators. Measurable aspects of a plant's response to measurable aspects of the site's weather need to be quantified in standardized units calculable for the periodic intervals delineating phases of plant growth.

— Conduct field trials of selected indicator varieties on various soil types at selected climatic sites within the ecological range of the crop species or variety. These trials would serve to (1) calibrate the models, (2) define the ranges of their reliability, and (3) establish the forms in which weather data should be collected. The water supply element of the environment can be controlled on sites where precipitation is deficient, thereby increasing the number of variables that can be studied at such sites in a particular year.

A large number of field trials have already produced productivity information, some of which might be used. The literature usually consists of data triads (V, E, R) listing the observed yields of chosen varieties when tested in the environmental sequences occurring during the year of each test. Usually very little of the data describing the environment is recorded, nor are observations recorded of the specific times at which critical phenological stages occurred. Thus most existing data triads are not very useful for predicting crop response. This is especially true for subtropical and tropical areas.

Some important V x E interactions have not been identified and studied. For example, while evapotranspiration can be estimated successfully from suitable weather and plant stage-of-growth data, the role of the photoperiod in any specific V x E interaction is not now clear.

-- Study risk and strategy. This will require the processing of sets of historical or synthetic weather data sequentially through the crop production model to arrive at distributions of expected yields at benchmark sites, using techniques similar to those used in operational hydrology (Ahmed et al. 1976, Fiering 1967). It also could lead to strategies for management as the growing season begins and progresses, e.g., what is the best course if soil moisture is deficient at

planting time? What is the best use for limited supplies of irrigation water? What would be the best course if reliable short-term weather forecasts become available?

A past weather history is only one of a very large set of equally likely future sequences which may well contain both longer and more intense periods of drought. Statistical methods to generate equally likely future sequences need to be devised. Here again, the approach would be analogous to that used in hydrology for generating alternative future sequences of streamflow.

The effects of long-term and short-term climatic trends also could be taken into account in devising weather data sequences, but this topic is not included in this profile.

-- Develop an information system. The foregoing concepts point to the desirable design parameters for an information system. A great deal of care needs to go into such a system. Without foreclosing the options, a distributive rather than a centralized approach seems most appropriate, with emphasis being placed on simplicity and usefulness to large numbers of people without specific expertise.

Initial emphasis should be given to the first four topics, but research and planning should begin on the last two. This profile contains no discussion of pests. These are certainly a major hazard related to weather and other factors. We feel that they deserve a separate but coordinated effort.

IMPACT

The primary benefits can be summarized as increases in the ease, confidence, and rapidity with which crop technology can be transferred. Given better predictions about production probabilities and risks over the long run, better choices of cultivars could be made and improved strategies implemented in shorter development periods. Knowing the risks better would lead to more realistic capital financing and planning, with an improved success ratio.

While the proposed research would by no means eliminate field trials, these would probably not need to be as extensive as is now commonly supposed. A broader spectrum of weather impacts could be collected in a shorter time because data from different sites could be analyzed within a common framework. With broader knowledge for planning experimentation, the need for duplication would be reduced and more efforts

could be devoted to new areas of biological research. Present crop breeding strategy is to select varieties that have good productivity over a broad range of environmental variances. This is effective, especially in reducing risk. But it is possible that a broader range of varieties, more closely related to specific sites could result in even greater productivity. This also would reduce the hazards of relying on a very narrow genetic base for a very large share of our crop production.

The research also should provide a better understanding of the relative importance of environmental resources, technological inputs, and changes in husbandry. Given the limited resources for long-range investment in interventions, better allocation choices could be made by farmers. Eventually, enough might be known about global weather risks to refine estimates of the need for national and international food reserves. Similarly, the proposed research could lead to improved decision making prior to and during the growing season as the result of more knowledge about potential short-term risks.

A beneficial impact on the seed industry also could be expected. Seed producers would want their products included in the research. As a result of work already underway in the United States on maize, a number of breeders have agreed to use open pedigree lines for field testing and to cooperate in improving the data collected.

Globally, an assessment of impact in monetary or productivity terms is difficult if not impossible to make. Decreasing lead time to achieve even modest increases in production and decreasing annual variances in production would both have tremendous value.

The chance that the program would be successful is high. For three or four key crops, significant results could be obtained in three or four years. Realization of most of the potential advances would probably take 10 to 15 years.

IMPLEMENTATION

A multidisciplinary effort will be required. There also must be a means of communicating with and utilizing research stations in many countries. The entire effort needs to be organized under a single administrative unit, but the work will require the involvement of a number of institutions in the United States and abroad. A coordinated international testing system is visualized, but without the addition of new test sites. The problem is not a lack of sites but identification of a reasonable set of pertinent

variables to be measured and a strategy to coordinate research activities.

Each site must be carefully characterized and the collection of data monitored by a trained observer. The major additional expense at each selected site will be for weather instruments (many will already be on hand) and for the personnel required to record plant growth and weather data.

The pedigree of all crop cultivars should be known so that the research genotype can be readily identified and related to commercial seed lines. "Indicator" sets of about 20 open-pedigree lines which cover the genetic spectrum of each major crop can be selected.

The growing season, from land preparation through harvest, is the most important monitoring period, but weather events during the nongrowing season also are important. Winter precipitation, for example, may fill the soil with moisture and thereby improve growth. Full-year monitoring is usually necessary in tropical areas, however. Double and intercropping may be practical, and the same crop may be grown during more than one season.

A daily record should be made of important weather factors, such as temperature, radiation, relative humidity, and so forth. A periodic record of plant development from planting through harvest also is needed, and a complete water and soil amendment record must be kept. Plant use of these added elements can be computed theoretically, but practically speaking, computed values will have to be checked and adjusted, depending upon analyses of samples taken just prior to and immediately following the growing season. All tillage practices and their times of occurrence need to be recorded, along with information on pests and methods used to control them. Major storm damage to plants should be noted in enough detail to estimate probable effects on plant development afterwards.

All data should be collected in a systematic and uniform manner. Centers staffed by multidisciplinary groups of scientists should analyze the data statistically and prepare it in standard form. Various groups of scientists capable of developing approaches to the proposed research already exist. Once sufficient data are available, some of these groups will use it.

The data and whatever crop management information is developed from them should be maintained in a form and manner that is accessible for both public and private needs.

Much of this work can be done by existing international institutes, AID-sponsored crop centers, and national research centers. The number of test locations for a single crop could be as few as 7 to 10

at the beginning, with cthers being added when necessary. Each test site should be characterized to the greatest degree possible, and sequential weather and crop data should be uniform and readily retrievable. The designing of standards and procedures for data collection will be an important function of the research managers.

It is expected that the research program would cost $2 to $4 million per year for three years for each of two or three key crops, possibly maize, soybeans, and wheat. If successful, the program could be extended to other key crops at approximately the same level of spending.

In order to plan and implement the project, interdisciplinary symposia or workshops involving knowledgeable and interested professionals will have to be organized, possibly through AID contractual channels or under NAS sponsorship. These should include international authorities and users. Once the details are worked out, central management could be assigned by contractual arrangements to appropriate institutions given defined responsibilities for developing appropriate programs with international centers, national research agencies, foundations, and private enterprises, such as seed companies. These programs would train observers, instruct researchers, devise data processing and retrieval systems, and develop the details of the models.

Report of Subgroup C, Study Team 4

FERTILIZERS

INTRODUCTION

Fertilizer is a basic input in the food production chain. While other inputs, particularly land, water, and farm machinery, also play key roles in increasing food production, increasing their availability requires long-term development strategies which are difficult to implement in the short run and frequently beyond the control of the individual farmer, particularly the small farmer. But an increase in the availability of fertilizer, coupled with improved seeds and adequate credit, provides the means to increase crop yields and income quickly and effectively at the small farmer level.

It was precisely this combination of fertilizer, seed, and credit that was employed by the policymakers of nation after nation during the food shortage of 1972-74 to blunt the threat of famine. This combination, with fertilizer as its key, is likely to receive even greater attention in the future as the battle to ward off starvation intensifies and the struggle to distribute rural income equitably expands.

As important as fertilizer is in increasing food production, the developing countries are only now beginning to recognize the special role it plays in overall agricultural development. As recently as 1974-75, only 26 percent of the total fertilizer used in the world was consumed by the developing countries, where two-thirds of the world's population resides. The average fertilizer use rates for the individual nations of the developing world are frequently 1/25 to one-fifth the rate of use in the principal fertilizer-consuming high-income countries.

This disparity may increase even more in the years to come unless major efforts are made by the develcping world. Recent estimates of fertilizer use by the United Nations Industrial Development Organization (UNIDO) (1977) suggest that total nutrient consumption will increase from 81.7 million tons in 1975 to 307 million tons by the year 2000. Using the Lima Declaration of December 1975 as a guideline for Third World development strategies, UNIDO (1976) states that

40 percent of the consumption by the year 2000 needs to be in the developing countries and that these same nations should be self-sufficient in nitrogen and phosphate production by that time. The accuracy of these figures can be debated. The need, however, to step up efforts to promote more intensive and extensive use of fertilizer in food-deficit developing countries seems to be beyond question.

Serious constraints exist on increasing fertilizer use in the developing countries, however, particularly at the small farm level. Unfavorable price relationships between fertilizers and farm prices have not been conducive to persuading farmers, particularly small farmers, to use large quantities of fertilizer. While comments on food policy are beyond the scope of this report, the prospects for improving the cost-price relationship between fertilizer and farm crop prices by reducing the cost of fertilizer are not bright. There is every indication that the costs of fertilizer will increase sharply in the years ahead due to at least three factors: the rising costs of raw materials from which to make fertilizer as natural gas and oil become scarcer and the difficulty of mining phosphate rock increases, sharp rises in the cost of nitrogen and phosphate fertilizer production facilities, and rapidly rising costs for transporting and distributing fertilizer.

There is an additional set of constraints limiting fertilizer use which particularly affects the small farmer. These can be classified under the broad category of risk. The small farmer who uses recommended quantities of fertilizer, particularly on food crops, takes high risks if water availability is unpredictable and beyond control, if crop response to fertilizer (particularly phosphate, potash, and micronutrients) is questionable or unknown, if credit is expensive and/or unavailable, and if the price the farmer receives for farm products is uncertain.

It was the contention of many in the late 1950s and 1960s that sufficient information and expertise on fertilizer use existed in the high-income countries, and that the problem of low crop yields in the developing countries could readily be solved by applying the available information and expertise. The experience of the 1970s has shown this not to be the case. Most of the fertilizer materials used today were designed for use in temperate climates in a well-managed and sophisticated agriculture. Such conditions do not exist in much of the developing world. As a result, modern fertilizers have not performed well where the basic distribution system needed to move fertilizers is undeveloped, where water regimes are poorly developed or uncontrollable, and where the basic

field research needed to predict plant response to fertilizer is lacking or where the educational system needed to transmit this information is not functioning properly. As a result, fertilizer efficiencies in the developing countries are substantially below those achieved by farmers of the high-income countries.

There also are serious obstacles to increased fertilizer production in the developing countries. Present production methods are based upon the capital-intensive technology of the high-income countries where investment capital and skilled labor are more available. In most developing countries, foreign exchange limitations are a serious problem. Thus efforts to expand high technology, capital-intensive fertilizer production facilities present difficult problems in finance and operations. In addition, there is a reluctance to use a technology that is energy-intensive. As energy sources become scarcer and more costly, even the high-income countries are faced with developing more energy-saving technology.

Finally, time and resistance to change also are critical factors affecting efforts to increase food production through fertilizer use in the developing countries. The agricultural support facilities available to the high-income countries were assembled over many decades. The developing world does not have this luxury; it must transform its agriculture more rapidly if famine is to be averted. Such a rapid transformation is exceedingly difficult in traditional societies, where cultural patterns often impede the diffusion of innovations.

We have taken due note of the importance of providing for adequate plant nutrition in combating world hunger and of the recent events that might jeopardize achievement of that goal. The team has prepared the following research profiles describing key areas where research is needed. Five topics were selected from an original list of 12: (1) low-cost sources of nitrogen for use in fertilizers, (2) nutrient efficiency, especially of crops grown on tropical soils, (3) mining and processing methods for recovery and utilization of phosphate ores, (4) recycling organic wastes as a source of nutrients and as soil conditioners, and (5) the economics of fertilizer supply, pricing, distribution, and use.

These profiles do not cover every aspect of the problem because of space limitations and the need to establish priorities. Thus two priority items were dropped from consideration on the assumption that they would be covered elsewhere. These items were soil acidity and the techniques to be used to solve this problem, and fertilizer marketing, including distribution and storage.

Profile 1

LOW-COST SOURCES OF NITROGEN FOR USE IN FERTILIZERS

Adequate low-cost supplies of nitrogen for use in fertilizers are essential if the expanding food needs of the world's population are to be met. Currently, most of the world's nitrogen fertilizers are manufactured using the Haber-Bosch process developed in the early part of this century. Several other processes for nitrogen fixation--the arc process, the cyanide process, the nitridic process, and the cyanamide process--have been used with varying degrees of success, but because of high costs or other disadvantages they have faded into obscurity.

Under the Haber-Bosch process, ammonia is synthesized from a 3:1 volume mixture of hydrogen and nitrogen at elevated temperature and pressure in the presence of an iron oxide-based catalyst. The process utilizes nitrogen from the air and hydrogen from natural gas, naphtha, fuel oil, or coal in combination with steam. Natural gas is the dominant source (providing over 95 percent of the hydrocarbon source in the United States and about 70 percent for the world as a whole), followed by naphtha and fuel oil. Coal and electrolytic hydrogen are minor sources.

Since production of nitrogen fertilizer is inextricably linked with the energy situation, great concern has developed in many countries over existing or impending shortages of natural gas, naphtha, and fuel oil for ammonia synthesis. This has generated considerable interest in coal as an alternative, especially in the United States. It also has prompted a search for new sources of hydrogen, as well as for processes for fixing nitrogen without the introduction of hydrogen.

RESEARCH NEEDS

Improving the Production of Ammonia from Coal

The research need is to develop a direct and economical method of creating ammonia through coal gasification by way of hydrogen production. The ideal coal gasification process would allow the use of a wide variety of coal, operate at the high pressures compatible with ammonia processes, produce a compatible gas comprised essentially of hydrogen and carbon dioxide without undesirable contaminants (such as trace elements, tars, oils, phenols, and chars), and have simple, reliable coal feed, ash discharge, and heat recovery systems.

A number of new coal gasification reactors are being developed in this country. These should be evaluated in light of the above criteria, modified if needed, increased in size, and coupled with standard conversion and gas purification equipment to supply an existing ammonia plant, utilizing a significant portion of the plant's equipment. Such a converted ammonia plant would serve as a prototype installation from which technical and economic data could be obtained in order to evaluate the reliability of the process. Assuming that the process proved reliable, it would be incorporated in the construction of new ammonia plants or retrofitted to existing ammonia plants that now use natural gas or feedstocks.

The development and demonstration of an effective second generation ammonia-from-coal plant should be possible within eight years and should result in about a 25 percent reduction in the energy required to produce ammonia.

Impact

The development of an improved process for obtaining ammonia from coal would permit the United States and other coal-rich countries to utilize coal increasingly as a source of hydrogen to produce nitrogen fertilizers. This would assure these countries enough fertilizer to meet agricultural needs while freeing them from the need to spend large amounts of foreign exchange to buy oil or gas for fertilizer production. The ultimate impact would be more food at a lower cost than would result otherwise. The need for the development of a prototype plant for the creation of ammonia from coal is immediate. Fortunately, existing research and development activities make such a step feasible in the near future.

Part of the U.S. research community is already deeply involved in the development of new technology for the conversion of coal to liquid and gaseous hydrocarbon fuels (American Institute of Chemical Engineers 1974-75). This substantial research effort has stemmed from national concern over decreasing reserves of petroleum and natural gas and the resulting dependence upon crude oil and petroleum products imported from the Organization of Petroleum Exporting Countries (OPEC). Unfortunately, however, no significant portion of the research is devoted specifically to the task of obtaining ammonia through coal gasification.

There are some coal-based ammonia plants in operation or under construction in India, Turkey, and South Africa. These plants are based upon German technology largely developed before World War II, and they are expensive to build, relatively inefficient in terms of resource use, and dependent entirely on proprietary foreign technology. None of these plants utilizes the high-temperature, high-pressure technology applicable to nearly all types of coal that is particularly useful for ammonia production, and all are apparently subject to low reliability.

Given the vast worldwide reserves of coal and the limited and dwindling supplies of petroleum and natural gas, an immediate and intensive effort to develop the coal gasification process can easily be justified, particularly in the United States. This country, which depends almost entirely upon natural gas as an ammonia source, is currently consuming about 65 billion cubic meters of natural gas per year. The latest estimate of known reserves is 5.8 trillion cubic meters, and U.S. gas production is declining by 6 percent annually (Bureau of National Affairs 1976). Unless large new gas reserves are found or home use of gas is greatly curtailed, the United States will be in a serious position within relatively few years. Using petroleum as a source of ammonia production offers no real alternative since the United States is already excessively dependent upon oil imports from the OPEC countries. On the other hand, the U.S. Department of the Interior (1975) estimates that the United States has coal reserves totaling about 3,600 billion tons. Part of this reserve is located in or near some of the nation's most important agricultural areas. Coal reserves also are immense worldwide (Table 6).

The United States is currently using about 19 billion cubic meters of natural gas annually for ammonia production. About 1,200 cubic meters of pipeline quality gas are required for each ton of ammonia produced. About two-thirds of the gas used in producing each ton is used for the chemical

Table 6. Estimated total original coal resources of the world (in billions of tons)

Continent	Identified resources	Estimated total resources
Asia (includes European U.S.S.R.)	2,700	10,000
North America	1,725	4,000
Europe (includes Turkey)	275	725
Africa	82	225
Oceania (Australia, New Zealand, New Caledonia)	64	120
South and Central America	27	36
TOTAL	4,873	15,106

Source: U.S. National Committee of the World Energy Conference (1974).

constituents of ammonia, while the other one-third provides process heat.

A number of countries besides the United States have ample coal reserves but are short of oil and/or natural gas (U.S. National Committee of the World Energy Conference 1974). These include several European countries as well as Botswana, India, South Korea, Swaziland, Taiwan, Tanzania, Thailand, and Zaire.

Implementation

Essentially this research effort involves selection of the most promising coal gasification process currently under development that meets the criteria for ammonia production, linking it to an existing ammonia plant, making modifications in both the gasification and the ammonia plants, and demonstrating the process and system. Total estimated costs, depending on the scale of the installation, range from $50 to $100 million.

The U.S. government would provide all or part of the funds, depending on the extent to which private industry participated. No other country has as much at stake, and no other country has done as much work in developing improved coal gasification procedures. The necessary collaboration would involve a gasification developer, an ammonia producer, and the U.S. government through the Energy Research and Development Administration (ERDA), the Tennessee Valley Authority (TVA), or both. Both ERDA and TVA have taken initial steps to fund a research and demonstration program.

Develop Alternate Sources of Hydrogen for Ammonia Production

The most significant amount of external energy used in the Haber-Bosch process is associated with the hydrogen production step. Therefore, research on new means of hydrogen production has potentially large benefits. Several research alternatives have been proposed and are in various stages of study.

One of these is to improve the technology available for direct hydrolysis of water to produce hydrogen. This process is still used in a few parts of the world where excess low-cost power is available, but the input of electrical energy is so great that the process usually is not economical. One new untested scheme, however, is to utilize the difference in temperature between the surface of the ocean and its depths to

generate electricity and to produce hydrogen (Francis et al. 1976b).

Another suggestion is to use some combination of electrical and chemical processes to produce hydrogen from water, resulting in less consumption of external energy than straight electrolysis. One system under preliminary study uses an intermediate sulfur cycle in which water and sulfur dioxide are electrolyzed, producing sulfuric acid and hydrogen. The sulfuric acid is then decomposed by heat or chemical means to regenerate the sulfur dioxide for recycling.

Thermochemical processes would involve a system of high-temperature chemical reactions to recycle all intermediate compounds, the net effect being use of a high-temperature heat source to produce hydrogen and water (Graff 1976). Combining a thermochemical water splitting process with the high temperatures available in nuclear power plants may be a promising approach.

Another possibility is a newly discovered photoinduced electron transfer process, which may lead to an economic procedure for producing hydrogen (Sprintschnik et al. 1976). This involves using organic ruthenium complexes to catalyze water splitting reactions driven by sunlight. Others (Wrighton et al. 1976, Fujishima and Honda 1972) are looking into photoelectrochemical cells to electrolyze water.

Waste hydrocarbons are receiving some attention as potential sources of raw chemical material or energy. Although additional development work is needed to evaluate the economics and to establish the reliability of the process, they also could be used as a source of energy for ammonia production.

All of the above have possibilities and should be investigated. The first four (with the possible exception of water hydrolysis) are long-range projects that would probably require a development period of 20 to 50 years, and chances for success appear minimal. The proposal on waste hydrocarbons covers a period of less than 15 years, and chances for success seem assured. However, their use for fertilizer production would be in direct competition with other needs for new energy sources, or alternative beneficial uses of the wastes.

Impact

Developing new methods of hydrogen production would eliminate dependence upon conventional sources of hydrocarbons or hydrogen as sources of ammonia. It also would provide additional alternatives that might be used in countries without oil, natural gas, or coal reserves. For such countries, independence from

foreign sources of energy could save many millions of dollars annually in foreign exchange.

Processes involving water hydrolysis are particularly appealing. As pointed out, however, there are few situations where sufficient low-cost energy is available. Breeder reactors could lower the cost of electrical energy enough in certain parts of the world to justify electrolysis of water, but this is not likely to happen before the turn of the century. Hydropower is often the lowest cost source of energy, but only a few countries, such as Norway, have sufficient hydroelectric energy to justify water hydrolysis. It is for this reason that ocean thermal power could be a very important source of energy for water hydrolysis. Its use, however, would be limited to tropical waters, the only ones with sufficient temperature difference (25° to 27°C at the surface and 5°C at 700 to 1,200 meters) to act as both a source and sink for a Rankine cycle system to generate electric power. Large amounts of water would have to be pumped, but the resource is tremendous--80 million km^2 of tropical waters between 10° N and 10° S latitudes. Furthermore, the energy source is available 24 hours a day. It is felt that such a system could be built with existing technology, and that it would be cost-competitive with domestic fossil fuel and nuclear power plants. The process would require the shipping of ammonia or hydrogen produced at sea to land areas.

It is too early to assess the possibilities of new electrochemical, thermochemical, or photocleavage approaches for producing hydrogen from water. Of the very few thermochemical processes with demonstrated laboratory feasibility, none appears to combine low enough investment with high enough thermal efficiency to realize the full potential advantage over electrolysis. The future prospects of both electrochemical and thermochemical processes depend upon major breakthroughs, which may or may not come. The photocleavage approaches are in their infancy. The ultimate economic advantages, however, may be very great, and basic research should be continued and possibly increased. There is less immediate urgency, however, in view of the present availability of other hydrogen feedstocks and alternative ways of producing hydrogen.

The use of waste hydrocarbons as a supplement to other sources for ammonia production appears to offer a real advantage, not only in the high-income countries but also in the developing countries. The problems, however, are formidable because of the high and variable levels of impurities. Furthermore, the economics of hydrogen production by this method are probably marginal, even when credit is given for the

disposal of waste material. The amounts of waste hydrocarbons, however, are enormous, and additional work on evaluating the economics and refining the processing steps on a demonstration scale seems justified. It is interesting to note that the city of Seattle has announced its interest in building a 365-ton-per-day demonstration-scale unit to produce ammonia from garbage.

Implementation

The plan for using the temperature difference between the ocean surface and its depth should be evaluated by a competent outside team of scientists and engineers. If the plan shows promise and is sufficiently developed, thought should be given to a demonstration plant. A consortium of private companies and government agencies might be set up to provide initial financing.

Electrochemical, thermochemical, and photocleavage processes for hydrogen production are still in the preliminary stages of development, particularly the latter two. If sponsoring institutions and industrial firms are unable to carry on their preliminary investigations, it would appear that research grants should be made by the National Science Foundation or private foundations to continue the work. If a promising process emerged, consideration should be given to enlarging development funds, possibly through ERDA.

The use of waste hydrocarbons as a source of raw chemical materials or energy should receive continued attention, especially from the economic and technical standpoints. The importance of the problem would justify selective funding by the Environmental Protection Agency (EPA) or ERDA, or both. The International Fertilizer Development Center (IFDC), AID, and FAO might all give consideration to supporting projects in the developing countries. Developments in other countries should be closely monitored.

Total annual funds required for work on alternate sources of hydrogen will range from $1 to $3 million.

Develop Abiological Processes for Fixing Nitrogen Without Using Hydrogen

The concept of developing catalysts that would provide an economically practical alternative to the Haber-Bosch process and eliminate the need for hydrogen is attractive. One possibility involves the catalytic reaction of nitrogen and water to produce ammonia

directly. This would require external energy, probably electricity. The typical catalyst is a transition metal complex based on such metals as molybdenum, vanadium, and titanium. Another possibility is direct catalytic production of nitric acid from air and water. Theoretically this method would not require an input of external energy. The major problem is finding an effective catalyst that would allow direct production of nitric acid at a low temperature without forming intermediate oxides of nitrogen. This is a long-range project that might require 50 years to achieve. As of now, chances of a major breakthrough appear minimal.

Impact

With the possible exception of biological fixation, abiological processes for fixing nitrogen without the need for hydrogen would be the best methods of producing nitrogen fertilizers. Unlimited amounts of nitrogen might be produced without concern for sources of hydrogen. The roadblocks to success, however, may be enormous. Developing a process with greater efficiency than the Haber-Bosch method is extremely difficult since the latter is based on materials in which the sun's energy has been captured, and most subsequent reactions proceed exothermally. Nevertheless, the advantages of abiological processes not requiring hydrogen could be so great that basic research should be continued and encouraged. Outside support seems necessary since researchers in the field report that adequate funding is very difficult to obtain.

Implementation

Several projects are in very fundamental stages of development. Because of difficulties in obtaining other funding, the National Science Foundation should be encouraged to support this work. Without such support, some of the projects may have to be terminated. Although the chances for success may be minimal, the potential importance of these projects is such that the small amounts necessary to continue research (about $500,000 a year) would seem well justified. Later, if it appears that certain processes warrant further development, consideration should be given to substantial government and industrial support.

Biological Fixation of Nitrogen

This topic is covered by Subgroup B, "Land and Water," and by Study Team 1.

Profile 2

PLANT NUTRIENT EFFICIENCY, PARTICULARLY IN TROPICAL REGIONS

The major food crops of the developing world are rice, wheat, maize, sorghum, and millet, and a starch group including cassava, yams, and plantain. These crops occupy 93 million hectares, 62 million hectares, 56 million hectares, 71 million hectares, and 16 million hectares, respectively, out of a total of 666 million ha, or 45 percent of the currently cultivated land in the developing world. The grain crops occupy 42 percent and constitute a major portion of the inhabitants' diets wherever they are grown (FAO 1974b).

Since all of these crops are nonlegumes, increases in yield from the application of nitrogen fertilizer are sizable wherever these crops are grown. Most soils, especially those in the tropics, are inherently deficient in phosphorus, and historically crop plants have responded to applications of fertilizer phosphorus. Potassium usually is deficient in the soils of the high-income countries where intensive cultivation is practiced, but deficiencies are less common in the developing countries. Deficiencies of the other elements required by food crops are more widely scattered and generally site-specific. Where they occur, however, crop yields may be severely limited.

Improved efficiency of use of plant nutrients can lead to increased yields per hectare, and thus to increased food production, as a consequence of more effective use of improved seeds, irrigation, pesticides, and other management practices. Improved efficiency also can result in increased uptake and utilization of plant nutrients applied to the soil. (See Profiles 1, 2, 5, and 6 of Subgroup B for further material on fertilizers.)

RESEARCH NEEDS

Improving Fertilizer Nitrogen Efficiency

Losses of fertilizer nitrogen applied to the soil are substantial. When conventional fertilizers, for example, are applied by traditional methods, paddy rice plants usually absorb only 25 to 50 percent of the nitrogen applied, although the recovery rate of upland crops may be greater. The unrecovered nitrogen is lost either through leaching, denitrification, or ammonia volatilization, depending on the situation.

The mechanisms of nitrogen loss need to be identified and the probability of losses assessed. A major research effort devoted to testing existing principles regarding the rate, timing, and placement of conventional nitrogen sources is required. Further research on the use of nitrification inhibitors with ammonium sources to reduce denitrification also is needed. The development of a urease inhibitor to prevent the hydrolysis of surface-applied urea until it moves into the soil would reduce nitrogen loss and extend the conditions under which urea could be used.

Research on new fertilizer nitrogen sources or on modification of present sources to increase efficiency should be a continuing objective. The chance of success is high.

Impact

Present fertilizer nitrogen usage in the world is about 40 million tons annually, of which about 18 million tons are used in the high-income countries, 7 million tons in the developing nations, and the remainder in the centrally planned nations. Based on total cropland, the usage is about 45 kg/ha in the high-income countries compared to about 10 kg/ha in the developing countries (FAO 1974b). Although reliable data are lacking, available information indicates that most of the fertilizer used in some developing countries is applied to plantation or export crops rather than domestic food crops. The exceptions to this may be rice and wheat, especially in Southeast Asia.

In industrialized East Asia, rice yields average about 5,000 kg/ha with a fertilizer usage (nitrogen plus phosphate plus potassium) of 200 to 300 kg/ha, while in the least-developed countries of Southeast Asia yields average 1,200 to 1,700 kg/ha with a fertilizer usage of less than 10 kg/ha (von Uexkiill 1975). Experiments with rice in the Philippines have shown increases of from 2,000 to 3,000 kg/ha from the

addition of from 100 to 140 kg/ha of fertilizer nitrogen when other management practices were adequate. It must be recognized that the use of fertilizer is only a part of good crop management, and that fertilizer additions without adequate water, suitable seeds, and good management will often result in only limited gains at most. About 30 million hectares of land are used for rice in the developing countries of East and Southeast Asia. An increase in fertilizer nitrogen usage on much of this acreage should result in at least a 10 to 25 percent increase in yield. This would supply a minimum of 4.5 million tons of additional rice annually.

Improvements in fertilizer nitrogen efficiency through the development of slow release nitrogen fertilizers and more effective timing and placement of conventional fertilizers would benefit all segments of the rice-growing world, both high-income and developing. Where 100 to 150 kg/ha of nitrogen are applied, actual plant uptake usually ranges from 40 to 70 kg/ha. If the efficiency of the fertilizer nitrogen used on rice were increased, the rate of application per hectare could be substantially reduced on highly fertilized fields and the additional fertilizer applied on currently underfertilized crops. Improving fertilizer efficiency in the developing countries has the potential to increase response from 10 kg grain/kg of nitrogen to 20 to 25 kg grain/kg nitrogen at modest rates of application (von Uexkiill 1975).

Except for rice, grain crops are grown throughout the world. Maize, for example, is widely grown on approximately 25 million hectares in Latin America, 16 million hectares in Africa, and 14 million hectares in the Far East. Yields in these three regions are about 1,000 to 1,200 kg/ha, with little variation among regions and little variation among countries within any region. Little or no fertilizer nitrogen is used on this maize since most of it is grown on small holdings under shifting cultivation. By contrast, the approximately 26 million hectares used for maize (corn) in the United States yield about 4,500 kg/ha (FAO 1974b). This land receives at least one-half of the 8 million tons of fertilizer nitrogen used in the United States, or nearly 150 kg/ha on the average.

The response of maize to nitrogen fertilizer has been maximal with applications of from 60 to 120 kg nitrogen per hectare in experiments in Puerto Rico and Brazil when the most efficient methods of application have been used. In these experiments yields of 5,000 or more kg grain/ha have been achieved (Cornell 1974-75). Rates of nitrogen application and yields obtained are not greatly different in the United States when the most efficient placement methods are used. Thus with

modest application of fertilizer nitrogen applied most efficiently, it may be possible to increase yields in many areas of Africa from 1,200 kg/ha to 2,000 kg/ha. If this were accomplished on 10 million hectares, the increased production would be 8 million tons of grain annually.

In many areas of the world substantial improvements in wheat yields also may be obtained. In Mexicc, for example, the combination of improved varieties, improved fertilization, and irrigation has raised yields from less than 1,000 kg/ha to 3,500 kg/ha in the past 25 years (FAO 1974b). This is the largest change of any nation in the world and indicates what can be accomplished with the proper combination of practices. Obviously, however, not all countries could accomplish changes of this order due to lack of water or other necessary resources.

Millet and sorghum likewise offer prospects for increased production through improved fertilizer nitrogen practices. These crops are grown in the drier regions of the world, where water is often the first limiting factor in crop production. Experiments in the semiarid regions of West Africa with millet have given yields of 2,000 kg/ha without nitrogen fertilization and 2,700 kg/ha with fertilization; sorghum experiments have yielded 1,700 kg/ha without nitrogen and 3,900 kg/ha with fertilization (Charreau 1974). Since average yields of these two crops are only 700 to 800 kg/ha in Africa, the potential for improvement is great even where rainfall is limited.

Improving Fertilizer Phosphorus Efficiency

Crop response to fertilizer phosphorus depends upon the amount and kind of phosphorus applied in the past, the levels and availability of natural and applied phosphorus in the soil, the amount of liming, and the physical and chemical makeup of the soil. Considerably less is known about phosphate fertilization of soils and crops of tropical regions than of temperate regions.

Research is needed to evaluate procedures for predicting the fertilizer phcsphorus requirements of food crops grown on a wide range of past fertilization practices and soil conditions, especially in tropical areas. Studies on fertilizer phosphorus sources, rates of application, and methods of placement need to be carried out to obtain maximum utilization of phosphorus. Particular attention should be given to the evaluation of the less soluble phosphates, including various rock phosphates, as sources of phosphorus on the highly weathered soils of the tropics. The chance for success is good.

Impact

Most soils throughout the world are naturally deficient in phosphorus. Thus whenever land has been brought into crop production, one of the first, if not the first, amendment added to increase crop production has been fertilizer phosphorus. As a result, many soils have received some phosphate at some time in their cropping history. The differences between phosphorus usage in the developing and the high-income countries are great, however. In the developing world phosphorus usage is about 3.5 million tons, a rate of 5 kg/ha, while in the high-income countries usage is 14 million tons, a rate of 35 kg/ha. In the centrally planned nations usage is about 7 million tons (FAO 1974b).

Studies of soil genesis and morphology in the tropical and subtropical regions have shown that the same weathering processes occur in both regions, with differences being related to degree of weathering intensity. Strongly acid soils high in exchangeable aluminum tend to be more widely distributed in certain tropical and subtropical regions than in temperate zones. Thus some soils of the southeastern United States are nearly identical chemically and mineralogically to soils in the Amazon Basin, the Brazilian campo cerrado, and the llanos of Colombia.

There is some indication, supported by some but by no means sufficient data, that fixation of phosphorus in tropical and subtropical soils has been grossly overrated. Evidence is becoming available to show that the residual carryover of phosphorous is such that little or no response may result for several years. Many of the tropical soils (ultisols and oxisols) are severely phosphorus-deficient, and the rates of phosphorus required to obtain adequate yields are higher than on less deficient soils. The residual effects of phosphorus applied at low rates to such soils are usually very small. It may be possible to utilize less soluble and less costly phosphorus sources to provide adequate phosphorus for plants, depending on the chemistry and acidity of a particular soil. Obviously, less phosphate has been used in the developing countries, and the levels are much lower on the whole.

If improved methods can be developed to predict fertilizer phosphorus requirements for the wide range of soil conditions found, fertilizer phosphorus can be used much more efficiently. Soils where crop response would be obtained would receive phosphorus, and those where response would not be obtained would not. Only after the necessary experimentation could any estimate of the area involved be predicted. These experiments,

however, should result in conservation of phosphate materials.

Potassium, Sulfur, and Micronutrients

Deficiencies of potassium, sulfur, and micronutrients are often site-specific. Information is needed to identify situations, especially in the developing countries, where deficiencies of these elements are likely to occur. Research is needed on the sorption of zinc in relation to phosphorus and lime additions. Adaptive research to evaluate the soil testing procedures used to determine levels of potassium, sulfur, and the micronutrients in a wide range of soils would improve the efficiency of fertilizer materials. The chance for success in this area is good.

Impact

Ultisols and oxisols usually have a very low capacity to supply potassium, and crops grown on them with adequate amounts of other elements often respond to additions of potassium. The low cation-retention capacity of these soils generally lowers their ability to retain applied potassium. On coarse-textured soils where rainfall is high, split applications of potassium may be required for increased efficiency. Coatings to reduce the availability of less soluble sources of potassium also may increase efficiency. Characterizing soils that respond to potassium and devising methods to use potassium fertilizers efficiently should result in increased food crop yields. There are perhaps 50 million hectares of sandy oxisols in Brazil alone that might respond to potassium fertilizer. Coarse-textured soils also are widespread in West Africa. (Kass 1976).

Sulfur content varies widely in tropical soils. Sulfur availability is also highly variable, depending on organic matter content of surface soil, soil texture, and previous soil management. Responses to added sulfur have been observed in Brazil on the campo cerrado, in many parts of Africa, and in parts of India. The groundnut and the forage legumes appear to be particularly sensitive to sulfur deficiency.

The number of experiments measuring plant response to sulfur have been few, and the areas that are currently or potentially deficient in sulfur are poorly delineated. Suitable methods of estimating available sulfur in highly weathered leached soils would result in more precise recommendations for sulfur additions. It is impossible to estimate the increased food

production that might be obtained since so little is known about the extent of sulfur-deficient areas. Where it is deficient, however, added sulfur brings substantial increases in yield.

Under traditional systems of cultivation in developing countries, with their relatively low yields, micronutrient deficiencies are seldom a limiting factor in crop production. But efforts to intensify crop production have brought into focus the need to supply one or more of the micronutrients. So far, no systematic study has been made of the role of micronutrients in crop production on tropical ultisols and oxisols, but deficiencies of zinc, molybdenum, and boron appear to be common. Increased yields have sometimes resulted from additions of micronutrients, but in other instances yields have shown no response.

The addition of zinc in experiments on the campo cerrado of Brazil, where zinc deficiency has been identified, has resulted in raising maize yields from 2,000 kg/ha to over 6,000 kg/ha (North Carolina State University 1974). Obviously, yield increases of this magnitude would go far toward solving food production problems. Increases in yield where micronutrient deficiencies exist, however, are more likely to be in the range of 10 to 30 percent. If micronutrient deficiencies exist in any large soil area, the potential for increased food production is greater.

Research on the utilization of plant nutrients also should lead to improved energy efficiency, particularly in the manufacture of fertilizer nitrogen and the processing of phosphate rock. Concurrently, it should be possible to reduce capital investment in the manufacture of fertilizer nitrogen. More judicious use of fertilizer phosphorus coupled with the use of less highly processed phosphate rock should result in reduced capital investment in phosphate processing facilities.

Increased plant nutrient efficiency will result in increased income to the farmer as the effective per unit cost for fertilizer is reduced and crop yields are increased.

IMPLEMENTATION

The chances of success in this research are high, particularly for work that concentrates on nitrogen and phosphate. A minimum expenditure of $10 to $15 million per year should be budgeted, with 50 percent or more of this total allotted to nitrogen research.

Research in improving fertilizer use efficiency will be primarily fieldwork at locations where the crops are grown. Routine laboratory work will be

required to aid in characterizing soils where the experiments are carried out. Many of the developing countries lack enough competent scientists to carry out such a program. Thus coordination with international centers will be essential. Regional research planning conferences should be useful. Basic work on such problems as denitrification might well be done in the high-income countries where such work has been started or is already well underway.

Fertilizer material development would be largely a function of IFDC and TVA. Private enterprises, however, ultimately would supply the fertilizer materials developed.

In many developing areas simple experiments that give a yes or no answer may well suffice if more detailed experiments have been conducted on similar soils. An example of this is the Cerrado Research Station near Brasilia where detailed field experiments are done followed by simple experiments in outlying regions to validate results obtained at the main station. In this way the time needed to obtain useful results can be shortened significantly. It is essential that a complete characterization of the soil at each experimental site be made. This should include taxonomic classification (see Profile 4, Subgroup B), using the U.S. Soil Taxonomy and other systems, as appropriate. Only in this way can the necessary technology transfer be accomplished. The results of all too many experiments in the past have proved to be useless for all practical purposes because soils were not characterized. Thus a reasonably modern soil chemistry laboratory will be needed in each country, along with staff to operate it.

A competent staff of field researchers also will be required. If possible, staff personnel should be indigenous to the country where the research is being carried out. Outside support should be confined to that necessary to get the research done. In some countries the outside support will be extensive, while in others only a redirection in effort will be required.

All aspects of the soil fertility research effort will need to be coordinated. Work on liming or micronutrients, for example, necessarily will have to be coordinated with work on fertilizer efficiency. Many developing nations are short of staff to carry out such a program. Additional personnel will need to be trained, and remaining deficiencies will have to be remedied by personnel from the United States and other high-income countries.

The research proposed here should have an immediate impact on food production in the developing nations since it should produce results that can be translated

immediately into recommendations to farmers. There will be a continuing need for this kind of research as long as new lands are developed, new fertilizer material is made available, and new food crop varieties come into existence. Furthermore, as fundamental research continues on biological nitrogen fixation, denitrification, enhanced photosynthesis by crop plants, and basic soil chemistry, there will be a need for the kind of adaptive research proposed here. Therefore, while the proposed research, if accomplished, will have an immediate impact on food production, the need for this type of research will continue indefinitely.

Profile 3

MINING AND PROCESSING METHODS FOR PHOSPHATE ORES

Phosphorus is an essential nutrient in the food production chain and is thus one of the keys to increased food production. In order to solve the world's food problem, phosphorus must be universally available in adequate quantities at a reasonable price.

Phosphate rock is the raw material used in the manufacture of most phosphate fertilizers. This kind of rock is widely distributed throughout the world (Notholt 1975). Reserves are extremely large and are being discovered at a rate faster than they are being consumed (Emigh 1972, 1975). Estimates of total phosphate ore reserves range from 81 billion tons (Table 7) to 1,300 billion tons. The difference in these estimates is due to the absence of an agreed-upon definition of what should be classed as reserve. Some estimates include the total phosphorus in known deposits; others include only that recoverable under present technological and economic conditions.

While total reserves appear to be ample to meet global needs for the foreseeable future (Lehr 1976, unpublished data TVA), only 15 to 20 percent of the world's known reserves can be used with present technology. The remaining 80 to 85 percent are considered unavailable under present conditions. Beyond that, the technology used to mine and beneficiate (treat) phosphate ores (P_2O_5) is extremely wasteful. It is common, for example, to discard up to 60 percent of the total P_2O_5 present in the initial ore processing.

Major phosphate mining activity is restricted to a few dozen of the several hundred known deposits. These easily mined deposits are found mainly in the United States (Florida and North Carolina), North Africa (Morocco), and the U.S.S.R. (Kola).

Barring major discoveries of new reserves and the development of new technology for beneficiation, rock

Table 7. 1975 known phosphate rock reserves

Location	Quantity (million tons)	Percent P_2O_5	Potential demand[a] (production units, 600 tpd P_2O_5)	Potential supply[b]
North America	7,413	23-32	45	297
Europe (including U.S.S.R.)	4,700	6-26	125	122
Africa	45,883	12-32	15	2,041
Latin America	14,000	31	38	603
Asia[c]	9,632	15-38	100	228
TOTAL	81,628	6-38	323	3,291

[a]Number of 600 (P_2O_5) tpd H_3PO units needed to supply projected demand for 20 years. UNIDO (1977).
[b]Number of 600 (P_2O_5) tpd H_3PO units known deposits can support for 20 years. Stangel (1976).
[c]Includes Japan and Oceania.

mines in Florida will soon have to shift to lower grade rock and may cease operation altogether shortly after the year 2000. Similar shifts to lower grade rock have already occurred in the Kola deposits of the U.S.S.R. and may soon occur in North Africa as well. The life of all of these deposits could be extended significantly if more efficient mining methods were practiced and better beneficiation methods were developed and used.

The problems in the mining and beneficiation of phosphate ores and their potential effects on food production are becoming matters of increasing concern. If new technology is not developed to utilize phosphate ores more efficiently, the cost of phosphate fertilizer can be expected to rise steadily in price over the next 25 years. There is also the danger of exhausting economically minable higher grade reserves. In addition, there are strong reasons to believe that "cartel style" price increases will continue to occur since most of the minable product is concentrated in only two or three nations. This cartelization is likely to pose a problem for countries lacking indigenous phosphate and/or foreign exchange reserves.

Any one of five developments, however, could alleviate the present trend of rising phosphate costs: (1) the identification and development of economically minable reserves at additional locations around the world; (2) the establishment of improved mining and beneficiation technology to improve the quality of present commercial phosphate products; (3) the development of new or improved acidulation technology specifically designed to handle inferior grades of raw materials; (4) the discovery of new or improved purification processes for chemical intermediates as alternatives to items 2 or 3; and (5) a modification of final product specifications (based upon actual agronomic performance in tropical and subtropical areas) from the existing high water solubility criteria developed from empirical use in the temperate zone.

Item 5 needs immediate attention in view of the availability of lower grade phosphate rock in many developing countries and the lack of agronomic data on the suitability of such rock for tropical soils, applied either directly or after partial treatment. Phosphate fertilizer products should be evaluated in terms of how their impurities react in tropical soil systems. Most impurities result in water-insoluble phosphate residues. We must determine whether these are detrimental to tropical or even temperate soils, especially in view of developing interest in maintenance forms of phosphate fertilizers.

RESEARCH NEEDS

Establish a system that will accurately assess the size, quality, and economic potential of global reserves of phosphate rock. No comprehensive, up-to-date data base on world tonnage, ore quality, and technical and economic potential of each deposit exists. This is partly due to incomplete geological data and partly because of the proprietary nature of the information. There is no standardized system of phosphate classification, and no consistently clear distinction is made between total resources and economic reserves. Therefore, work should be done to classify and characterize deposits in accordance with mutually agreed upon universal and specific standards. In addition, the establishment of comprehensive data on total resources, economic reserves, and marketable ores (also in accordance with a universal code) is vitally needed.

Such an endeavor, by its very nature, will be long term (15 to 20 years). It will require international cooperation and constant updating. Each of the above items (classification, characterization, technical and economic potential) is a research area in itself, but all of them must be integrated into a comprehensive analytical model.

Develop new and/or improved methods for the mining and beneficiation of phosphate ores. Development of better beneficiation methods is an open-ended program complicated by an ever-expanding variety of ore types requiring upgrading and the search for a technology that will permit rejection of specific impurities other than silica and allow, if it is present, the recovery of uranium. Specific attention should be paid to the processing of ores having the following properties: (1) high silica, (2) high organic matter, (3) high iron/aluminum content, (4) high content of aluminum/iron phosphates, (5) high content of carbonates, and (6) high chlorides (Hignett et al. 1976). Of these, high carbonate ores should be given first priority so that some of these vast deposits can be ready for use by the year 2000. An estimated two-thirds, or more, of all sedimentary phosphate deposits fall into this category.

Develop new or improved acidulation technology designed to handle inferior grades of raw materials. Assuming it becomes impossible or impractical to improve beneficiation or mining techniques, it may be possible to improve acidulation technology so that phosphate can be extracted from ore despite the presence of such undesirable substances as magnesium, iron, aluminum, and chlorine. Effective research in

this area will be dictated by the properties of the deposit in question.

Improve techniques to determine the suitability of phosphate rock for direct application to soil. While certain aspects of direct application of rock might be studied in short-term research, long-term trials are needed to measure the full benefits. To date, only a few sources of phosphate rock have been tested, and these for relatively short periods (less than three years). A large number of phosphate rock types should be tested over a wide range of climatic, soil management, and cropping conditions to determine their full value as phosphate sources.

Determine the availability of AOAC insoluble phosphates under tropical and subtropical conditions. Present Association of Official Analytical Chemists (AOAC) standards adhered to by the majority of world industry were established from agronomic comparisons of crop growth in temperate areas. It is possible that the insoluble phosphate formed by the presence of impurities such as iron, aluminum, and magnesium becomes available to plants when phosphate is applied to certain tropical areas, especially those with acid soils. Should this prove to be true, a modification of present AOAC standards would be justified and might make possible the utilization of ores currently deemed unsuitable.

Work should start immediately on determining the suitability of these so-called "insoluble" phosphates in tropical agriculture as well as thermal phosphates, such as Rhenania and fused magnesium phosphate. The nature of this work is very complex, and success will not be achieved easily. Nevertheless, the effort should be made because the potential benefits are considerable.

IMPACT

Approximately 80 percent of the known reserves of phosphate are believed to be unavailable under present technological and economic conditions. If research results in suitable mining, beneficiation, and processing techniques for each of the major types of phosphate rock, the life of currently known phosphate reserves could be extended from 30 years to as many as 230 years, even if an average loss of 50 percent in all ores is still assumed. Using a value of $200/ton of phosphorus, a very conservative estimate of 81 billion tons in reserve, a 25 percent P_2O_5 ore content, and a 50 percent recovery through mining and beneficiation, the value in 1976 dollars might equal nearly $2 billion. An advance of this magnitude is probably

unlikely, but genuine benefits can be achieved. All that is needed to improve the prospects is a small step in any of the four areas outlined.

Impact if Research Is Not Done

The present shift to low-grade rock with 50-68 BPL (bone phosphate of lime--a standard term used to describe the quality of phosphate rock) will result in a sharp increase in soluble impurities other than silica, thus causing higher prices for marginal grade phosphate. This soluble material reacts with the phosphate at a later stage in processing and makes the phosphates insoluble, in accordance with standard quality control methods. Removal of these impurities is costly. Leaving them in the beneficiated material will increase the amount of insoluble phosphate and make it increasingly difficult to meet current AOAC analysis specifications. If this problem is not corrected, it will become impossible and/or too costly to manufacture high analysis fertilizers, such as diammonium and monoammonium phosphates. Should this occur, current high analysis products will disappear and transportation costs per unit of plant nutrient will increase, leading eventually to a substantial rise in the cost of finished phosphates.

Phosphate Rock for Direct Application

Only about 7 percent of the total phosphate now used in agriculture is applied directly to the soil as phosphate rock. This method is used primarily in the U.S.S.R., France, Brazil, Sri Lanka, Malaysia, and Indonesia.

Recent research has revealed that up to 65 percent of all the rice grown in Asia could benefit substantially from applications of phosphate rock (Engelstad et al. 1972). Less precise research shows that at least 50 percent of the soils in Latin America and Africa also are responsive to phosphate rock. The potential savings in foreign exchange are substantial, especially for countries with reactive phosphate rock deposits. It is estimated, for example, that the equivalent of 323 plants, each capable of producing 600 tons of phosphate per day, will have to be constructed between 1980 and 2000 to meet annual demand, estimated at 76 million tons per year by the turn of the century (UNIDO 1977). About 40 percent of these plants may be built in the developing countries, at an investment cost of about $19.5 billion. Assuming that one-half of the future need could be met equally well through

direct application of phosphate rock, a potential savings of $9.8 billion could be realized. Over a 25-year period this averages out to about $392 million per year.

Since phosphate rock is not processed into concentrated forms of fertilizer, an additional savings would be realized through the reduction of production losses often estimated at 10 percent of total output. Using 50 million tons as average annual demand and assuming a cost of $200/ton, an additional savings of $1 billion would be realized. Thus gross savings per year would be in the neighborhood of $1.4 billion per year, assuming that the transportation cost is the same for ground phosphate rock as for concentrated phosphate fertilizers. Since the savings would be substantial, research into direct application of phosphate rock needs to be expanded.

Reduced Losses in Mining and Beneficiation

Present mining and beneficiation procedures are quite wasteful. Losses during the beneficiation process may range from 35 to 60 percent. Given current technology and estimated annual demand by the year 2000, annual losses in wasted ore alone would be 114 million tons of P_2O_5. Assuming a reduction of only 10 percent in losses during beneficiation, about 11.4 million tons of P_2O_5 could be saved, worth $2.28 billion. This estimate is conservative; it does not take into account actual mining costs, which are further increased by the need to remove up to 15 cubic meters of overburden waste per ton of final rock product.

Providing Wider Sources of Phosphate Rock

The greatest impact of this research would be to provide many developing countries possessing significant deposits of phosphate rock (India, Sri Lanka, Peru, Zaire, Angola, Brazil, Colombia, Senegal, Jordan, Egypt) with the economic ability to develop their own reserves. This can be a very important option for countries seeking to protect themselves against arbitrary price increases by major suppliers. As an example, Morocco raised the price of phosphate rock from slightly less than $8/ton in 1972 for 80 BPL material to more than $80 in late 1975--an increase of 900 percent. While competition, mainly from Florida producers, has since forced prices downward ($30/ton in late 1976), the quality of phosphate ores in Florida has begun to decline, and less Florida rock is moving

in international trade. It seems entirely possible that by 1990 Morocco and various Middle Eastern countries (Jordan, Egypt, Saudi Arabia) will supply 75 percent of the world trade in phosphate rock. If this were to happen, a cartel could easily be formed to control phosphate prices.

Alternatives to Associated Mining Industries

Deposits of heavy metal ores such as copper, nickel, and zinc are being discovered with increasing regularity in many developing countries, the most notable of which are Chile, Peru, Zaire, Zambia, the Philippines, Indonesia, Panama, and Brazil. The huge needs of the industrial nations will stimulate rapid development of these deposits. Because of pollution problems in the industrial nations and the desire of the developing countries to increase their foreign exchange earnings, an increasing number of the latter are attempting to accomplish both the mining and smelting processes.

The economics of this latter step is frequently marginal unless the nation is willing to vent the resultant air pollutants or can find an economic means of recovering and using the resultant sulfur or sulfuric acid. One very attractive method is to use the sulfuric acid in processing phosphate rock into finished phosphate fertilizers. If deposits of phosphate rock were located nearby or easily available, the sulfuric acid could easily be disposed of and two industries, mineral smelting and production of phosphates, might become economically feasible where neither would have been economical if developed individually. The development of the Jhamar-Kotra deposit (Rajasthan, India) using by-product acid from the Hindustan Zinc Company is an example. The potential benefits of such joint efforts are great. Other countries where this could happen include Peru, Chile, Indonesia, Mexico, the Philippines, Zaire, and Zambia.

IMPLEMENTATION

There are few precedents within the United States, or within the world phosphate industry for that matter, to suggest how the proposed research should be done. The phosphate fertilizer industry has generally ignored research and development of mining and processing methods to improve the recovery and utilization of phosphate ores. Only a few companies operating in Florida and North Africa have even attempted to conduct any research. Furthermore, these

companies are not likely to spend funds to make phosphate processed from other deposits more competitive with theirs. While public and private producers should eventually take the lead in this overall effort because of their own interests, the manner in which this can be accomplished is unclear. The impetus for most of this research will have to come from government agencies.

Such agencies as TVA, the U.S. Bureau of Mines (USBOM), and the U.S. Geological Survey (USGS), along with equivalent agencies in other countries including the Centre d'Etudes et de Recherches des Phosphates Mineraux (CERFOS), the Australian Mineral Development Laboratory (AMDEL), the Commonwealth Scientific and Research Organization (CSIRO), and the Japanese Phosphate and Compound Fertilizer Manufacturers Association (JAPCA), and internaticnal research centers, such as the International Fertilizer Development Center (IFDC) and the International Superphosphate and Compound Manufacturers Association, Ltd. (ISMA), should join forces to work on the problems. IFDC, with its present mandate to develop new and/or more efficient fertilizers for the developing countries, seems ideally suited to take the lead in this task.

Establish a system that accurately describes world phosphate reserves. The first step should probably be to determine the magnitude, location, and quality of present phosphate reserves. This would entail pooling existing information. IFDC, with the help of USBOM, USGS, ISMA, AMDEL, BRGM (Bureau de Recherches Geologiques et Minerales), and others, could collect, consolidate, and maintain this information. This work should begin immediately, with substantial information becoming available within five years and--if politics permit--coordination of the global development of phosphate deposits within 15 years. Maintaining the data will be a continuing task. The cost of this work is estimated to be $1 million per year.

Characterize all known deposits lacking complete information pertaining to the chemical and physical properties. Gaps in knowledge about the chemical and physical properties of important phosphate deposits should be documented. The agencies most likely to be involved in carrying this out would be AMDEL, USGS, USBOM, and IFDC. IFDC, through its centralized collection of data, also could coordinate this effort. Results could be expected in three to five years, with a major impact within 15 years. This also would be a continuing task. Depending upon the depth and extensiveness of the work, the annual cost would range from $5 to $15 million.

Determine the suitability of phosphate rock for direct application to soils. A limited amount of work on the suitability of phosphate rock for direct application is already underway. RENO, a French fertilizer company, has been a pioneer in this kind of research. More recently, TVA, IFDC, and some of the international centers, such as

IRRI, CIAT, and IITA, have become interested in investigating the prospects for direct application of phosphate rock to soils in the tropics. This research, along with work on impurities in concentrated phosphates, should be expanded and organized into several cooperative networks either along commodity lines or soil-climate groupings. This work is long term and could begin immediately, with IFDC serving as research coordinator. There are enough trained personnel in U.S. and foreign universities and institutions to carry out this work. Meaningful results can be expected within 5 to 10 years, but the full impact cannot be expected for 15 to 20 years. The annual cost of this work would be small, probably no more than $750,000 to $1 million.

Develop a methodology for improved beneficiation and acidulation of problem phosphate ores. Major work in this area should be delayed until the first two steps listed above are completed and a full assessment of present deposits is made. Lack of sufficient personnel prevents anything but the most basic work at the present time. The cost may be considerable, depending upon the deposits under investigation. Initial costs of $1 million per year should be expected, with an increase to about $5 million a year after the first two steps have been completed.

A much larger and better distributed pool of trained fertilizer and phosphate mineral technologists is needed. Unless this pool is established, it will be impossible to implement an aggressive research and development program. University centers and industry must begin a cooperative effort to furnish the qualified persons needed to implement new programs. There is no established university center for broad-scale beneficiation research, although minor programs are conducted at North Carolina State University, the Colorado School of Mines, and perhaps a few others. In fact, there is no center of phosphate chemical technology in the United States apart from TVA and the newly established IFDC. The modest CERFOS staff is a singular example in western Europe, but it no longer emphasizes basic research. Australia and New Zealand have three or four small groups, the most notable being AMDEL, CSIRO, and the New Zealand Fertilizer Manufacturers Research Association (NZFMRA).

While TVA and IFDC should be the major research agencies, a number of other institutions should be strengthened in order to train qualified personnel and carry out certain key aspects of the proposed research. This strengthening should begin now, although its full impact on the industry will not be felt for 10 to 20 years.

The lag in phosphate research suggests that a condition that occurred in the iron ore mining and processing industries may be repeated. The latter, when suddenly faced with a raw material crisis, had to mount expensive crash research and development programs in ferrous metals. A similar crisis in the phosphate industry may emerge by the

mid-1980s. Whether producers and governments face up to the need to commit significant amounts of funds to research in phosphate mining and processing now will directly determine the role phosphorus will play in meeting world food requirements in the year 2000.

Profile 4

RECYCLING ORGANIC WASTES AS NUTRIENTS AND SOIL CONDITIONERS

The use of organic wastes as a source of plant nutrients is an ancient practice that until recently was an important feature of virtually all established agricultural systems. It was not until the post-World War II period that organic wastes were relegated to a subordinate role by the huge growth in supplies of chemical fertilizers and concomitant reductions in price. Sharp price increases for all inorganic fertilizers between 1973 and 1975, however, resulted in at least a slight interruption in the continuing rise in chemical fertilizer use throughout the world. These price increases particularly affected developing countries that depend on imports for the major portion of their fertilizer supply, since such countries have limited available foreign exchange. The consequence has been a resurgence of interest in agricultural use of organic wastes. The use of these wastes is culturally acceptable in the developing countries and also has the advantage of substituting labor for part of the high capital costs of chemical fertilizers. Alternate ways of providing for adequate plant nutrition are discussed in numerous other profiles of this and other study teams.

Although there is a tendency to regard organic wastes simply as an alternate source of nitrogen, phosphorus, and potassium (N, P, and K), in fact such materials differ in many crucial ways from inorganic fertilizers. They generally are bulky and unpleasant to use. On the other hand, they contain a large amount of many different organic and inorganic compounds in addition to the primary plant nutrients. The soil conditioning value of organic material is very beneficial, especially in soils with low organic matter content. The research described in this profile is intended to determine both the benefits and disadvantages of using organic materials.

Successful implementation of widespread organic waste recycling could have a significant effect on vexing questions of environmental quality. While the benefits to food production may be more evident in the developing world, the ecological ramifications may be of more immediate benefit in the high-income countries.

RESEARCH NEEDS

Developing Waste Treatment Systems (Short-term Effort)

Onfarm Wastes

Many forms of wastes generated on the farm (crop residues, animal dung, household wastes) can be recycled for use on agricultural land; in many areas this is an old established practice. Such materials, however, can be improved as soil conditioners and fertilizers as well as made easier to handle by appropriate treatment, such as composting. The principles of composting are reasonably well known already, and no major scientific breakthrough is needed or expected. But an effort should be made to develop easily constructed, inexpensive composting systems designed to handle the quantity and types of wastes typically found on farms in various developing countries. These systems should be designed to eliminate the potential disease and odor problems from wastes while conserving nutrient values to the maximum feasible extent. A complementary effort should be devoted to the development of inexpensive equipment for applying the materials to cropland and incorporating them into the soil to minimize losses. The chance for success of this work is high.

Off-farm Wastes

Urban sewage and refuse, food processing wastes, and other materials can be improved as fertilizers and soil conditioners either by composting or by anaerobic digestion (methane generation). The latter method, which is more difficult, permits recovery of the energy value of wastes when the quantity and composition of the wastes are appropriate. Both treatment methods largely retain the nutrient content of the wastes and eliminate the potential health and environmental problems that might otherwise arise.

Both types of treatment are already receiving appreciable attention in the United States and other high-income countries. An applied research and

development program is needed to develop systems that require little capital and that would be suited to the kinds of waste collection and sewage treatment systems found in the various developing countries. There also should be a concurrent research effort to evaluate the potentially adverse environmental effects (leaching to groundwater, accumulation of heavy metals) of applying these materials to cropland, with the goal of minimizing these effects. The chance of success for this work is good.

Improving Data on Crop Response to Organic Wastes (Medium-term Effort)

The simplest way to compute the value of a particular organic material for application to soil is in terms of its nitrogen, phosphorus, and potassium content. But this widely used procedure ignores fundamental differences between organic materials and conventional chemical fertilizers, such as the large organic content and slow nitrogen release characteristic of organic wastes. It is likely that the yield response of a particular crop to organic waste materials may be different from the response to the same quantity of N, P, and K in conventional fertilizers; likewise, the difference in immediate availability may result in a different efficiency of nutrient uptake by plants.

In order to make the best use of available organic materials, it is necessary to have more complete data on the responses of various crops to various kinds of organic fertilizers and amendments. This research should be specific to a variety of important soil types and cropping systems of the developing world and should include various combinations of organic and inorganic nutrients. These experiments should include continued applications over a period of several years to allow the full effect of organic matter additions to be observed.

Improving Understanding of the Effects of Adding Organic Matter to Soils (Long-term Effort)

Achieving the full potential advantages of organic materials ultimately will require a more thorough knowledge of the transformations that organic matter undergoes in various soils and different climatic situations, and its interaction with the soil characteristics that determine crop productivity. This research would be more general and of a somewhat less applied character than the program described above.

Instead of simple observation of what the effect of organic materials is on crop yield, it would be intended to determine how and why this effect occurs.

The questions with which this research would be concerned, particularly in developing countries where less knowledge is available, would include:

-- What increases, if any, in soil organic matter content will result from continued application of organic wastes at various rates, in various cropping systems, and for various soil and climate conditions?
-- How do organic materials affect the efficiency with which nutrients in chemical fertilizers are used by crops?
-- What effects will additions of organic matter have on soil structure, water retention, aeration, etc.?
-- To what extent can additions of organic matter help reduce erosion?
-- Can additions of organic matter be used to enhance biological nitrogen fixation?
-- How reliably are pathogen problems eliminated by composting?

IMPACT

A simple summation of the plant nutrient content of various forms of organic wastes is sufficient to indicate the potential value of such materials as a partial substitute for conventional fertilizers. A recent World Bank study (1974) has estimated that in the developing world the N, P, and K content of all organic wastes in 1971 came to 48, 16, and 39 million tons, respectively. The largest contributor was cattle manure, which accounted for approximately one-third of the total for each nutrient.

In contrast to the 103 million tons of these three nutrients in organic wastes, total consumption of N, P, and K in the form of inorganic fertilizers in the developing world in 1971 was approximately 13 million tons. Thus the nutrient content of organic wastes was about eight times that of all inorganic fertilizers used. The value of these nutrients in 1973 prices was about $16 billion.

These figures, of course, represent only a theoretical value, not the quantity of nutrients actually obtainable from wastes. Some loss of nutrients is inevitable during the collection, storage, and use of these materials. In addition, economic factors and the lack of certain facilities will make it inadvisable, if not actually impossible, to recycle all organic waste. Nevertheless, the enormous size of the

potential supply makes it evident that even small increases in the proportion of nutrients that are recycled would result in appreciable increases in food production, either by adding to the total supply of plant nutrients in a given area, with a resulting increase in yields, or by permitting a reduction in inorganic fertilizer use, thereby freeing fertilizer for use elsewhere.

A good example of the way organic wastes can enhance food production is provided by China, a country that appears to have achieved virtual food self-sufficiency despite having a relatively high population/cropland ratio and a relatively low level of industrial development. Recycling of wastes is an important principle in Chinese agriculture. While it would be a great oversimplification to ascribe the level of production solely to this practice, it seems clear that waste recycling is a major factor. It is worth noting that, until fairly recently, conventional inorganic fertilizers have played a relatively minor role in Chinese agriculture, and that recycling of organic wastes is still widespread despite the recent expansion of fertilizer manufacturing facilities. This is not to suggest that other developing countries can solve their own fertilizer problems by following the Chinese example. The availability of labor, the prevalent cropping systems, and the distribution and density of the population and livestock are all important considerations in determining the best way of using organic wastes in a particular country. Still, the Chinese example offers convincing evidence that the potential value of organic wastes actually can be realized in practice.

The research described earlier is intended to increase the contribution that organic wastes can make to total food production. It can be expected to increase the benefits obtainable from recycling a given amount of wastes, and to increase the amount recycled. To some extent, of course, achievement of the former will serve to achieve the latter as well, i.e., if the benefits of organic wastes can be increased, more recycling will be practiced. However, recycling also can be stimulated through better knowledge of the benefits of organic materials. The use of organic wastes entails appreciable problems since they generally are bulky, difficult to collect and store, and unpleasant (and perhaps pathogenic) to use. Consequently, many wastes will not be recycled unless the benefits are clear and convincing. Their value as soil amendments, in contrast to their use as a source of nutrients, may be relatively subtle and may not become apparent until after extended use accompanied by careful study. Consequently, the proposed research is

intended to determine more accurately the value of such material as now used as well as to increase its benefits by developing more efficient methods.

The research programs we have outlined can be expected to have the following specific implications for organic waste recycling:

-- Elimination of the health and odor problems of wastes, especially human wastes; increases in the availability of nutrients for plants; reduction in the bulkiness of the material; improvement in ease of storage and application of the material; and recovery of the energy value of the waste (anaerobic digestion only).
-- Permit more rational decisions as to whether recycling is justified under a particular set of conditions; permit more rational decisions on whether construction of new facilities to increase recycling is justified; more efficient allocation of available wastes among various crops; and more efficient combinations of organic wastes and conventional fertilizers.
-- Possible increases in recycling through fuller recognition of long-term benefits; development of modified cropping systems to take fuller advantage of organic wastes; and possible reclamation of soils that have been depleted or eroded seriously due to poor management.

It is impossible to predict to what extent this research, especially its medium-term and long-term components, will actually result in greater use of organic materials. Indeed, one of the reasons for the work is to acquire a better picture of the benefits that could result from greater use. Nevertheless, increasing research into organic wastes very likely will reveal that the full potential benefits are not yet recognized even though costs and problems are largely understood. Therefore, at least some increase in both the perceived and actual benefits obtainable from organic wastes can be expected. This outcome will presumably lead to their playing a more important role in agriculture in the developing world in the future. As was seen earlier, even a small increase in the use of organic wastes would be equivalent to a substantial fraction of the entire supply of inorganic fertilizer in the developing world.

The chance of success for this research is good, particularly over the long run. The costs will increase with time, as land and population pressures increase, from $800,000 to $1 million per year initially to $5 to $10 million per year after 10 years.

IMPLEMENTATION

The development of improved composting and anaerobic digestion systems could be carried out at any of the institutions currently involved in such work in either the developing or high-income countries. If done in a high-income country it would be advisable to have the assistance of persons familiar with economic conditions and agricultural practices in various developing areas. In addition, the systems should be tested under actual conditions before they are introduced throughout a large area. It also may be desirable to utilize social scientists during this testing phase to help ascertain and overcome the reluctance of local populations to make greater use of organic wastes.

Research on yield response must necessarily be undertaken at widely dispersed locations selected to cover a broad range of soils, cropping systems, and other conditions throughout the developing world. This work is similar in principle to fertilizer yield response testing. No additional expertise or facilities is required beyond those found at regular experiment stations.

The proposed work on the long-term effects of organic matter will require a fairly intensive and sophisticated investigation. This would probably best be undertaken at a small number of excellently staffed and well-equipped research centers in various parts of both the high-income and developing countries. The financial support for such research should be stable enough to permit the planning and execution of long-range experiments. As research results become available, additional work could be undertaken at a larger number of facilities in order to apply basic results to a broader range of specific circumstances.

Profile 5

FACTORS THAT DETERMINE THE SUPPLY, PRICE, DISTRIBUTION, AND USE OF FERTILIZER

There are serious constraints to increased fertilizer use in the developing countries, and these constraints are likely to persist unless action is taken to eliminate them or minimize their effects. These constraints became particularly apparent during the recent food crisis (1972-74). The first constraint is that planners in many developing countries have a very limited understanding of the economic and social factors that persuade farmers to increase food production through increased fertilizer use. This lack of understanding, coupled with extensive efforts to stimulate fertilizer use during the recent crisis, resulted in severe imbalances in benefit-cost relationships in food production at the farm level (Stangel 1974b). Extremely high fertilizer prices resulted in a drop in use of 2.1 million tons from the 1974 record of 84 million tons of nutrients. This was the first time a drop in the global use of fertilizer had occurred since 1945 (Couston 1976).

The second constraint is that many planners apparently have a poor understanding of the factors that govern fertilizer supply. It was uncertainties of supply and the anticipated effect of fertilizer shortages on food production programs that led the governments of many developing countries to assume, either partially or totally, the tasks of procuring and distributing fertilizer and, in many cases, producing it as well. The reaction to shortage was the building of excessive inventories and, in some cases, the construction of excessive production facilities as a hedge against the future. A lack of ready access to critical information, particularly from outside the country, prevented planners from taking into account the actions of other producer and consumer countries.

The 1972-74 sequence of events clearly showed that no information system existed to integrate information on fertilizer use and production into a set of

alternatives that would allow national planners to make rational judgments about inventories and production capacity. While such a system may have been less important in the past because of the leveling effect of the free enterprise system, the increased involvement of the public sector in the fertilizer industry makes it important to create such a system.

It may be argued that the recent fertilizer shortage was the result of an extraordinary set of concurrent events that are not likely to happen in the future. However, such a conclusion assumes that substantial increases in the use of fertilizer will not be needed to meet the increasing demand for food.

RESEARCH NEEDS

There are several areas of research that need immediate and sustained attention over the next 15 to 20 years if the disruptive effects of cyclic imbalances in supply and demand are to be avoided or at least minimized.

Establish a better understanding of the economic and social factors that influence the farmer's use of fertilizer. A central cause of confusion during the recent food and fertilizer crises was the lack of information, or at least confidence in existing information, on the factors that influence the farmer's consumption of fertilizer and production of food. Thus a major research effort is needed to clarify and identify the economic and social factors that influence the adoption of and demand for fertilizer. While it is generally believed that profitability is the key factor, experience to date suggests that the situation is much more complicated. Scarcity of capital and credit, risk, uncertainty, and attitude toward change are but a few of the factors that must be considered more thoroughly. Government planners and policymakers, in particular, do not have the answers, nor a full awareness of the importance of understanding these factors in motivating the farmer to use fertilizer. Without this understanding, large increases in food production are not likely to occur in the developing countries.

Both the long-term and short-term prospects on this area of research are good.

Develop improved methods of demand analysis and forecasting of fertilizer use. At the peak of the recent fertilizer shortage there were wide discrepancies in fertilizer supply and demand forecasts by TVA, UNIDO, the World Bank, FAO, and USDA. This led to considerable confusion and helped cause the current (1976) situation of oversupply, overinvestment, and

manufacturing overcapacity. While numerous attempts have been made to improve forecasting, most have proven inadequate to describe anything but the most aggregated and mature markets (Griliches 1958; Heady et al. 1966; S. W. Park 1975, unpublished data, dissertation, University of Minnesota; Mukhopadhyay 1975). Present techniques have proven extremely inadequate in forecasting fertilizer use over the short run, in new markets with little historical data, or in situations where major changes in the input/output relationship develop. A major effort should be made to improve the accuracy of short-run (6 to 24 months) and long-run (5 to 10 years) demand analyses and forecasting, particularly in new markets where volatile situations are more likely to occur. Refinement in forecasting is needed especially to predict use of fertilizer by small farmers as contrasted to use by the larger commercial farmers. This type of work is needed for each of the major nutrients (nitrogen, phosphorus, and potassium) and, where feasible, for major fertilizer products as well. Methodologies need to be developed that can predict more effectively the circumstances that cause major shifts in fertilizer use among crops and regions within a country and that can estimate the effect this may have on food production. The chance for success in this work is good but will require a long-term effort.

Develop a methodology for reliable forecasts of fertilizer supply. There are many instances in which investments in new capacity either do not occur or occur at too slow a rate to satisfy demand. There also has been a nagging problem of underutilized capacity (Stangel 1974a). No methodology has been perfected to predict either short-term (6 to 24 months) or long-term (5 to 15 years) fertilizer supply. Just as in the case of demand, reliable forecasts of supply cannot be developed without reliable data. Research is essential to identify the factors that determine aggregate supply. Such a methodology should be equally applicable to local, national, regional, and global situations and should allow comparisons between programs leading toward regional cooperation (sharing of markets and raw materials) and programs stressing independence. The chance for success of this work is high.

Develop a system that will collect and transmit reliable information quickly and cheaply to central points. Assuming that the proper methodology is developed and that the factors that influence fertilizer supply and demand are identified, a network must be developed that will allow this information to be integrated quickly and inexpensively. Research needs to identify key indicator crops, regions, and countries likely to influence overall fertilizer

demand, international prices, and future investment decisions. Also communication systems that will permit rapid transfer of data both to central points and to points of implementation should be developed. The chance for success in this area is high.

Develop investment analysis tools for policymakers. The central problem facing the decision maker in developing countries is to determine where scarce finances will have the greatest impact on food production. Fertilizer programs in their present form are very expensive and therefore very much in the forefront as competitors for scarce financial resources. What is needed is a methodology that can integrate the important factors determining supply and demand. Such a methodology must be able to present feasible alternatives that can be understood by decision makers. Specifically, the system should assess alternatives in the use of raw materials, capital, plant investment, transport and storage systems, and personnel required to produce, market, and use fertilizer. The information should be factual, comprehensive, and readily available. The chance for success of this work is good.

IMPACT

One can always argue that it is virtually impossible to identify and integrate all the factors influencing either supply or demand. It is true that judgments regarding future impacts will always have a subjective bias. Still, significant advances have been made in the analytical ability to simulate future supply and demand levels. The potential benefits from further refinement of these research techniques are substantial.

Greater Production of Food

Had the effects of the recent fertilizer shortage been minimized, the food supply available to the developing world at the time of the crisis would have been greater. Assuming a current global fertilizer use rate of 80 million tons of nutrients, a static rather than 7 percent growth rate, and a 10 to 1 yield of food per unit of plant nutrients, as much as 56 million tons of food may not have been produced in 1975 that would have been produced had normal fertilizer prices prevailed. This represents about one-half the total amount of food and feed grains moving in international trade, and is about one-half of 1975 grain consumption in India. Much of this estimated loss might have been

avoided had a more efficient system been available to detect quickly and accurately changes in farmer attitudes toward fertilizer use and to predict fertilizer demand as a result of various policy decisions. Such a forecasting system would provide the fertilizer industry, both public and private, with better indications of the capital requirements needed for additional plants. It also would provide an assessment of whether there will be a predictable supply.

Fewer price fluctuations would aid in particular the food-short countries such as India, Bangladesh, Indonesia, and the People's Republic of China, where many people subsist on marginal diets and foreign exchange for fertilizer imports is in short supply.

The long-term benefits of this research would be even more substantial. It has been stated elsewhere in this report that worldwide fertilizer use will increase nearly fourfold by the year 2000, and that the proportion used by the developing countries may increase from its present (1975) level of 26 percent to approximately 40 percent by the year 2000. Pinstrup-Anderson (1976) estimated that between 50 and 67 percent of all increases in food production are likely to be related to increased use of fertilizer. Thus fertilizer has extremely important implications for Asia, where land resources are limited and substantial increases in food production must come through increased production per unit area of land.

More Efficient Use of Local Currency and Scarce Foreign Exchange

The situation in Indonesia is a specific, yet somewhat typical, example of the cost to a society when accurate information is not available or not properly communicated to the appropriate policymakers. As of April 1976, Indonesia had nearly 2.5 million tons of fertilizer in inventory (1976, personal communication, AID Mission, Jakarta, Indonesia). This was 10 times the normal level, but was accumulated in response to the crisis. This fertilizer was purchased at a cost of nearly $750 million (assumes an average C.I.F. [cost, insurance plus freight] price of $300 per ton of product). Furthermore, Indonesia, with the help of the World Bank, the Asian Development Bank, and others, is actively engaged in building plants that will not only supply all of Indonesia's nitrogen needs by 1977 but also will make it a major nitrogen exporter by 1980 (Shields et al. 1975). This will occur at a time when the world faces surplus nitrogen capacity.

The annual interest costs of current inventories and projected excess capacity are estimated at $90 to $100 million ($67 million on inventory and $30 million on excess plant investment of $300 million). In addition, there is a tremendous cost in lost opportunities. The Indonesian government has prematurely tied up $800 million to purchase fertilizer and fertilizer plants, money that could have been used more effectively elsewhere in the economy.

Indonesia is not the only example of inefficient allocation of scarce resources. Similar errors in judgment, although of a lesser magnitude, have been made in the Philippines, Bangladesh, India, Venezuela, Ghana, Colombia, Ecuador, Brazil, and for that matter, the United States and Western Europe.

It can be argued that optimum inventory efficiency must be balanced against the security such a fertilizer production capacity affords a nation. Such an expenditure, it can be argued, must be put into the same context as national defense spending. Yet, however justifiable the stockpiling of excessive production capacity may seem on a national security basis, such hoarding of fertilizer could have taken the lives of many people if favorable weather had not prevailed globally in 1974 and 1975.

Provide an objective basis for interregional and global cooperation in utilizing scarce raw materials. Perhaps the ultimate benefit of forecasting and demand analysis research is the eventual quantification of the economic advantages that can accrue when nations pool their resources to produce fertilizer. At present, for example, the energy-rich OPEC countries flare enough natural gas each year to fuel 250 thousand-ton-per-day NH_3 plants while the rest of the world is expanding production capacity utilizing costly and frequently less efficient materials to produce fertilizer (Stangel 1976). Such a situation is irrational and wasteful of increasingly scarce nonrenewable resources. Regional and global cooperation in using scarce resources to produce fertilizer must become a reality.

Foster, et al. (1976) present an example of the potential capital savings to the individual nations of the Association of South East Asian Nations (ASEAN) through regional cooperation. Full regional cooperation in nitrogen and phosphorus production would mean a savings of at least $183 million in five years plus annual savings in operating costs of upwards of $119 million. Such examples also could be developed for the Bolivia, Chile, Colombia, Ecuador, Peru Common Market (ANDEAN) region, the OPEC countries, West Africa, and elsewhere. Some of the methodology for improved demand analysis has been partially developed by the World Bank, IFDC, and others.

IMPLEMENTATION

While the research topics proposed are not new, efforts in these areas need to be intensified. It is recommended that the proposed research be carried out through a research method called Systems Analysis/Operations Research (SA/OR), employing a multidisciplinary approach to food and fertilizer problems.[3]

The SA/OR concepts have existed for 35 years and have been applied elsewhere. They appear to be ideally suited to an analysis of the current fertilizer situation in individual countries in order to improve production and distribution systems (NRC 1976b). Problems in inventory control, production scheduling, transportation control, maintenance, deployment of warehouses and retail centers, and investment in new production units have all been thoroughly studied in the United States and to some degree in Europe and Japan as well. Many of these concepts also can be applied to the problems faced by other countries in promoting fertilizer production and use. The SA/OR approach can help the developing countries in three ways: (1) through the development of models and analogs to provide an understanding of the factors of a sound fertilizer system and how they interact; (2) by predicting the probable consequences of alternative actions and policies not only within, but to some degree, outside the fertilizer system; and (3) by aiding in the selection of the best choices given certain goals and constraints.

This kind of analysis should be carried out at a number of levels including, as a minimum, the regional and country levels. The proposed research should be organized along three lines, each employing the network concept.

The first line of research should deal with the economic and social factors that determine fertilizer demand at the farm level. Scientists from the developing countries and international centers should be organized and assigned the task of determining and assessing these factors. A good personnel base exists in U.S. universities, various international centers, and such countries as India, Brazil, Bangladesh, and Kenya.

The second line of research, developing a methodology to identify the relevant data influencing methods of forecasting supply and demand, also should be organized by establishing a network of interested scientists and technicians in countries capable of carrying out such studies. Key indicator countries might include India, People's Republic of China, United States, U.S.S.R., Japan, Colombia, Brazil, Zaire, Saudi

Arabia, Morocco, Sri Lanka, and others. National research centers (university or other) would probably serve as the main focal points. The World Bank, UNIDO, FAO, or possibly the newly formed International Fertilizer Development Center might act as coordinator.

The third line of research should deal mainly with establishing a system to collect and process relevant fertilizer data from indicator countries at various national, regional, and international centers, sc that all the data come together at a central point and, in turn, can be assessed by any of the cooperating units. A central processing facility could possibly be managed and operated by IFDC, the Statistics Unit of FAO in Rome, the Fertilizer Unit of the World Bank, the Investment Center of UNIDO, or through a combined effort of these organizations.

The data gathered through the above three lines of endeavor would then be available for use in policymaking decisions. With adequate access to the central processing facility, scientists in the field or at regional centers would need to supply the proper methodology and then determine the alternatives open for use in solving a given problem.

The probability of success in this effort is high since segments of this system already exist in the fertilizer industry. What is lacking is the integration across the segmental boundaries. Much of what is being proposed has already been accomplished in the oil industry, in banking, and in commodity trading. The benefits of this research would be almost immediate (within five years), but the main impact would be felt in about 10 to 20 years, at which time the projected world food crisis will be at its peak.

The initial price of such research could be substantial, but it would be recovered either through the avoidance of another fertilizer crisis, thereby preventing overprocurement of fertilizer and the unnecessary building of fertilizer plants, or conversely, by increased use of fertilizer needed to meet food production needs. The main cost will be the establishment ($7 to $9 million) and maintenance ($1 million per year) of a data gathering network. Once the network was operational, a considerable portion of the cost of securing information could be recovered from private industry, governments, and financial institutions. An additional cost of $1 million per year will be needed to ensure proper research on modeling and forecasting. This will be particularly critical during the first 10 years of the effort. This effort should be coordinated with other implementation systems proposed by Study Team 8.

Report of Subgroup D, Study Team 4

ENERGY AND EQUIPMENT

INTRODUCTION

The major intent of the research proposed by the World Food and Nutrition Study is to provide for the well-being of an increasing number of people through a more adequate diet in the face of finite and decreasing energy reserves. This will require increased annual food production per hectare since farmland is limited in most of the food-short nations. Increased farm productivity in the past has come primarily through more intensive use of energy resources to achieve higher yields.

Wherever food is grown and consumed, energy must be expended at various points in the food production process. In the high-income countries, the energy input is generally in the form of petrochemicals applied through tractors and other sophisticated equipment. At the other end of the spectrum, Stout, et al. (1973) report that people are the major source of energy used to produce food in such less developed parts of the world as equatorial Africa. By means of high inputs of energy, including both the energy expended in agricultural operations and the energy used to manufacture fertilizer, pesticides, and farm equipment, the U.S. farmer can produce food for approximately 50 people. In countries where human and animal energy are still dominant, however, the farmer often has difficulty producing enough food to feed one family.

Surprisingly, the amount of nonhuman energy used in the developing countries is quite large. Makhijani (1975) estimates energy use to be equivalent to 30 million barrels of oil per day. Revelle (1976) estimates that total energy used by farmers in rural India exceeds that used by U.S. farmers. The energy used in the developing countries does not come from commercial fuel, such as oil or coal, but from indigenous supplies of wood, cow dung, food crops, and other renewable sources that do not pass through market channels. Yet, while the amount of energy of all kinds consumed per capita in developing nations is substantial, the useful work from that energy is

limited due to inefficient conversion methods. Human and animal power are very inefficient in the hot tropical climates found in most developing countries. Even the "nonworking" people who gather wood, grass, or dung for cooking fuel spend considerable time each day scavenging for enough material to cook their meager meals, thus using up a substantial portion of the energy contained in the food they eat.

Many forms of energy exist, but to be utilized they have to be transformed into work through chemical processes or mechanical devices. Several alternative energy systems exist in agriculture, as shown in Figure 3. Once energy inputs are transformed into a "useful energy pool," they can be used for food production, as shown by the list of final uses in Figure 3. Biological materials provide a major energy input for Third World countries. Some of these are consumed directly as food by man or animal, and others provide fuel for fire or feedstocks for chemical processes that provide useful energy. Other types of energy inputs (e.g., petrochemicals) would be useful in the developing countries, but several factors limit their use. In most of these nations energy use must be balanced by the need to conserve scarce foreign exchange. All sources of energy must be evaluated relative to cultural, environmental, and economic factors so that the proper combination for most efficient utilization can be selected.

The fate of the agricultural worker displaced by mechanization has been one of the most controversial subjects in the introduction of new technology to the developing nations (Merle 1975). This is a logical concern since agriculture supplies about two-thirds of all jobs in most of these nations (FAO 1974c). There are many variables involved, as revealed by the divergent information that comes from nations where agriculture is highly mechanized. The United States, for example, now uses only .02 farm workers per hectare of cropland. There were 1,562,000 fewer hired workers on U.S. farms in 1974 than in 1935 (U.S. Department of Labor 1975), while tractors in use on farms grew by 3,328,000 (Intertec Publishing Corporation 1975). During this same period, the number of farms in the United States decreased by 58 percent and consequently the number of hired workers per farm remained essentially the same. Japan, on the other hand, which uses five times as much power per hectare as the United States, continues to use about two farm workers per hectare, more than any other nation. For intensive agriculture where land is scarce, mechanization can be utilized in combination with high labor inputs.

There are two major needs related to farm labor in the developing nations that may appear to be

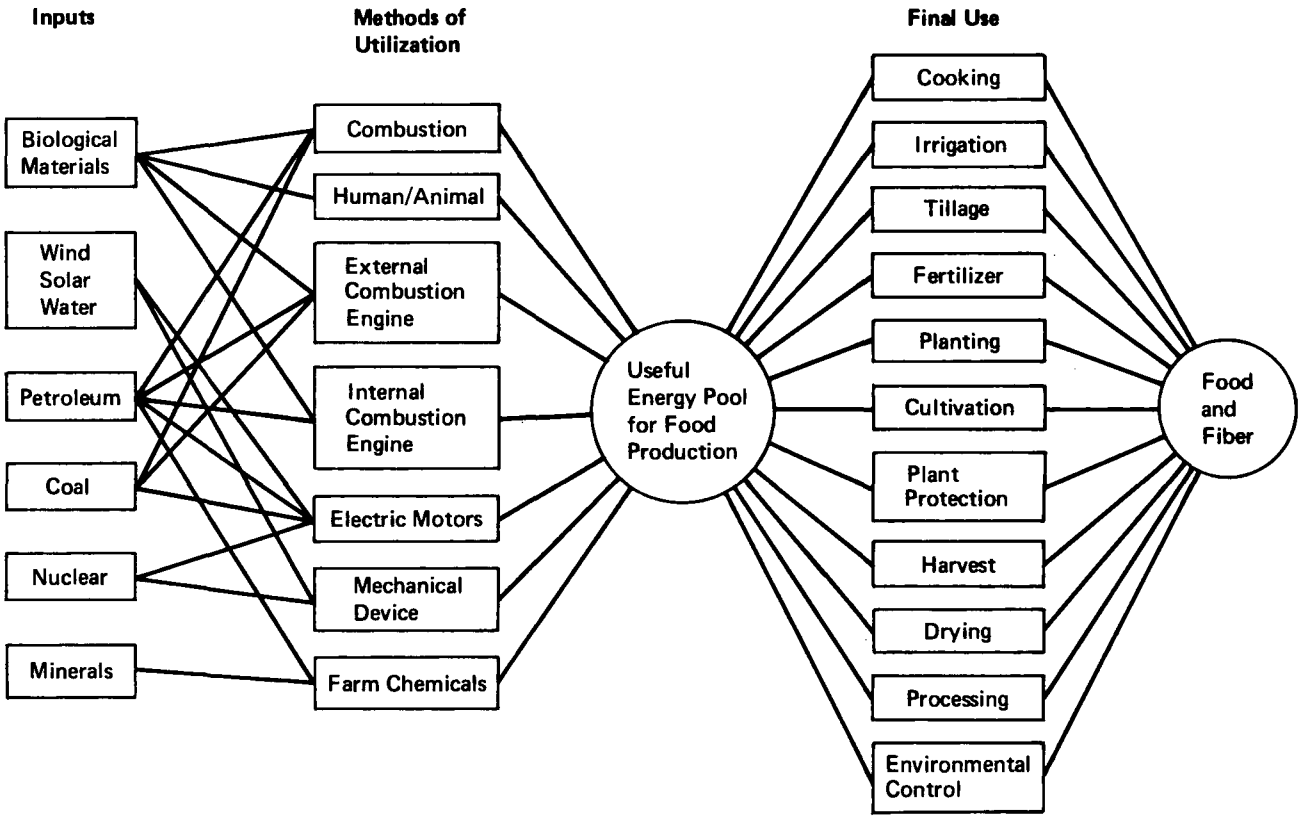


FIGURE 3 Energy flows in agriculture from alternative systems.

incompatible. The first is to improve the productivity of the farm worker so that the nation will have enough food and the farm worker will have a decent standard of living. Farm workers must produce enough food for the nonfarm families (one-third of the population) plus their own families. The situation is the worst in Nigeria, India, Indonesia, and Bangladesh, where each farm worker must produce enough food for 4 to 5.5 persons (FAO 1974c). Since at least .2 tons of cereal grains must be produced per capita for even marginal nutrition, a minimum productivity goal is an average of 1 ton of grain annually per farm worker.

The second need is to utilize displaced labor. This usually involves development and expansion of the industrial complex. The manufacture of simple farm equipment, food and feed processing, and the creation of export markets for labor-intensive finished goods must be followed by the development of heavier industry, especially where natural resources are available. Japan and Taiwan have followed this path successfully and the People's Republic of China appears to be developing in this way. Intensification of multiple cropping or intercropping is another way to utilize surplus labor.

Given the social, economic, energy, and technological constraints, the optimal approach to food production in the developing countries may be quite different from that in the Western world. The abundance of low-cost, unskilled labor and the scarcity of capital in the developing countries suggest that new approaches are needed to transform indigenous energy resources into useful work. Our research recommendations are aimed at evaluating human and animal capabilities for farm work, alternative farm mechanization systems, and alternative agricultural energy sources. Research must lead to solutions that make economic sense both to the government and to the individual farmer.

While technologically sound solutions to food production problems in the developing countries are of basic importance, the institutional policies that affect the adoption of these solutions by farmers also are critical. Decision making at the household level may be affected by cultural values that present barriers to the adoption of new technology. Determining these values and the effects of changing them to permit the utilization of new technologies should be investigated.

Public policies also have an important impact on incentives for agricultural production. Credit availability, minimum wages for agricultural workers, pricing of equipment and energy inputs, technical assistance, education, and stability of agricultural

prices are only a few of the areas in which government officials can affect production incentives. Thus institutional issues should be evaluated to assess their effects on the adoption of new technology. (Profile 2 of Subgroup A discusses research priorities for evaluating institutional issues.)

Profile 1

OPERATIONAL PARAMETERS FOR HUMANS AND ANIMALS

INTRODUCTION

Whether densely populated, as in the subcontinent area of India and Bangladesh, or sparsely populated, as in central Africa or South America, the tropics are for the most part short of food. In these areas an enormous amount of human and animal effort is involved in the production of food, but unfortunately neither humans nor animals are efficient "machines" in the tropical environment. Human and animal productivity are limited by high daily temperatures, generally high humidities, a high incidence of disease, and inadequate or imbalanced diets, all resulting in an apparent low level of motivation.

If it were possible, or even desirable, to fully mechanize agriculture directly in the developing countries, this study would not be needed. But the plethora of both people and animals in the food production system, the lack of petroleum fuels, and economics dictate a major role for human and animal labor for a number of years to come. Thus farm labor productivity in the tropics needs to be improved by means of suitable equipment and improved farming systems. Fundamental studies of human physiological responses are an important part of research and are needed as building blocks for several other research efforts proposed in this report.

Energy Efficiency of Humans and Animals

Estimated animal energy conversion efficiency is low for both bullocks and human beings (Makhijani 1975), generally below 5 percent. Suggs and Splinter (1961a, 1961b) demonstrated that this efficiency depends greatly and predictably on environmental conditions and on workload. The reason for this low efficiency in humans is that the human system is unable

to dissipate heat effectively through evaporation, radiation, or convection under tropical conditions. The laws of thermodynamics simply restrict the human being, or the animal, to low levels of conversion efficiency in a tropical climate. Neither human nor animal can convert food into enough mechanical energy to increase the amount of food produced above elemental requirements. Therefore, auxiliary energy sources may well have a greater impact on the ability of a human being to raise foodstuffs in tropical conditions than in the temperate regions.

RESEARCH NEEDS

Climate

Studies should be made in human and animal factors engineering to determine: the physiological responses of humans and animals to work conditions in the tropics, the limits to which acclimation can take place, and the energy cost of using humans or animals as sources of mechanical power in food production systems.

Knowledge of work output versus energy input would allow selection of those cases where it is more efficient to use biological resources such as wood, roughage, and feedstuffs through mechanical systems than through animal systems. Various studies of the comparative efficiency of men, animals, and machines have been made (Makhijani 1975, Morrison 1949, Splinter 1975, Kaul and Splinter 1964, and Hill and Erickson 1975). These studies have led to comparisons of the relative efficiency of humans, animals, and machines in performing specific tasks under specified conditions and of their interactions. Irrigation and fertilization, for example, tend to increase the energy input per hectare, but such inputs reduce the total energy requirements per ton of grain. Interpretation of such results must take into account the 24-hour-per-day, 365 days per year maintenance that is required in biological systems. Whereas a horse may have an instantaneous efficiency of 25 percent in converting food to work, its annual efficiency is closer to 2 percent. Ergonomic studies of the work response of humans and animals under various environmental stress conditions are needed to quantify work output versus energy input parameters. By understanding the operational parameters, we can estimate the amount of physical work that can be reasonably expected from humans and from work animals such as the bullock and water buffalo under various environmental conditions

and establish a more rational strategy toward mechanization in balance with labor-use intensity.

In addition to the ability to perform mechanical work, human beings have special attributes: discrimination and selectivity. Splinter and Suggs (1968) have developed a model of human ability to discriminate between, select, and load objects as a function of rate of handling and level of sorting. These studies show that human performance in a work situation can be characterized and generalized. Such ergonomic studies for various tasks in the production of foodstuffs in the tropics would greatly aid in determining mechanization priorities.

The social implications of mechanization are, of course, important. Social structure and social mores have a definite bearing on the acceptance or utility of mechanization. These factors, along with timeliness and economics, must be considered in such mechanization research.

Diseases and Parasites

The effects of diseases and parasites on work capacity are, in a quantifiable sense, virtually unknown. But the effects are obvious to anyone who has visited tropical areas where both humans and animals suffer from a wide range of health problems (Stout et al. 1973). The reduction in work output due to these endemic factors must be quantified to make rational evaluations of the physical energy available for raising food and the economic burden of ill health (see Study Team 2, Subgroup A report, especially on zoonotic diseases).

Diet

The effect of diet on the available energy output of horses is well known (Morrison 1949), but diet can have important effects on the work capacity of other animals as well. The response of animals to environment from the standpoint of weight gain in feedlots is now being clarified (Teter et al. 1973). If the environmental response of animals to workloads can be determined, models can be developed so that the energy input/output relationships for work animals over a wide range of environmental factors also can be determined. Such research is needed to make regional or global estimates of work capacity and to evaluate alternative options.

Motivation

A commonly held opinion in the industrialized nations is that the work population in most developing countries is poorly motivated. Such an opinion overlooks the obvious and rational adaptation of work habits to severe environmental stresses. Nevertheless, socioergonomic studies that relate motivation to specific kinds of mechanization, including air conditioning, would be helpful in developing mechanization strategies.

IMPACT

Fifty to sixty percent of the world's people invest their physical effort directly or with the aid of work animals to produce food (Makhijani 1975). Thus precise knowledge of the work processes of humans and of various work animals can have a direct effect on strategies for increasing productivity in food production.

A person performing physical work must eat enough to have energy above that required for general maintenance. To work at a rate of .075 kilowatts and an efficiency of 10 percent, for example, a human being must consume 5,100 calories per day. On a maintenance basis, these are enough calories for more than two people. Reducing the requirement for physical work would free up food not then required for producing human energy.

Quantification of the effects of disease and inadequate diet on work capacity and motivation could help guide programs of health and dietary assistance as well as mechanization (see Study Team 9).

Identification of the selectivity, discrimination, and physical effort requirements of tasks, coupled with the critical time required for completion, would greatly assist in the development of mechanization strategies. Those tasks that require primarily physical effort with little discrimination should be mechanized first; for those tasks where the human ability to discriminate is important, mechanization efforts should be delayed. Thus a task-by-task analysis of various cropping systems would allow a more rational approach to mechanization efforts (see Subgroup A).

This research would have short-term effects and would serve primarily as a foundation for the research on mechanization detailed in the following two profiles. U.S. manufacturers also could use this information because they already use human factors

engineering in the design and layout of tractors, combines, and other equipment.

IMPLEMENTATION

Work physiology studies at the E. I. du Pont Haskell Laboratory and the U.S. Army Laboratories have set the limits of exposure of human beings to various environments. Studies at North Carolina State University have developed response surfaces of work output, heart rate, oxygen consumption, and respiration rate over a range of temperatures and humidities. Limited measurements of the work output of animals have been conducted in India and the United States (Missouri and Iowa). However, there is insufficient knowledge concerning diet, disease, acclimation of breeds, or even the general effect of environmental stress on the work output of animals or people.

A comprehensive study will require climate-controlled chambers with treadmill dynamometers and instrumentation for measuring heart rate, oxygen consumption, respiration rate, and body temperature in order to develop performance response surfaces for the various work animals and humans under both tropical and temperate work conditions. Environmentally controlled conditioning chambers also are needed to determine the limits of acclimation as environments change (e.g., from hot-dry to hot-humid). Diagnostic facilities are needed to monitor and evaluate the effects of diseases or parasites on work performance.

A team of agricultural engineers, human and animal physiologists, medical doctors, and veterinarians, together with a support staff of electronic and laboratory technicians, would be required. Because of the level of expertise and degree of instrumentation and environmental controls necessary, it is suggested that research centers in the United States be used for this study. Some facilities already exist, such as the University of Missouri Animal Physiology Lab, the USDA Meat Animal Research Center [in Nebraska], the Human Factors Lab at North Carolina State University, the Bio-Environmental Lab at Kansas State University, and industrial laboratories such as du Pont's Haskell Laboratories.

Once fundamental relationships are known and techniques are developed, adaptive research studies would be needed using local animals and local residents. Indigenous scientists (probably trained at the U.S. facilities mentioned) could carry out most of these studies.

Since the research would build on existing techniques, a high probability of success is expected.

For human factors studies, the currently unused facilities at North Carolina State University could be put back into operation, and the qualified staff on hand used to train foreign scientists. At least one research team each should be trained for the humid tropics, the high mountains, and the desert. These teams would then determine the parameters of work with native populations in the developing countries. A budget of $300,000 to $400,000 per year for four years would be needed. The fifth year, funds would be needed to organize an international conference and to disseminate the information.

For studies of animal physiology, only limited facilities exist (at the University of Missouri, for example). Annual funding of $400,000 to $500,000 would be required for four years, again culminating in a conference for dissemination of results the fifth year.

Profile 2

OPTIMAL ALTERNATIVE AGRICULTURAL POWER SYSTEMS

There are many trade-offs to be considered in determining the best form of farm mechanization for a developing nation or even a small portion of one. The key decision makers need better information than is currently available on this subject. These key decision makers include governments, farm equipment manufacturers, farm equipment dealers, lending institutions, and farmers.

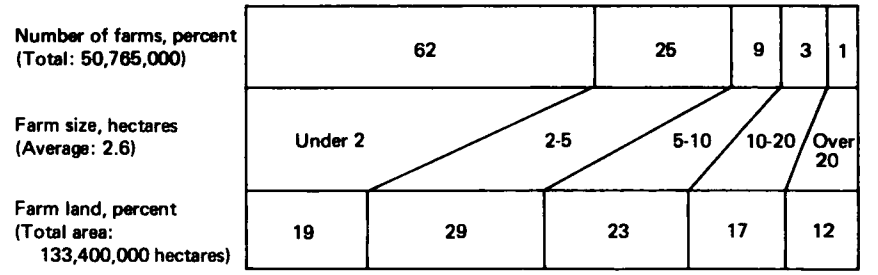
RESEARCH NEEDS

The major research need is to determine, for each important and comparatively homogeneous area, the agricultural power systems that offer the best benefit-cost ratios. This research should focus on the transfer of the most appropriate technology, considering farm income, available energy, and rural employment. The study would be short term in nature, dealing more with technology transfer than with development of new mechanisms.

In order to make a significant contribution toward alleviating the world food problem, research should concentrate on middle-sized farms. Farm sizes in some of the Latin American nations, such as Argentina, are larger than in the United States. However, most of the food-short nations in Africa and Asia are characterized by small farms (International Association of Agricultural Economists 1969). The farm size distribution in India, which is typical for nations on these two continents, is used for illustration (Figure 4). Farms of 10 hectares or more account for 29 percent of the farmland in India and for 4 percent of the farms. The 25 to 30 kilowatt tractors currently produced in that country are only economical for individual use on farms over 10 hectares. Thus the major challenge is to develop power and machinery

systems for the 96 percent of the farms of less than 10 hectares.

Farmers with less than 2 hectares will probably continue to use traditional methods of farm production, although they can expect to borrow some equipment from neighbors with larger farms. Fortunately, these small farmers can adopt new technologies, such as improved seeds, fertilizer, and plant protection chemicals, since these items are divisible into quantities suitable for the smallest farm plot. The 34 percent of the farms between 2 and 10 hectares should be the primary candidates for elementary mechanization if appropriate equipment can be developed and introduced. Several institutions have developed a number of tillage, planting, and harvesting machines especially matched to the needs of the small farmer (IRRI 1975). Since there is a wide variety of human- and animal-powered machines available, as shown in Table 8, major stress should be placed first on adapting existing machines to local cropping practices and soils. It should be recognized that capital investment for mechanization increases progressively as the degree of mechanization increases from hand-powered tools to self-propelled combines. New equipment is particularly needed for intercropping, minimum tillage,



SOURCE: Adapted from Ford Foundation (1972).

FIGURE 4 Distribution of farms and land in India, 1962.

Table 8. Alternate agricultural power systems

	Human	Animal	Electric motors	Internal combustion engines	4-8 kw walking tractors	10-15 kw riding tractors	20-30 kw riding tractors	S-P combines
Mobile operations								
Tillage	X	X			X	X	X	
Planting	X	X			X	X	X	
Cultivation and plant protection	X	X		X	X	X	X	
Harvesting	X	X			X	X	X	X
Transport	X	X			X	X	X	
Stationary operations								
Lifting water for irrigation		X	X	X	X			
Threshing	X	X	X	X	X	X	X	
Drying	X		X	X				
Processing	X	X	X	X	X			

nonconventional power systems, and the harvesting of specialized crops, such as yucca or manioc. These are long-term projects requiring extended funding and effort.

Farm equipment for some operations, such as broadcasting fertilizer, comes in a variety of capacities with generally proportional prices. U.S. farmers employ equipment using the same principle of a spinning disk to operate everything from a worker-carried Cyclone spreader holding 10 kg to a three-point hitch spreader holding 500 kg to a tractor spreader holding 5,000 kg. Other forms of mechanization, however, such as tractors, have not yet been scaled down successfully with retention of performance and at a cost proportional to capacity.

Farm Income

The benefit of these research projects can be measured in net farm income. Improvements in income generally will be achieved through higher food production per hectare, but also may result from switching to a crop with a higher cash value. The first information needed to determine net farm income is production cost. There is a need, especially in the developing nations, to pay particular attention to return on capital investment. The study of production costs must be broad enough to include the nation as a whole as well as individual farmers.

Energy

Petroleum resources are finite in supply, and their costs have been a major financial burden for the developing nations for the past three years. Research studies should be conducted to determine how to use energy inputs efficiently while increasing food outputs. Humans and animals consume renewable energy resources, and a research-demonstration project must account for the amount of land required to support these two sources of energy. The object of the research would be to increase the marketable surplus of food from each farm.

Kaul and Kumar (1975) have shown that there is a great difference in the efficiency of various wheat threshers used in India. Similar studies need to be made on all agricultural implements to determine where purchased energy can be reduced without reducing food production.

Rural Employment

Human labor is involved in all agricultural power systems. While the human being is relatively efficient when dexterity and selectivity are needed, he or she is relatively inefficient for tillage, lifting irrigation water, or threshing. Research should be conducted to determine which operations or portions of operations should continue to be done by humans and which would profit from a higher level of mechanization.

Research on rural employment as it relates to mechanization must consider many factors. Increased mechanization may increase or decrease employment in farming although it generally increases employment in agricultural support industries.

Abercrombie (1975) and Stout and Downing (1975) point out that the health of the agricultural sector depends on both its production of food and its provision of productive employment. Harrington (1975) notes that while employment as well as food production increased during rapid tractorization in the Punjab region of India, the same variables do not exist in other countries.

Farm wages are not only a function of inherent labor productivity but also of capital investment in land utilized in a complementary fashion with labor. While total farm wages may be the best single indicator of the economic position of hired farm workers, the proposed research should include other components. These include number of farm workers, days of work per year, and daily farm wages. The relationship between farm wages and industrial wages before and after farm mechanization in a particular area is an important indicator of the relative demand for farm workers.

IMPACT

While the economic impacts of some forms of farm mechanization are known, one purpose of the proposed study would be to make a better determination of these impacts, thus leading to those forms of mechanization that will effect the greatest increase in food production.

Pumpsets powered by electric motors or diesel engines have made a major contribution to world food supplies. These pumpsets are so popular in India that farmers have spent more to mechanize the lifting of water for irrigation than for all other forms of farm mechanization combined. By 1974, over $1 billion had been spent for over 4 million pumpsets (Anonymous 1975). About half of these are powered by 2 to 4 kw electric motors and the other half by 3 to 6 kw diesel

engines. Pumpsets are now responsible for about 8 million tons, or 8 percent, of India's food grain production through increased yields. These pumpsets also are responsible for an additional 4 million tons, or 4 percent, of India's food grain production since they permit a second crop to be grown each year. The additional food produced by the use of pumpsets in India exceeds average food imports for the past 10 years. Furthermore, the amount of land irrigated by pumpsets can be at least doubled in India. The proposed research would help spread the use of the pumpset to other developing countries.

Multiple cropping also can be increased with farm mechanization. The sun is available year-round for growing crops in most developing nations, and irrigation can assure adequate water during periods without rain. But a higher level of farm mechanization is required to get one crop harvested quickly and a second crop planted since harvesting, tillage, and planting are all labor-intensive (see Profile 1, Subgroup A). Artificial drying, especially of rice, is another form of farm mechanization required for multiple cropping if one of the crops is harvested during the monsoon season.

Good small rice dryers are available from Japan. Some storage problems may be encountered when both temperatures and humidities are high, but suitable storage equipment is available in some parts of the world.

Crop yields also can be increased through improved tillage and planting. Adequate farm power permits the farmer to provide deeper, better prepared seed beds. In some instances, it permits the tilling of heavy baked soils before the rains come. Fertilizer grain drills have increased wheat yields 10 to 15 percent over traditional hand seeding. For India alone, this represents a potential increase of 3 million tons of wheat. Limited tests have shown 40 percent increases in maize yield with the use of rowcrop planters. Both machines increase yields through more uniform stands and better fertilizer placement (Giles 1975). Mixed cropping or intercropping means an overlap of growing periods, with associated and as yet unresolved problems for mechanization.

Food production also can be increased through mechanization by reducing losses (IRRI 1974a). Properly adjusted threshers and combines have smaller losses during the threshing process than traditional bullock trampling. Mechanical threshing also is more timely and has resulted in wheat arriving at market three weeks earlier than previously in North India. Further adoption of threshers and combines will reduce grain losses from rain, fire, rats, birds, cattle, and

pilferage. Because of their size, the combines used in Europe and North America generally are not well adapted to conditions in the developing nations. They are usually too large for the fields, and they are not equipped to save the straw, often important in the developing nations. Flooded rice also results in serious traction and flotation problems. That harvest mechanization is not always the answer is illustrated by an experience in Colombia. Lloyd Johnson (1976, personal communication, CIAT, Palmiro, Colombia) has developed a system for year-round production of rice in Colombia using intensive labor; the use of combines was found not to be cost-effective.

Petroleum is used to make fertilizers and plant protection chemicals as well as for providing fuel for pumpsets and tractors. Improved agricultural power systems are most effective when used in conjunction with other improved technologies. Research would help establish which combinations result in sufficiently increased food production to justify larger purchases of energy (Kastens 1975).

If appropriate agricultural power systems are to be adopted on the majority of farms in the developing countries, the research findings must be made available to decision makers. National governments play a pivotal role in the adoption of farm mechanization since they often determine economic margins and risks for both the supplier and the farmer. Profile 2 of Subgroup A provides a more thorough examination of the relationship between government institutions and the farm equipment industry.

More consistent government policies and actions would result in a more rational division of the farm equipment market between local manufacturers and importers. The size of the market within a country has a major influence on the economic and social trade-offs of importation, local manufacture, or importing components and assembling the more complex equipment. Sound government actions would benefit both local and foreign manufacturers, thus resulting in lower equipment costs to farmers.

Unless a system of credit exists, most farmers in the developing countries will not be able to move toward mechanization. Credit institutions need to know which types of mechanized equipment offer the highest net benefit given the variety of crops and farm sizes in their particular areas. If mechanization makes considerable economic sense for the farmer, the institution can extend credit more liberally without taking undue risks. Thus more money would be available to borrowers.

Farmers in the developing nations generally are unable to assume high risk investments. An

agricultural extension service would be able to provide information and vocational training to match a farmer's crops and size of operation with a range of appropriate agricultural power systems. With this information the farmer would be able to determine which kind of mechanization would result in the greatest increase in net farm income and whether it would be more advantageous to own the equipment individually or share in the cost with others. Cooperative and custom hiring have proven effective in many areas.

IMPLEMENTATION

The research required to determine optimum agricultural power systems requires a multidisciplinary approach in all cases. Each facet of the research requires the participation of at least an agricultural economist, agronomist, and an agricultural engineer. Most of the work must be done at sites representative of the major homogeneous areas in the developing nations.

Two or possibly three farming systems should coexist at specified research locations so that farmers can observe the differences between them. One section of the research station would be devoted to demonstrating the current dominant cropping system of the area, using the inputs and management practices of the above-average farmer. A second section would demonstrate the practices and machines recommended by the research station. A third section would demonstrate newer machines and innovations, such as unfamiliar crops with higher cash returns, minimum tillage, intercropping, and relay cropping.

At each research location the control base should be the level of mechanization used by the majority of farmers with medium-sized farms. Some preliminary evaluation will be required to assure that the equipment matches local crops and soils. While several levels of investment in farm mechanization may be possible, most research projects should limit the choices to three or four. For each level of mechanization, work capacity per cropping season should be determined. In some instances, high work capacity in combination with high unit cost will suggest that that type of equipment is primarily for multifarm use. In general, a farmer's purchase of farm equipment is probably limited to an amount no greater than the farm's annual cash receipts. This places a ceiling on the size of equipment the farmer can afford while the need to perform farm operations on a timely basis places a lower limit on the work capacity required of the equipment to be effective. The engineering

challenge is to design equipment within these limits that will serve to get the farmer out of the vicious circle within which he or she now operates. Studies will have to be made of food production from comparable farms that utilize different levels of mechanization. Beyond this, the team should determine the level of food production that can be achieved with optimum farm management and improved inputs at various levels of mechanization. Large changes should be explored, including the introduction of irrigation, mechanized tillage, planting, harvesting, the growing of an additional crop each year, and new farming systems, such as higher cash value crops, intercropping, and relay cropping. The latter two may require the creation of new planting and harvesting equipment.

The economic portion of the research should be supervised by a farm management specialist with support from an engineer in the selection of farm equipment to study. Farm records should be kept for a few years on similar-sized farms with different levels of mechanization. The engineer can set up demonstrations of other farm power sources and equipment not commonly used in the area but available on the world market. Local evaluation of imported equipment is generally necessary, since users must become familiar with it before an attempt is made at a benefit-cost analysis.

Concentrated study of the end use of energy is a relatively new science. This type of study will require at least an economist and an engineer and possibly persons from other disciplines with specific expertise in energy. Current uses of energy that originates on the farm and that is purchased by the farmer must be studied over a range of farm sizes for farms with varying levels of mechanization. Resources used to produce food for either humans or animals should be included (FAO 1976).

The relationship between rural employment and agricultural power systems is one of the most difficult and challenging portions of the research required. This research must be done in the developing nations, with inputs from an economist, an engineer, a sociologist, and/or a labor specialist.

A variety of studies is needed for different geographic areas. Statewide studies have the advantage of integrating the many unknown economic and social variables that influence rural employment. These are meaningful in states like the Punjab, India, which have good statistics and are rather homogeneous. In other instances, much smaller areas must be chosen to get valid results. The studies also should include areas like Thailand, where custom operation is widely accepted.

An initial impact can be achieved by setting up demonstration teams utilizing a wide spectrum of the plows, tractors, planters, harvesting equipment, threshers, etc., which have been developed around the world. For crops, soils, or practices for which machinery has not been developed, efforts to devise appropriate equipment must be pursued.

The cost of each two- or three-person team would be $100,000 per year. In view of the large number of developing countries, a minimum of 12 teams would be required to have any impact, and twice that number would be more appropriate. A minimum of five years would be required to develop sustainable programs. Biannual coordinating conferences should be conducted to allow cross-fertilization of ideas.

Profile 3

NONPETROLEUM ENERGY SOURCES

The farming systems of the advanced countries have attained a high level of productivity by depending on fossil fuels to manufacture inputs and power farm machinery (Heichel 1976). Recent interruptions in the world energy supply have made us keenly aware of the vulnerability of these farming systems to energy shortfalls or price rationing.

The developing countries, whose rising populations and increasingly sophisticated diets (Brown and Pariser 1975) could potentially increase world petroleum energy requirements of the entire food chain--production, processing, home preparation--by about elevenfold by the year 2000 (Kastens 1975), face a particular burden in assuring their food needs in a future of scarce energy. One approach to lessening the vulnerability of the developing countries to energy shortfalls is to develop more frugal power systems than those now available. Mindful that most developing countries have meager proven reserves of petroleum and other fossil fuels (Hubbert 1969) and inadequate foreign exchange to compete for the remaining finite reserves (Table 9) on world markets, we propose the development and utilization of nonpetroleum sources of farm power. Nonpetroleum energy includes the renewable carbohydrates produced by photosynthesis and such virtually inexhaustible natural resources as sunlight, wind, and falling water. Some technologies for using nonpetroleum energy sources are more practical and immediately applied than others, and we now list, in descending order of importance, the technologies most relevant to the needs of the developing nations.

Table 9. Recoverable reserves[a] and anticipated lifetime of world fossil energy sources and uranium

Source	Quantity	Lifetime[d]
Natural gas	200×10^{12} m^3[b]	15 years
Crude oil	275×10^{9} m^3[b]	35 years
Shale oil	54.4×10^{12} m^3[b]	165 years (U.S. only)
Coal	10×10^{12} tons[b]	300-500 years
Uranium (U_3O_8)	$1,430 \times 10^{3}$ tons[c]	35 years (fission only)

[a]Proved reserves plus undiscovered resources.
[b]NRC (1975b).
[c]Hubbert (1969).
[d]Extrapolations of current and expected rates of use at current relative prices. For a discussion of the constraints on these extrapolations, see Gardner (1975).

RESEARCH NEEDS

External Combustion Engines

The reciprocating steam engine or steam turbine has potential for stationary applications, such as threshing or grinding of grain, oilseed extraction, pumping irrigation water, or providing a central source of electric power for a village. Coal, crop residues, wood, or other replenishable carbohydrates could be the principal fuel sources for these external combustion engines. To utilize these materials, however, it is necessary first to determine the prospects for using wood and other plant materials as fuel sources for these engines. This involves an assessment of the economically recoverable fuel materials and of the quantities of wood or other plant materials that could be regularly harvested without fear of erosion or degradation of soil fertility (Eckholm 1975). Keener, et al. (1975) estimate the energy output/input ratio for tree plantations to be 27/1, for corn or alfalfa forage 10/1, for corn grain 7/1, and for wheat grain 4/1.

Second, a reevaluation must be made of existing steam engine technology (the Rankine cycle) for both piston and turbine engines to determine which sizes are

appropriate in developing countries and the efficiency of various organic fuels, both solid and liquid. We must assess whether scaled down turbines are practical for developing countries and, if so, whether they would be more appropriately manufactured in the United States or in the developing countries.

Finally, the relative price at which the technology of external combustion engines becomes competitive with that of internal combustion engines must be determined.

Solar Energy

A traditional and widespread use of solar energy is to dry such agricultural products as grain, forage, fruits, oilseeds, fish, and meat. The customary technique is to spread the materials to be dried in a thin layer on the ground and expose them to solar radiation and wind. Evaporation, water heating, distillation, and cooking are other potential applications of solar energy to local energy economics (NRC 1972b). To facilitate the exploitation of solar energy, we recommend research to:

-- Assess the potential application of solar energy for crop drying, water heating, and cooking in the developing countries.
-- Determine the extent to which solar drying will speed crop harvests, diminish crop losses to vermin and weather, permit harvesting of crops during rainy weather, and enhance storage of agricultural products.
-- Determine the most efficient methods of collecting and using solar energy for specific applications. For example, many developing countries have a plastics industry. Plastic film sunlight collectors currently being tested for grain drying in the U.S. Midwest may well be useful in the developing countries. One major application may be the drying of organic fuels, such as crop residues, for external combustion engines. Hot water for household use could be collected by simple means, but a practical solar energy cooker has yet to be developed.

Water

South Asia, South America, and Africa have large unexploited water energy resources (Hubbert 1969). Water power could be used for the threshing or grinding of grain, to press oil from seeds, or to provide central electrical power for a village, among other

things. Small waterwheel technology is centuries old, and the technology for large-scale utilization already exists. Specifically, we need to determine (1) the daily and seasonal streamflow characteristics of rivers and small streams, and (2) the prospects for updating waterwheel technology to provide the power required for specific applications.

Wind

Wind power might provide the energy for stationary agricultural applications such as the grinding and threshing of grain and or the pumping of water for domestic and livestock consumption. The technology of low blade-tip velocity turbines (the common windmill) and high blade-tip velocity turbines (windchargers) is well developed. In order to apply this technology in the developing countries, research should be conducted to (1) determine, through a network of climatological stations, the areas where wind speed and duration are above the minimum needed for practical application of windmills, and (2) adapt existing wind power technology utilizing indigenous materials for specific uses with low horsepower requirements.

Methane

Human and animal wastes and crop residues can be utilized to generate methane gas for household or village use in cooking, lighting, stationary engines, and refrigeration. Although the prospects for methane generation seem attractive (Makhijani 1975), the efficiency of the process is sensitive to changes in the temperature of the digester and to such chemical conditions as ammonia concentration and the carbon-nitrogen ratio of the feedstock materials. According to Smith (1976), large digesters require continuous stirring to prevent obstructions from occurring and, therefore, have small power output if part of the methane generated is used to power the stirring. They also require a high level of management. Furthermore, conflicting appraisals of the efficacy of small methane generators have come from the examination of systems being used in the developing countries. To resolve the conflicting views on this technology, it is necessary to (1) survey the operational status of small household methane generators in the developing countries and the power requirements they serve, and (2) compare small- and large-scale digester methodology and evaluate the economic, physical, chemical, environmental, and

feedstock constraints on each system, following studies by Hamilton Standards, Inc. and others.

Methanol, Ethanol, Hydrogenation, and Pyrolysis

Successful exploitation of these technologies might yield a substitute for the petroleum fuels needed by mobile sources of power. All of these technologies are highly sophisticated and relatively inefficient in net energy yield, and are at present inapplicable to the immediate needs of the developing nations.

Nuclear Energy

Because of the relatively brief expected lifetime of nuclear fuels (Table 9), and the expense, sophistication, and safety problems of the technology, nuclear sources of power seem less applicable than the foregoing technologies to the immediate needs of the developing nations.

IMPACT

The development and utilization of nonpetroleum energy sources in the agricultural sectors of the developing countries would insulate their food production systems from interruptions in the supply of scarce and expensive fossil fuels. In view of the finite reserves of petroleum and natural gas, an operational technology for use of alternate fuels may well arise in the developing countries first and then move, as it must at some future time, to the high-income countries.

External combustion engines and wind power are logical energy sources for assuring a sanitary water supply from a well or for introducing grinding, threshing, and oilseed extraction technologies in localities where they are now precluded by absence of a suitable energy source. External combustion is particularly attractive where feedstocks, e.g. rice hulls, have a negative value. There is also some potential for driving a heat engine using heat directly from solar collectors or by heating a fluid medium, such as freon.

Successful utilization of solar energy would reduce dependence on wood and dung as cooking fuels in the developing countries. Wood might be reallocated to external combustion engines, where it could be burned more efficiently than in open fires, while dung could be utilized as fertilizer or feedstock for methane

generation. Improved methods of solar drying might facilitate multiple cropping by speeding crop harvest and, additionally, diminish crop losses and assure successful crop storage.

The potential energy of water might be used to provide stationary power for grinding, threshing, and other individual or village uses now precluded by lack of a suitable energy source. Electricity generated by water or wind power would have multiple applications in food production.

Methane generation would produce a relatively clean fuel for household cooking and other open combustion processes, or possibly for use in stationary internal combustion engines. Liquid fuels for mobile uses would result from the development of alcohol, hydrogenation, and pyrolysis technologies.

IMPLEMENTATION

Answers to questions involving external combustion, solar energy, water power, and wind power must be sought in the developing nations where the knowledge will be used. Engineers must determine the applicability of existing steam engine and turbine technology to specific uses in these countries. Timeliness and monetary impact must be determined, and strategies to adapt existing knowledge to new uses must be conceived. Foresters, agronomists, and economists must learn how much renewable fuel is available in areas where external combustion engines are proposed and how it competes in price with coal and petroleum. Engineers must learn how various fuels can be burned efficiently to generate steam. This research would be short term since most of the technology has already been developed and only the problems of application must be overcome.

Research into solar energy should apply current knowledge about drying, heating, and cooking with solar reflectors to specific needs in the developing countries. Adaptive research to answer unforeseen questions on the collection and storage of solar energy must be initiated as needed.

Engineers and hydrologists must determine the rate and reliability of river and small stream flows in the developing countries. Engineers must determine power requirements of specific applications and attempt to adapt existing water power technology to these new uses. Economists and engineers should work jointly to identify optimum locations for water-powered energy plants. Government action on water rights also will be required.

To utilize wind power, a climatology group should be formed to organize data on wind speed and duration as a prelude to choosing logical sites for the installation of facilities. Present windmill technology could be applied.

Investigations into the technology of methane generation by anaerobic digestion should proceed in the developing countries, and the extent of use and efficacy of methane generation should be documented. Small (household) and large (farm and village) digesters should be tested in laboratories in the high-income nations to determine the limitations of each type.

When petroleum fuels become economically unattractive, mobile power applications will require alcohol, hydrogenation, pyrolysis, or coal-based fuel. Because the returns would be small, research on alcohol, hydrogenation, and pyrolysis is likely to be confined to the high-income countries until the processes attain commercial application. Although the high-income countries have the resources to do the research, questions about the applicability of alcohol, hydrogenation, and pyrolysis might be answered fruitfully through joint investigations by public and private institutions in both the high-income and the developing countries. This would be long-term research, and practical use of the results would ultimately depend on economics and the availability of petroleum fuels.

Funding

Private industry could conduct the major investigative effort into external combustion engine development, both in the United States and, to some extent, in the developing countries. To determine the feasibility of such engines, government contracts would be necessary. An investment of $1 million per year for design, development, and testing would be required for a minimum of two years.

Solar energy research is now being set up in the United States and throughout the world. Some additional funding may be needed.

Major water power plants are expensive and must be supported by major financial organization. Waterwheels, however, could be constructed locally at a low cost.

Wind energy conversion is being investigated quite thoroughly, but additional funding may be required. Investigation of the use of methane from biological sources should be supported at a modest level, perhaps

$100,000 per year, with additional funding later fcr the installation of demonstration units.

NOTES

1. Lee Martin emphasizes the illustrative nature of the data in the tables rather than their detailed accuracy. The tables illustrate the kinds of information needed for phase one studies.
2. This may be 33 million hectares too large because of a possible error (acres instead of hectares) about data of irrigated land in the People's Republic of China (Peterson 1974).
3. This technique has been called systems analysis, management science, operations research, and cybernetics. All these terms have related meanings and the National Academy of Sciences has combined these to SA/OR.

REFERENCES

Abercrombie, K. C. (1975) Agricultural Mechanization and Employment in Developing Countries. Monthly Bulletin of Agricultural Economics and Statistics. Vol. 24, No. 5. (Published by the Food and Agriculture Organization of the U.N., Rome).

Ahmed, J., C. H. M. van Bavel, and E. A. Hiler (1976) Optimization of crop irrigation strategy under a stochastic weather regime: a simulation study. Water Resources Research 12(6):1241-1247.

American Academy of Arts and Sciences and Hebrew University (1975) Symposium on Arid Land Development--Potentialities and Problems. Boston, Mass.

American Institute of Chemical Engineers (1974-75) Coal Processing Technology. Vols. I (1974) and II (1975). New York, N.Y.

Andrew, C. S., A. D. Johnson, and R. L. Sandland (1973) Effect of aluminum on the growth and chemical composition of some tropical and temperate pasture legumes. Australian Journal of Agricultural Research 24:325-339.

Andrews, D. J. and A. H. Kassam (1976) The importance of multiple cropping in world food supplies. In: Papendick, et al.

Anonymous (1975a) Brief on Indian Agriculture. New Delhi, India: Office of the Agricultural Attache, American Embassy.

Anonymous (1975b) O decreto do Polocentro. Jornal do Brasil. January 30, page 26.

Asian Development Bank (1969) Asian Agricultural Survey. Seattle: University of Washington Press.

Bennett, H. H. and W. R. Chapline (1928) Soil Erosion a National Menace. Circular No. 33, U.S. Department of Agriculture.

Blaut, J. M. (1959) Microgeographic sampling; a quantitative approach to regional agricultural geography. Economic Geography 35:79-88.

Bradfield, R. (1972) Maximizing food production through multiple cropping systems centered on rice. Rice, Science and Man. Los Banos, Philippines: International Rice Research Institute.

Brinkman, R. and A. J. Smyth, eds. (1973) Land Evaluation for Rural Purposes: Summary of an Expert Consultation, October 6-12, 1972, Wageningen, The Netherlands. Publication No. 17, International Institution for Land Reclamation and Improvement.

Brown, A. W. A., T. C. Byerly, M. Gibbs, and A. San Pietro, eds. (1975) Crop Productivity--Research Imperatives. East Lansing: Michigan Agricultural Experiment Station and Charles F. Kettering Foundation.

Brown, N. L. and E. R. Pariser (1975) Food science in developing countries. Science 188:589-593.

Bureau of National Affairs (1976) Energy Users Report. Washington, D.C.

Buringh, P., H. D. J. van Heemst, and G. J. Staring (1975) Computation of the Absolute Maximum Food Production of the World. Department of Tropical Soil Science, Agricultural University, Wageningen, The Netherlands.

Charreau, C. (1974) Soils of Tropical Dry and Dry-wet Climatic Areas of West Africa and their Use and Management. Agronomy Mimeo 74-26, Department of Agronomy, Cornell University, Ithaca, N.Y.

Cornell University (1974-75) Soil Fertility in the Humid Tropics. Progress Report. Ithaca, N.Y.

Couston, J. T. (1976) Current Situation and Longer-Term Outlook. Third Session, Commission on Fertilizers, Food and Agriculture Organization of the U.N., June 8-11.

Dalal, R. C. (1974) Effects of intercropping maize with pigeon peas on grain yields and nutrient uptake. Experimental Agriculture 10:219-224.

DaSilva, A. R. (1976) Melhoramento genetico para resistencia a toxidez de aluminio e manganes no Brasil: antecedentes, necesidades e posibilidades. Ciencia e Cultura 28:147-149.

DeWit, C. T. (1968) Food production; past, present, and future. Der Stickstoff (Duesseldorf, Germany) 15:68-80.

Doorenbos, J. (1975) The role of irrigation in food production. Agriculture and Environment 2:23-54.

Duckham, A. M. and G. B. Masefield (1970) Farming Systems of the World. London: Chatto and Windus.

Dudal, R. (1965) Dark clay soils of tropical and subtropical regions. Agriculture Development Paper 83, Food and Agriculture Organization of the U.N., Rome.

Eckert, J., N. Dimick, and W. Clyma (1975) Water Management Alternatives for Pakistan: A Tentative Appraisal. Water Management Technical Report No. 43, Colorado State University.

Eckholm, E. P. (1975) The Other Energy Crisis: Firewood. Paper No. 1. Worldwatch Institute, Washington, D.C.

Eckholm, E. P. (1976) Losing Ground: Environmental Stress and World Food Prospects. New York: Norton.

Emigh, G. D. (1972) World phosphate reserves--are there really enough. Engineering and Minerals Journal 173(4):90-95.

Emigh, G. D. (1975) Phosphate Rock. Industrial Minerals and Rocks. New York: American Institute of Mining Metallurgical and Petroleum Engineers.

Engelstad, O. P., J. Getsinger, and P. Stangel (1972) Tailoring of Fertilizers for Rice. TVA Bulletin Y-52. Tennessee Valley Authority, Muscle Shoals, Ala.

Evans, H. J., ed. (1975) Enhancing Biological Nitrogen Fixation. Proceedings of a workshop, June 6, 1974. Division of Biological and Medical Sciences, National Science Foundation, Washington, D.C.

Fiering, M. B. (1967) Streamflow Synthesis. Cambridge, Mass.: Harvard University Press.

Food and Agriculture Organization of the U.N. (1974a) FAO Proposals for Action, World Food Conference. Rome: FAO.

Food and Agriculture Organization of the U.N. (1974b) Production Yearbook, 1973. Rome: FAO.

Food and Agriculture Organization of the U.N. (1975) Population, Food Supply, and Agricultural Development. Rome: FAO.

Food and Agriculture Organization of the U.N. (1976) Energy for Agriculture in Developing Countries. Monthly Bulletin of Agricultural Economics and Statistics, Vol. 25, No. 2.

Ford Foundation (1972) Agricultural Mechanization in India. New Delhi: Ford Foundation.

Foster, T. H., O. W. Livingston, and P. J. Stangel (1976) The Potential for Regional Cooperation in Fertilizer, A Methodology Study. Bulletin T-2. International Fertilizer Development Center, Florence, Ala.

Foy, C. D. (1974) Effects of aluminum on plant growth. Pages 601-642, The Plant Root and Its Environment, edited by E. W. Carson. Charlottesville: University of Virginia Press.

Framji, K. K. and I. K. Mahajan (1969) Irrigation and Drainage in the World: A Global View (2nd edition). New Delhi: International Commission on Irrigation and Drainage.

Francis, C. A., C. A. Flor, and S. R. Temple (1976a) Adapting varieties for intercropped systems in the tropics. In: Papendick, et al.
Francis, E. J., G. L. Dugger, W. H. Avery, H. L. Olsen, and W. B. Shippen (1976b) An Analysis of the Ocean Thermal Plant-Ship to Produce Anhydrous Ammonia. Paper presented at American Chemical Society meeting, April 1976, New York.
Fujishima, A. and K. Honda (1972) Electrochemical photolysis of water at a semiconductor electrode. Nature 238:37-38.
Gardner, B. D. (1975) Energy Models and Market Prices. Paper presented at the annual meeting, Operations Research Society of America, May 2, Chicago, Ill.
Giles, G. W. (1975) The reorientation of agricultural mechanization for the developing countries. Agricultural Mechanization in Asia (Tokyo) 6(2):1-16.
Goetze, N. (1973) Research for Rainfed Agriculture in Research Needs for On-farm Water Management. Proceedings of an International Symposium, Park City, Utah, U.S. Agency for International Development.
Graff, G. (1976) A "hydrogen economy" by the year 2000? Chemical Week 118(23):53.
Gray, C. (1975) Role and function of international institutes and how they are created. Proceedings of Soil and Water Management Workshop, U.S. Agency for International Development.
Griliches, Z. (1958) The demand for fertilizers: an economic interpretation. Journal of Farm Economics 40:591-606.
Hardy, R. W. F., P. Filner, and R. H. Hageman (1975) Nitrogen input. Pages 133-176, Brown, et al.
Harrington, R. E. (1975) Agricultural engineering and productivity. Agricultural Mechanization in Asia (Tokyo) 5:41-48.
Harwood, R. R. (1976) Application of science and technology to long range solutions: multiple cropping potentials. Pages 323-440, Proceedings, Nutrition and Agricultural and Economic Development in the Tropics, edited by N. S. Scrimshaw and M. Behar. New York: Plenum Press.
Harwood, R. R. and E. C. Price (1976) Multiple cropping in tropical Asia. In: Papendick, et al.
Heady, O. E., J. T. Pesek, and V. Y. Rao (1966) Fertilizer Functions from Experimental Data with Associated Supply and Demand Relationships. Agriculture and Home Economics Experiment Station Research Bulletin No. 543, Iowa State University.
Heichel, G. H. (1976) Agricultural production and energy resources. American Scientist 64:64-72.

Hignett, T. P., E. C. Doll, O. W. Livingston, and B. Raistrick (1976) Utilization of Difficult Phosphate Ores. Presented at International Superphosphate and Compound Manufacturers' Association meetings, September 1976, The Hague, The Netherlands.
Hill, L. D. and S. Erickson (1976) Economics of agricultural energy use. Agriculture World 2(3):1-6.
Hill, R. W., R. J. Hanks, J. Keller, and P. V. Rasmussen (1974) Predicting Corn Growth as Affected by Water Management--an Example. CID-211(d)-6, Utah State University.
Hubbert, M. K. (1969) Energy resources. In: Naticnal Research Council. Resources and Man. San Francisco: W. H. Freeman.
Indian Agricultural Research Institute (1972) Recent Research on Multiple Cropping. Research Bulletin 8. New Dehli: IARI.
International Association of Agricultural Economists (1969) World Atlas of Agriculture. Volumes 1, 2, and 3. Chicago, Ill.: IAAE.
International Bank for Reconstruction and Development (1974) Organic Fertilizers: Problems and Potential for Developing Countries. Background Paper No. 4. Washington, D.C.
International Institute for Tropical Agriculture (1974) Annual Report. Ibadan, Nigeria: IITA.
International Rice Research Institute (1972-74) Annual Reports. Los Banos, Philippines.
International Rice Research Institute (1974a) Agricultural Equipment Development Research for Tropical Rice Cultivation. Semi-annual Progress Report No. 19. Los Banos, Philippines.
International Rice Research Institute (1974b) Annual Report. Los Banos, Philippines.
International Rice Research Institute (1975) Agricultural Machinery Development Program. Terminal Report. Los Banos, Philippines.
Intertec Publishing Corporation (1975) Implement and Tractor. Kansas City, Mo.
Jackson, W. A., B. D. Knezek, and J. van Schilfgaarde (1975) Water, soil and mineral input. Pages 201-274, Brown, et al.
Jurion, F. and J. Henry (1969) Can Primitive Farming Be Modernized? Serie HORS 1969. Brussels: Institut National poul l'Etude du Congo.
Kahn, H., W. Brown, and L. Martel (1976) The Next 200 Years. New Yorк: Morrow.
Kass, D. C. (1976) Simultaneous Polyculture of Tropical Food Crops with Special Reference to the Management of Sandy Soils of the Brazilian Amazon. Ph.D. thesis, Cornell University.

Kastens, M. L. (1975) Productivity in world food supply system. Chemical Technology, November:675-677.
Kaul, R. N. and R. Kumar (1975) Types and Usage of Wheat Threshers in Punjab. India FPM-1/75. Punjab Agricultural University, Ludhiana.
Kaul, R. N. and W. E. Splinter (1964) Human Energy Requirements for Operating Selected Agricultural Implements. American Society of Agricultural Engineers Paper No. 64-128.
Keener, H. M., W. L. Roller, R. D. Kline, H. J. Mederski, and R. B. Curry (1975) Grown Organic Matter as a Fuel Raw Material Resource. NASA Report CR-2608.
Keller, J. and L. N. Leininger (1975) An Agricultural Technology Transfer System. Proceedings of Irrigation and Drainage Division Specialty Conference, Logan, Utah. New York: American Society of Civil Engineers.
Kellogg, C. E. and A. C. Orvedal (1969) Potentially arable soils of the world and critical measures for their use. Advances in Agronomy 21:109-170.
Klingebiel, A. A. and P. H. Montgomery (1961) Land-Capability Classification. Handbook No. 210. Washington, D.C.: U.S. Government Printing Office.
Kovda, V. A. (1971) The problem of biological and economic productivity of the earth's land areas. Soviet Geographic (revised and translated) 12(1):6-23.
Lal, R. (1975) Role of Mulching Techniques in Tropical Soil and Water Management. Technical Bulletin 1. Ibadan, Nigeria: International Institute for Tropical Agriculture.
Landsberg, H. (1975) Effective use of weather data and long range forecasting. Proceedings of Soil and Water Management Workshop, U.S. Agency for International Development.
Litsinger, J. A. and K. Moody (1976) Integrated pest management in multiple cropping systems. In: Papendick, et al.
Makhijani, A. (1975) Energy and Agriculture in the Third World. Cambridge, Mass.: Ballinger Publishing Company.
Mather, J. R. (1974) Climatology Fundamentals and Applications. New York: McGraw-Hill.
McNamara, R. S. (1974) Address to the Board of Governors, International Bank for Reconstruction and Development, Washington, D.C.
Merle, W. C. (1975) The Impact of Agricultural Mechanization on Employment and Food Production. Occasional Paper No. 1, U.S. Agency for International Development.

Morrison, F. B. (1949) Feeds and Feeding (21st edition). Ithaca, N.Y.: Morrison Publishing Company.

Mukhopadhyay, S. K. (1975) Demand Functions for Fertilizer: A Methodological Survey. International Fertilizer Development Center Monograph.

Nace, R. L. (1969) World water inventory and control. Pages 31-42, Water, Earth, and Man: A Synthesis of Hydrology, Geomorphology, and Socio-economic Geography, edited by R. J. Chorley. London: Methuen & Company, Ltd.

National Research Council (1972a) Genetic Vulnerability of Major Crops. Washington, D.C.: National Academy of Sciences.

National Research Council (1972b) Solar Energy in Developing Countries: Perspectives and Prospects. Washington, D.C.: National Academy of Sciences.

National Research Council (1974a) African Agricultural Research Capabilities. Washington, D.C: National Academy of Sciences.

National Research Council (1974b) More Water for Arid Lands. Report of the Board on Science and Technology for International Development, Commission on International Relations. Washington, D.C.: National Academy of Sciences.

National Research Council (1975a) Enhancement of Food Production in the United States. Washington, D.C.: National Academy of Sciences.

National Research Council (1975b) Mineral Resources and the Environment. Washington, D.C.: National Academy of Sciences.

National Research Council (1975c) Underexploited Tropical Plants with Promising Economic Value. Report of Board on Science and Technology for International Development, Commission on International Relations. Washington, D.C.: National Academy of Sciences.

National Research Council (1975d) World Food and Nutrition Study, Interim Report. Washington, D.C.: National Academy of Sciences.

National Research Council (1976a) Climate and Food. Washington, D.C.: National Academy of Sciences.

National Research Council (1976b) Systems Analysis and Operations Research: A Tool for Policy and Program Planning for Developing Countries. Washington, D.C.: National Academy of Sciences.

North Carolina State University (1973) Agronomic-Economic Research on Tropical Soils. Annual Report, Soil Science Department, Raleigh, N.C.

North Carolina State University (1974) Agronomic-Economic Research on Tropical Soils. Annual Report, Soil Science Department, Raleigh, N.C.

Notholt, A. J. G. (1975) Phosphate Rock: World Production, Trade, and Resources. Proceedings of the First Industrial Minerals International Congress, Metal Bulletin, Ltd., London.

Nye, P. H. and D. J. Greenland (1960) The Soil Under Shifting Cultivation. Technical Communication 51. Harpenden, England: Commonwealth Bureau of Soils.

Okita, S. and K. Takase (1976) Doubling Rice Production Program in Asia. Overseas Economic Cooperation Fund, Tokyo.

Orvedal, A. C. and K. T. Ackerson (1972) Agricultural Soil Resources of the World. Soil Conservation Service, U.S. Department of Agriculture.

Paladines, O. (1975) Management and utilization of native tropical pastures in America. Potential to Increase Beef Production. Cali, Colombia: Centro Internacional de Agricultura Tropical.

Papendick, R. I., P. A. Sanchez, and G. B. Triplett, eds. (1976) Multiple Cropping (Special Publication No. 27). Madison, Wis.: American Society of Agronomy.

Pearson, R. W. (1975) Soil Acidity and Liming in the Humid Tropics. International Agricultural Bulletin 30, Cornell University, Ithaca, N.Y.

Pereira, H. C. (1975) People, crops and livestock on watersheds. Proceedings of Soil and Water Management Workshop, U. S. Agency for International Development.

Peterson, D. F. (1971) Groundwater development and its use for land reclamation. Pakistan Journal of Science 23:159.

Peterson, D. F. (1972) Literature Review and Discussion of Water Management in Southeast Asia. Ottawa, Canada: International Development Research Centre.

Peterson, D. F. (1974) China Diary. Archives, Utah State University, Logan.

Pinchinat, A. M., J. Soria, and R. Bazan (1976) Multiple cropping in tropical America. In: Papendick, et al.

Pinstrup-Andersen, P. (1976) The Role of Fertilizer in Meeting Developing Countries' Food Needs. Presented at 12th annual conference of the Missouri Valley Economic Association, February 26-28, Tulsa, Okla.

President's Science Advisory Committee (1967) The World Food Problem. Volume 2. Washington, D.C.: U.S. Government Printing Office.

Radke, J. K. and R. T. Hagstrom (1976) Intercropping for wind protection. In: Papendick, et al.

Rawlins, S. L. and P. A. C. Raats (1975) Prospects for high frequency irrigation. Uniform, frequent irrigation optimized the root environment while drastically reducing water use. Science 188:604.

Reidinger, R. B. (1974) Institutional rationing of canal water in northern India: conflict between traditional patterns and modern needs. Economic Development and Cultural Change 23:79-104.
Revelle, R. (1976) Energy use in rural India. Science 192:969-975.
Ruthenberg, H. (1971) Farming Systems in the Tropics. Oxford, England: Clarendon Press.
Salinas, J. G. and P. A. Sanchez (1976) Soil-plant relationships affecting varietal and species differences in tolerance to low available soil phosphorus. Ciencia e Cultura (Brazil) 28:156-168.
Sanchez, P. A. and S. W. Buol (1975) Soils of the tropics and world food crisis. Science 188:598-603.
Sanchez, P. A. and G. Uehara (1977) Management considerations for acid soils with high phosphorus fixation capacity. Chapter 17, The Role of Phosphorus in Agriculture, edited by E. J. Kamprath, F. E. Khasawneh, and E. C. Sample. Madison, Wisc.: Soil Science Society of America.
Schumacher, E. F. (1975) Small is Beautiful: Economics as if People Mattered. New York: Harper and Row.
Seubert, C. E., P. A. Sanchez, and C. Valverde (1977) Effects of land clearing methods on soil properties and crop performance in an ultisol in the Amazon Jungle of Peru. Tropical Agriculture, Vol. 54.
Shields, J. T., O. W. Livingston, and E. A. Harre (1975) Asian Fertilizer Study. Tennessee Valley Authority, Muscle Shoals, Ala.
Simonson, R. W. (1955) Changing place of soils in agricultural production. Science Monthly 81(4):175-182.
Smith, R. (1976) Methane from Agricultural Residue. Proceedings of New England Conference on Energy in Agriculture.
Spain, J. M., C. A. Francis, R. H. Howeler, and F. Calvo (1975) Differential species and varietal tolerance to soil acidity in tropical crops and pastures. Pages 308-329, Soil Management in Tropical America, edited by E. Bornemisza and A. Alvarado. Raleigh: North Carolina State University.
Splinter, W. E. (1975) Alternative Uses of Energy in Agriculture. Proceedings of the 30th Annual Meeting of the Soil Conservation Society of America.
Splinter, W. E. and C. W. Suggs (1968) Theoretical model for man-machine system in repetitive loading operations. American Society of Agricultural Engineers Transactions 11(6):840-843.

Sprintschnik, G., H. W. Sprintschnik, P. P. Kirsch, and D. G. Whitten (1976) Communications to the editor. Journal of American Chemical Society 98(8):2337-2338.

Stangel, P. J. (1974a) Impact of the Energy Crisis and Other Factors on the Fertilizer Industry of Asia. Presented at the Asian Productivity Organization conference, November 26-December 2, Tokyo, Japan.

Stangel, P. J. (1974b) Worldwide Supplies of Fertilizer in Relation to Need 1974-1980. Presented at the American Society of Agronomy meeting, November 1-15, Chicago, Ill.

Stangel, P. J. (1976) World Fertilizer Reserves in Relation to Future Demand. Paper presented at Joint Workshop on Adaptation of Plants to Mineral Stress in Problem Soils, November 22-23, Beltsville, Md.

Stout, B. A. and C. M. Downing (1975) Counterpull: mechanization, rural working conditions. Ceres 8(1):43-46.

Stout, B. A., D. K. Kline, D. A. G. Green, and R. L. Donahue (1973) Agricultural mechanization in equatorial Africa. Agricultural Mechanization in Developing Countries, edited by M. L. Esmay and C. W. Hall. Japan: Shin Norinsha Co., Ltd.

Suggs, C. W. and W. E. Splinter (1961a) Effect of environment on the allowable workload of man. American Society of Agricultural Engineers Transactions 4(1):48-51.

Suggs, C. W. and W. E. Splinter (1961b) Some physiological responses of man to workload and environment. Journal of Applied Physics 16(3):413-420.

Teter, N. C., J. A. DeShazer, and T. L. Thompson (1973) Operational characteristics of meat animals. Parts I, II, and III, American Society of Agricultural Engineers Transactions 16:157-159, 740-742, 1165-1167.

U.N. Industrial Development Organization (1976) Lima declaration, December, 1975. Vienna, Austria.

U.N. Industrial Development Organization (1977) World-wide study of fertilizer industry 1975-2000. Vienna, Austria.

U.S. Agency for International Development (1974) Research Needs for On-Farm Water Management. Proceedings of an international symposium, October 1973, Park City, Utah.

U.S. Agency for International Development (1975) Proceedings of Soil and Water Management Workshop, Washington, D.C.

U.S. Department of Agriculture (1966) Agricultural Water Management in India: A Report by the Soil Conservation Service Team, Soil Conservation Service.

U.S. Department of Agriculture (1971) Guide for Interpreting Engineering Uses of Soils. Soil Conservation Service.

U.S. Department of Agriculture (1975a) The Agricultural Situation in the People's Republic of China and Other Communist Asian Countries: Review of 1974 and Outlook for 1975. Foreign Agricultural Economic Report No. 111. Economic Research Service.

U.S. Department of Agriculture (1975b) Research to meet U.S. and world food needs. Volume 1, Report of a Working Conference, July 9-11, 1975, Kansas City, Mo. Agricultural Research Policy Committee, Beltsville, Md.

U.S. Department of Agriculture (1975c) Soil Taxonomy: A Basic System of Soil Classification for Making and Interpreting Soil Surveys. Agricultural Handbook No. 436. Washington, D.C.: U.S. Government Printing Office.

U.S. Department of the Interior (1974) Water Resources Planning, Social Goals, and Indicators: Methodological Development and Empirical Test. PRWG 131.1. Technical Committee of the Water Resources Institutes of the Thirteen Western States, Office of Water Resources Research. Logan: Utah Water Research Laboratory.

U.S. Department of the Interior (1975) Coal Resources of the United States, January 1, 1974. U.S. Geological Survey Bulletin 1412. Washington, D.C.

U.S. Deparment of Labor (1975) Handbook of Labor Statistics. Reference Editor, Bureau of Labor Statistics.

U.S. National Committee of the World Energy Conference (1974) Survey of Energy Resources. New York, N.Y.

U.S. Salinity Laboratory (1975) Examples of Benefits from Salinity Research. NRP 20730, Riverside, Cal.

von Uexkiill, H. R. (1975) Recent Fertilizer Problems in Asia. Extension Bulletin No. 59, Food and Fertilizer Technology Center, Taipei, Taiwan.

Wolf, J. M. (1975) Soil-water relations in oxisols of Puerto Rico and Brazil. Pages 145-154, Soil Management in Tropical America, edited by E. Bornemisza and A. Alvarado. Raleigh: North Carolina State University.

Wortman, S. (1976) Food and agriculture. Scientific American 235(3):31.

Wrighton, M. S., D. L. Morse, A. B. Ellis, D. S. Ginley, and H. B. Abrahamson (1976) Photoassisted electrolysis of water by ultraviolet irradiation of an antimony doped stannic oxide electrode. Journal of American Chemical Society 98(1):44-46.

Wrigley, G. (1971) Tropical Agriculture; The Development of Production. London: Faber and Faber.

BIBLIOGRAPHY

Balis, J. S. (1976) An analysis of the options for farm mechanization. Agricultural Mechanization in Asia (Tokyo) 7(3):21-27.

Batty, J. C., S. N. Hamed, and J. Keller (1975) Energy inputs to irrigation. Journal, Irrigation and Drainage Division, American Society of Civil Engineers 101:293-307.

Chancellor, W. J. and J. R. Goss (1976) Balancing energy and food production, 1975-2000. Science 192:213-218.

Clark, S. J. (1975) Editor--Energy for Food Production. Proceedings, Energy Symposium III, Kansas State University, Manhattan.

Curlett, J. N. (1975) Energy Use in the Food System. Federal Energy Administration.

Energy Research and Development Administration (1976) Energy from Coal. ERDA Publication 76-67.

Esmay, M. L. and C. W. Hall, eds. (1973) Agricultural Mechanization in Developing Countries. Tokyo: Shin Norinsha Co., Ltd.

Evenson, J. P., D. L. Plucknett, and I. Horton (1973) A Proposed Classification for Agricultural Systems. Volume II, pages 63-89, Proceedings of Second International Symposium on Tropical Root and Tuber Crops, College of Tropical Agriculture, University of Hawaii, Honolulu.

Feth, J. H. (1973) Water Facts and Figures for Planners and Managers. U.S. Geological Survey Circular 601-1, Washington, D.C.

Gemmill, G. and C. Eicher (1973) The Economics of Farm Mechanization and Processing in Developing Countries. Agricultural Development Council Seminar, Michigan State University, East Lansing.

Green, M. B. and A. McCulloch (1976) Energy considerations in the use of herbicides. Journal of Scientific Food and Agriculture 27:95-100.

Greenland, D. J. (1975) Bringing the green revolution to the shifting cultivator. Science 190:841-844.

Gunkel, W. W., D. R. Price, G. M. Lucas, D. L. Murray, G. L. Casler, and S. Sutter (1976) Energy Requirements for New York State Agriculture. Agriculture Engineering Extension Bulletin 406, Cornell University, Ithaca, N.Y.

Hsiang, N. (1962) Certain Problems of Agricultural Mechanization. Jen-mim Jih-pao, Peking.

Hsiang, N. (1965) Stable and High Yields and Agricultural Mechanization. Jen-mim Jih-pao No. 3476, Peking.

Instituto Colombiano Agricola (1976) Prototipos disenados of construidos por el program de maquinaria agricola. Bolitin No. 15, Colombia.

International Crops Research Institute for the Semi-Arid Tropics (1975) International Workshop on Farming Systems, November 18-21, 1974, ICRISAT, Hyderabad, India.

International Rice Research Institute (1975) Proceedings of the Cropping System Workshop. Los Banos, Philippines: IRRI.

Khan, A. V. (1976) Agricultural mechanization and machinery production in China. Agricultural Mechanization in Asia (Tokyo) 7(2):60-65.

Kleis, R. W. (1976) Energy Related Impacts on Great Plains Agricultural Productivity in Next Quarter Century. Task Force Report, Great Plains Agricultural Council, University of Nebraska, Lincoln.

Krenz, J. H. (1976) Energy Conversion and Utilization. Boston: Allyn and Bacon, Inc.

Leopold, L. P., K. S. Davis, and the editors of Time-Life Books (1966) Water. New York: Time-Life Books.

Loomis, R. S. (1976) Agricultural systems. Scientific American 235(3):99-105.

McColly, H. F. (1971) A proposal for agricultural mechanization in the developing countries of Southeast Asia. Agricultural Mechanization in Asia (Tokyo) 2:21-25.

National Research Council (1962) Basic Problems and Techniques in Range Research. Publication No. 890, Agriculture Board, Division of Biology and Agriculture. Washington, D.C.: National Academy of Sciences.

National Research Council (1975) Agricultural Production Efficiency. Washington, D.C.: National Academy of Sciences.

Pollard, W. G. (1976) The long range prospects for solar-derived fuels. American Scientist 64(5):509-513.

Reed, T. B. and R. M. Lerner (1973) Methanol: A versatile fuel for immediate use. Science 182:1299-1304.

Roller, W. L., H. M. Keener, R. D. Kline, H. J. Mederski, and R. B. Curry (1975) Grown Organic Matter as a Fuel Raw Material Resource. National Aeronautics and Space Administration Report CR-2608.

Sanghi, A. K. and R. Klepper (1976) An Economic Analysis of Irrigated Farming with Diminishing Ground Water Reserves. Center for the Biology of Natural Systems, St. Louis, Mo.

Shell Oil Co. (1976) The National Energy Outlook 1980-1990. Shell Oil Co., Houston, Texas.

Smerdon, E. T. (1975) Fuel Use Estimates for Florida Agricultural Production. University of Florida, Gainesville.

Spedding, C. R. W. and N. R. Brockington (1976) Experimentation in agricultural systems. Agricultural Systems 1(1):47-56.

U.S. Department of Agriculture (1972) Soils of the World Map. Soil Geography Unit, Soil Conservation Service.

Ward, D. J. and H. R. Fortmann (1976) A National Program of Agricultural Energy Research and Development. Report to National Planning Committee of the Agricultural Policy Advisory Committee, Washington, D.C.

Washington State University (1975) Energy Facts and Figures. Cooperative Extension Service EM 3943, Pullman, Wash.

Weinberg, A. M. (1976) The maturity and future of nuclear energy. American Scientist 64:16-21.

Welch, R. O. (1974) The Energy Requirements of Meal Preparation; A Comparison of Restaurant vs. Home. Midwest Research Institute Report, Kansas City, Mo.

Wijewardene, R. (1976) Engineering for Farming Systems in the Lowland Humid Tropics. International Institute of Tropical Agriculture Technical Bulletin #6. Ibadan, Nigeria: IITA.

Report of Study Team 5

WEATHER AND CLIMATE

STUDY TEAM 5

A formal study team was not established. The recent National Academy of Sciences study, Climate and Food, Climatic Fluctuation and U.S. Agricultural Production, was drawn on heavily for materials in this field and the following consultants were used:

Wayne L. Decker (Chairman of the committee responsible for Climate and Food), University of Missouri
Louis J. Battan, University of Arizona
William C. Burrows, Deere and Company
Ray E. Jensen, National Oceanic and Atmospheric Administration
John E. Kutzbach, University of Wisconsin
Richard C. McArdle, U.S. Department of Agriculture
J. Murray Mitchell, Jr., National Oceanic and Atmospheric Administration

ACKNOWLEDGMENTS

We would like to thank the Committee on Climate and Weather of the Board on Agriculture and Renewable Resources (BARR) of the National Research Council. Although they are not responsible for this report, they provided the complementary publication, Climate and Food, and important assistance in the preparaticn cf this report. Special thanks goes to Philip Ross, Bille Hougart, A. Michelle Moore, and other BARR staff for their help in many ways.

TABLE OF CONTENTS

INTRODUCTION, 247
The Scientific Problem of Climate, 249
Climate and Climatic Change in Perspective, 250

Profile 1 Limits of Weather and Climate Predictability (Methodological Emphasis), 255
Profile 2 Predictions of Climate (Adaptive Emphasis), 261
Profile 3 Inadvertent Climate Modification Resulting from Human Activities, 266
Profile 4 Prospects for Weather Modification, 272
Profile 5 Adaptation of Agricultural Practices to Weather and Climatic Variability, 278
Profile 6 Use of Weather and Climate Information by Agriculture, 281

Appendix A Some Characteristics of the Climatic System, 288
Appendix B Example of the Use of Weather Data in a Risk Management Model for the Construction Industry, 291

INTRODUCTION[1]

Few things are so pervasive in human affairs as weather and climate. Both weather and climate, in their various manifestations, have a direct effect on such basic matters as the clothing people wear and the shelters they build. Weather and climate also have an effect on a wide range of other human activities, such as transportation and sports, for example, and both play an important role in the health of people everywhere.

Yet nowhere, perhaps, are the effects of weather and climate more evident, or more important, than in agriculture. It is for this reason that any study, such as the World Food and Nutrition Study, devoted to increasing the world's production of food and improving the nutritional quality of that food must be devoted in part to an analysis of the relationship of weather and climate to agriculture. This report highlights weather and climate research germane to agriculture.

As defined in other reports of the National Academy of Sciences (NAS) (NRC 1975, NRC 1976a), "...'weather' refers to events, such as precipitation and temperature, occurring within a two-week period, while 'climate' is associated with events occurring over longer time spans...(NRC 1976a)." Research on climate has evolved from the recognition that climate is much more than just "average weather," and this discussion will focus primarily on climate (see Appendix A for characteristics of the climatic system).

The weather and climate research areas discussed in this report are justified by several considerations:

-- In most regions of the world variability of climate is the rule rather than the exception. Thus it is safe to expect that future climatic variations will be of sufficient magnitude to have a significant effect on agricultural production (Beltzner 1976, Institute of Ecology 1976).
-- The world's food supply is precariously balanced, considering the possible effects of a single year's

poor harvest due to variations of weather and climate (NRC 1976a).

- A famine has occurred in some part of the world nearly every year since World War II, the most frequent causes being droughts and floods (Mayer 1975).
- A high probability exists that certain human activities, such as the burning of huge amounts of fossil fuels, may lead to climatic variations that would substantially alter global patterns of food production and the total amount of food produced in the world (Wilson et al. 1971, Kellogg 1977).

Effective national and international programs for climate and weather research are needed. Because of the high costs of weather research, agencies undertaking this research should develop strong competitive programs that make the maximum joint use of experimental design, equipment, and data. For example, the National Oceanic and Atmospheric Administration (NOAA), the National Science Foundation (NSF), and the National Aeronautics and Space Administration (NASA) participate in the worldwide Global Atmospheric Research Program (GARP). As another example, NASA and the U.S. Department of Agriculture (USDA) cooperate on many programs, especially on improved crop production forecasts. USDA, in cooperation with NASA and NOAA, has used the advanced technology of these and other worldwide programs to improve its research endeavors, such as the Large Area Crop Inventory Experiment (LACIE). The Center for Climatic and Environmental Assessment (CCEA) of NOAA has worked on statistical weather-crop yield models for U.S. and foreign areas. And, the Air Force Environmental Technical Application Center has supplied weather data to CCEA and other U.S. agencies.

The general climatic research and the agricultural adaptation research recommended are important for both the high-income and developing countries. But the leadership role will rest with the United States and other high-income countries. The results in many cases will not be immediately useful for the developing countries. However, some research is already quite applicable to the needs of those countries.

The amount of meteorological research undertaken in the developing countries varies widely, but some have made significant general climatic research contributions as well as helpful adaptations to their own country or region. A few countries have established meteorological research institutes which usually have strong connections with universities or agricultural research institutes that conduct research in agrometeorology. Weather services and research in

most developing countries are oriented toward aviation or general public needs, not agricultural needs. In some instances, the expense of facilities and equipment for such work is difficult to justify for the program of an individual country. Where this is the case, regional programs will have to be developed for adaptation of the more complicated programs (Pearce 1976).

Data bases do not exist in some developing countries and must be built up. Most countries have several decades of records of rainfall, but generally the data have not been analyzed. Because the scientific infrastructure is often lacking, plans should be made in the developing countries as well as in the high-income countries to relate agriculture to general climate and weather research.

Some international organizations are developing programs to support this type of research and some agricultural emphasis exists; for example, the World Meteorological Organization (WMO) has a Commission for Agricultural Meteorology and Hydrology. The Food and Agriculture Organization (FAO) of the United Nations has an active program for aiding these joint meteorological programs with agricultural implications.

THE SCIENTIFIC PROBLEM OF CLIMATE

The earth's atmosphere consists of a remarkably resilient layer of gases, an atmospheric envelope that rotates with the earth but does not rest quietly upon it. The earth's uneven distribution of solar energy causes winds which are strongly affected by the earth's rotation. Air circulation patterns in the atmosphere range downward in scale from the circumpolar wave-like vortex, or jet stream, at altitudes of 20,000 to 40,000 feet through anticyclones and cyclones (more commonly known as highs and lows), hurricanes, tornadoes, and mesoscale wind systems to convective clouds and, finally, small-scale turbulence. One of the central problems of meteorology is to explain these atmospheric disturbances and how they interact.

Intimately linked to the phenomenon of air in motion is that of energy distribution. Sources and sinks of heat energy vary in number and strength. They come into existence mainly in response to: the disposition of short-wave solar radiation; the flux of outgoing long-wave radiation from the land, sea, and air; the latent heat involved in phase changes of water; and the flow of sensible heat at the conjunction of the oceans and land with the lower atmosphere. Large quantities of heat are transported by the winds and by ocean currents.

As a result of GARP, scientists are now learning a great deal more about the processes of energy exchange among the atmosphere, the oceans, and the earth's surface. Greater knowledge should bring with it a much better understanding of air movements and their effects on the weather.

In addition to its thermal activity, the atmosphere is the site of many physical, chemical, and biological processes. Many of these, such as the photochemical processes of ionization and recombination of gaseous components, are only partly understood. The chemical equilibrium of ozone in the upper atmosphere has a profound influence on the earth's biological processes and may be a link between fluctuations in the emission of ultraviolet energy from the sun and atmospheric movements.

The distribution and decay of natural and artificial radioisotopes, and the chemical cycles of carbon, oxygen, phosphorus, sulfur, nitrogen, and other materials in the atmosphere are not fully understood, and neither are the processes by which particulate matter is transported, diffused, and removed from the atmosphere. These two problems, which have ecological significance, are related. The phase changes of water substances and the growth of cloud particles to raindrop size offer physical-chemical problems which are of practical as well as of scientific interest. Similarly, the fair weather electrical field in the atmosphere and the charge separation mechanism which transforms it under stormy conditions are problems requiring research.

Briefly, the atmospheric problem consists of: air motions; atmospheric energetics; and physical, chemical, and biological processes.

CLIMATE AND CLIMATIC CHANGE IN PERSPECTIVE

According to the best scientific evidence and interpretation, the earth came into existence (in a manner not fully understood) nearly 5 billion years ago. The primordial atmosphere was devoid of free oxygen and consisted of a mixture of hydrogen, methane, ammonia, and water vapor, from which the amino acids and simpler organic compounds were synthesized by an energy source, such as lightning or solar ultraviolet radiation. Volcanic activity added more water vapor, nitrogen, and carbon dioxide, and further cooling created warm shallow seas. The processes of life began more than 3 billion years ago in this warm oceanic broth in the form of anaerobic organisms dependent upon ambient organic molecules and amino acids for nourishment.

During the next billion years an equally significant step occurred. A few of the organisms succeeded in changing their mode of existence from one of dependence on fermentation to the more efficient modes of photosynthesis and respiration. This set the stage for the release of oxygen, the fixation of nitrogen, and further evolution, as cells developed the capacity to divide by mitosis, a necessary precondition for higher life. As organisms that could not tolerate free oxygen were replaced by more efficient respiring forms, oxygen-mediating enzymes appeared as the chemical catalysts governing metabolism, producing the structures prescribed by the DNA molecules.

These biological processes caused the oxygen content of the atmosphere to increase. Under the impact of ultraviolet radiation, oxygen molecules dissociated into highly reactive oxygen atoms and, particularly at upper levels, into equally reactive ozone which subsequently provided a protective shield for biological organisms with a low tolerance of ultraviolet radiation.

For most of the last billion years, the global climate has been comparatively mild. Warm spells on the order of several hundred million years have been interspersed with glacial periods of tens of millions of years. Thus glaciers covered much of the earth's land about 600 million years ago and again about 300 million years ago. Even during those glacial periods, however, there were repeated alterations between relatively warm and much colder periods.

Over the past half million years, relatively warm interglacial periods of about 10,000 years have occurred about every 100,000 years. We are currently in such an interglacial warm period which began about 10,000 years ago. This warm period followed a major glacial maximum 18,000 years ago which chiefly affected North America, northern Europe, and, to some extent, Siberia. During this transition period, climatic instabilities developed.

The warm period reached its high point 4,000 to 8,000 years ago at a level 2° to 3°C above present temperatures. A cold period 3,000 years ago gave way to warming which lasted from approximately 1100 to 1400, followed by the "Little Ice Age" which prevailed from 1400 to 1850. Global warming in the northern hemisphere of about 1°C persisted from 1850 until 1940, followed by a fall of 0.6°C since then.

Superimposed on these large-scale, long-term climate variations were climatic anomalies which had a more immediate effect on plant and animal development than the slower variations. Six periods of climatic anomalies between 1895 and 1974 have been identified as strongly affecting agriculture (Beltzner 1976). The

period 1895 to 1905, for example, was characterized by a wet period in the northwestern Great Plains; sustained drought in the Pacific Northwest; extreme cold in the Gulf States; heat waves in 1896, 1901, and 1904; East Coast and Great Lake storms; and the disastrous, hurricane-induced Galveston flood. Wheat yields in the United States varied from 14 percent above the average for the period to 16 percent below, with only two of the 11 years actually equaling the average. Crop yield variations in Canada were even greater.

Other climatic anomalies have included the Midwest drought of 1933-37, the Mexican drought of 1937-1941, a period of highly variable anomalies from 1950 to 1958, the unusually large amounts of sea ice from 1966 to 1972, the heavy snowfalls of 1970-72, and the late wet spring of 1974. The year 1972 was particularly unusual, with a very cold winter in northwestern Canada and the Soviet Union; droughts in Southeast Asia, the Sahel, Australia, Chile, and Peru; floods in North Africa and southern Europe; high ocean temperatures off Peru; and unusually good weather in the American Cornbelt.

Advances in agricultural technology can ameliorate but have yet to eliminate the harmful effects of climatic extremes. The 1976-77 drought in Kansas, for example, meant the abandonment of 920,000 hectares of land (17.6 percent of the state's total wheat acreage) and a 15 percent decline in yields from hectares actually harvested (U.S. Senate 1976). Clearly short-term weather and climatic anomalies (seasons to years) have important effects on agriculture.

In recent years, serious attention has been given to the impact of human activities on weather and climate. Quite apart from deliberate attempts to influence the weather, it is evident that human affairs associated with urbanization are beginning to have an effect on climate. Substantial evidence indicates that continued growth over the next few decades in the use of fossil fuels will increase the carbon dioxide content of the atmosphere and thereby might cause significant changes in the regional and world climate. The implications of this sequence of events are so great that the scientific problems and their social, economic, and political ramifications merit study as a matter of urgency (Baes et al. 1976).

A major question that arises is whether a balance can be achieved between the anthropogenic effects of heating the atmosphere and the changes in climate that will result from predictable alterations in the earth's solar orbit. The evidence at hand suggests that anthropogenic warming is likely to take place over a period of decades or centuries whereas natural cooling

would characteristically take place over thousands or tens of thousands of years. At the moment, our powers of prediction are not great enough to allow us to compare anthropogenic forces with natural ones. We do not know what constitutes the optimum global climate, and it is doubtful that what would be desirable for one part of the world would be desirable for another part (see Figure 1).

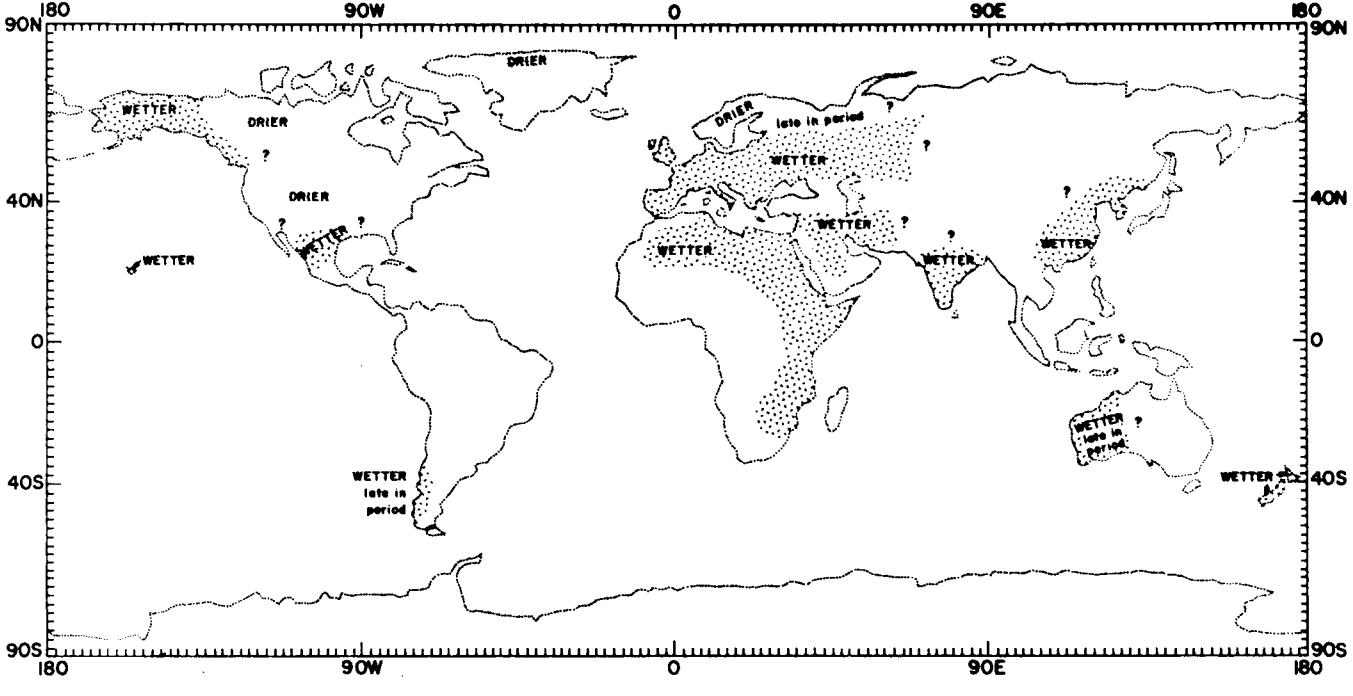
Recently, new evidence has been found to support the Milankovitch hypothesis relating climatic changes to the earth's orbit around the sun (Hays et al. 1976). Discrete spectral peaks, found at 23,000, 42,000, and 100,000 years, are related to dominant periods in the earth's solar orbit and explain substantial fractions of the total variance on those time scales. From this, it could be inferred that a decrease in the earth's temperature over the next several thousand years would lead to extensive glaciation if anthropogenic effects were eliminated.

Unresolved problems related to these topics constitute an endless litany. For present purposes, it is sufficient to identify four fundamental areas of climatological research with implications for agriculture:

- the limits of weather and climate predictability;
- predictions of climate on a monthly and seasonal basis, on an annual to decadel basis, and on a decadel to century basis, along with predictions of probable changes in climatic variability, at least for a 5- to 10-year period;
- inadvertent climate modification resulting from human activities;
- the prospects for designed weather and climate modification on a national and global scale.

Two additional research areas that would have a direct effect on agriculture are:

- the adaptation of agricultural practices to weather and climatic variability, with particular emphasis on the relationships between biological and meteorological variables;
- the use of weather and climate information by farmers and agribusiness managers, with particular emphasis on the use of (1) statistics to estimate characteristics of weather and climate variability, (2) benefit-cost analysis to reduce farm risks due to the weather, and (3) improved management methods in farm production, agricultural marketing, and resource use.



SOURCE: Kellogg (1977).

FIGURE 1 Schematic map of summer rainfall distribution during the Altithermal Period (Climatic Optimum). The terms "wetter" and "drier" are relative to the present. Blank area may indicate inadequate data.

Profile 1

LIMITS OF WEATHER AND CLIMATE PREDICTABILITY (METHODOLOGICAL EMPHASIS)

Little can be said at this time about the limits of climatic prediction. We do not know whether future changes in climates will be predictable in the detail and over the ranges of time needed to satisfy many needs of society. The theory and analytical methods for this research need to be advanced.

The prospects for predicting climatic variability due to mechanisms within the world's climatic system are also unknown. To assess the chances for predictability, mathematical models of the system that extend well beyond present models in geophysical scope and complexity are needed in order to deal with the many physical factors that account for climatic variability.

The task of predicting climatic variability due to factors arising outside the world's climatic system also require research. Here, successful climatic prediction will depend upon the predictability of the outside changes, which at the present time are virtually unknown.

A panel of experts reporting to the Executive Committee of the World Meteorological Organization (1977) states that:

> Until further progress in climate research is able to establish the possibilities for climate prediction through physical reasoning, the assessment of future climate developments must be based upon statistical inferences having a relatively low level of information content. Such inferences are confined largely to assessments of the probability of various alternative developments which are suggested as possible either by past experience or by insights into future impacts of man's activities.

RESEARCH NEEDS

For agricultural purposes, it is desirable to be able to predict (1) regional climatic anomalies that may last from several months to several years, (2) anthropogenic changes in climate that will offset or even reverse trends resulting from either internal or external climatic forces, and (3) long-term variations in the frequency with which regional climatic anomalies may be expected.

These predictions can be approached in two ways. The first is to examine past records in an attempt to identify connections between climatic anomalies in one part of the world and changes occurring later, either in the same area or elsewhere. It has been demonstrated, for example, that sea surface temperatures in the Aleutian Islands during the summer are correlated with abnormal air pressure patterns in the same area during the fall (Namias 1976, Dickson and Namias 1976). These pressure patterns in the Aleutians, in turn, appear to be linked to climatic anomalies over the North American continent. Similarly, the importance of atmospheric conditions over the Atlantic seaboard region of the United States in prolonging and reinforcing climatic regimes over the North Atlantic Ocean and other areas has been postulated. When further developed, these historical techniques may provide useful probabilistic predictions of climatic anomalies elsewhere on the globe.

The second way to make all three types of predictions is through climatic modeling, a concept developed by John von Neumann in 1960 and given strong impetus the following year by President Kennedy, who called for an international collaborative effort "in weather prediction and eventually in weather control (address before U.N. General Assembly, September 5, 1961)." In response to a resolution passed unanimously by the United Nations, (U.N. Resolution 1802, December 14, 1962), the world scientific community outlined an ambitious program (International Council of Scientific Unions 1965), which was converted in 1967 into the Global Atmospheric Research Program. GARP was given the following objectives: (1) to assess the atmosphere's transient behavior as manifested in the large-scale fluctuations that control changes of the weather, in order to increase the accuracy of weather forecasting; and (2) to specify the factors that determine the statistical properties of the atmosphere's general circulation, in order to gain a better understanding of the physical basis of climate.

The program designed to achieve these objectives has two parts: (1) the design and testing, both by computational methods, of a series of theoretical

models of various aspects of the atmosphere's behavior to obtain an increasingly precise description of atmospheric processes and interactions; and (2) observational and experimental studies of the atmosphere to provide the data required to design the theoretical models and to test their validity. Laboratory studies of the dynamic properties of rotating fluid systems also can contribute to a better understanding of the earth's general circulation and climate.

During the past few years a considerable amount of scientific effort has been devoted to achieving GARP's first objective, an assessment of the transient behavior of the atmosphere. In 1974, a field study lasting 100 days and covering an area of 20 million square miles was carried out to ascertain interactions among atmospheric disturbances in the tropics (Hammond 1975). Five thousand scientists and technicians from 72 countries were used to staff 1,000 land weather stations and a network of special upper-air sounding stations, as well as to collect atmospheric data while aboard 40 ships and 12 aircraft. Six earth satellites also were used to collect data. An even larger effort, one that will cover the globe, is now being planned for 1978-79.

Work also has been proceeding on the second GARP objective, specifying the factors that determine the general circulation of the atmosphere. In 1974, the Joint Organizing Committee (JOC), an overseeing scientific body appointed jointly by WMO and the International Council of Scientific Unions (ICSU), selected the following processes for intensive study:

-- Radiation: basic radiation processes, theory of radiant transfer, parameterization, modeling, and observational studies.
-- Clouds: cloud physics, modeling, parameterization, observational studies, and validation of models.
-- Land surfaces: momentum transfer, energy transfer, and transfer of matter; parameterization; sensitivity studies; and observational requirements.
-- Oceans: fluxes of heat, momentum, and water vapor; small-scale turbulence and internal waves; ocean models; ocean-atmosphere models; sensitivity studies; and observational requirements.
-- Cryosphere: dynamics and thermodynamics of sea ice, sheet ice, and glaciers; variations in extent of snow and ice; parameterization of planetary boundary over Antarctica; analysis and measurement of coupling between Antarctic ice sheet and underlying land and water; ice sheet models; and observational requirements.

-- Carbon dioxide: photochemistry, photobiolcgy, the global carbon cycle, monitoring of carbon dioxide levels, incorporation into atmospheric models, and sensitivity studies.
-- Aerosols: optical properties, incorporation into models of radiant energy transfer, sensitivity studies, and residence times in stratosphere and troposphere.

IMPACT

As mentioned earlier, the two ways to achieve climatic predictions that would be useful to agriculture are to make predictions based on historical weather data, and to collect current data and to elucidate basic atmospheric processes. Research of the second type, as outlined in the GARP objectives, is also a prerequisite for many other climatological studies. If the limits of predictability can be ascertained, progress on nearly every other aspect of weather and climatic research can be more effectively planned and executed.

The research directed toward the GARP objectives is expensive and has a high risk. Also, the timing of the effects is uncertain. However, the agricultural benefits--which are difficult to estimate--that could be achieved through improved allocation of resources and greater certainty of global and regional climatic change could be much more than the cost of research. This research can be expected to be particularly significant for weather prediction in the tropics with resultant benefits to agriculture. Various countries involved in the so-called Monsoon Experiment (MONEX), such as India, Japan, and the U.S.S.R., are already planning to adapt GARP research results to their own region (Pearce 1976).

Such research must stress observation and short-term predictions. The geostationary satellites could dramatically improve data coverage in the tropics and subtropics. This could lead, in turn, to improved "nowcasting," for example, to give warnings of storms and flash floods, better estimates of soil moisture, and improved crop surveys. According to Pearce (1976):

> Information is provided routinely by Weather Services specifically for the benefit of agricultural and hydrological interests in some tropical countries, e.g., East Africa. In many, however, the basic data itself is [sic] not being obtained and it will be necessary to set up an adequate network of surface stations to obtain this data [sic]

(temperature, moisture, rainfall, sunshine and wind) before any analysis can be carried out. In some countries stations with records (at least of rainfall) extending back as far as 50 to 100 years do exist, but this data has [sic] not been analysed; the station network (and communications) even in these countries is still inadequate for optimum benefits to agricultural and water resource interests to be achieved.

Some countries, e.g., Nigeria, East Africa and India, have at least one agricultural research institute which carries out agrometeorological research involving special instrumentation and sophisticated analysis techniques. Whether or not the present effort even in these countries in this research area is adequate for their future economic needs is difficult to judge. This is a matter which should be examined with some urgency, and standards set for the levels of agro- and hydro-meteorological research, includng basic observing networks, for all tropical countries. Attention should also be given to the use of weather satellite data, particularly since a geostationary satellite network covering the entire tropics is planned for 1979.

The ability to anticipate climatic anomalies, in particular, would help markedly in the management of food production, storage, and distribution. Research now underway on the relationship of climatic stress to protein content in rice by the International Rice Research Institute (IRRI) is one illustration of the potential benefits.

Much of the needed research could build on existing research efforts so the marginal costs often would not be excessive. The more general weather and climate research programs should not take funds from the more applied agricultural meteorology research. But where such programs are going forward, the more general research should consider agricultural implications to the extent practical.

IMPLEMENTATION

Planning for another global field experiment on the first GARP objective is now underway. This experiment will involve five geosynchronous meteorological satellites and four polar orbiting satellites, 4,500 land weather stations, numerous upper-air sounding

stations, 6,000 ships that will participate in a Voluntary Observing Ship program, and several hundred ocean buoys in the southern hemisphere. Specially instrumented aircraft, special communications links, and the most modern data management and processing systems will be used. In fiscal year 1976, the United States contributed a total of $23 million for the research, design, and development costs of this pending experiment, as well as about $1.5 million in support of the augmented observation system.

Work on the second GARP objective is also continuing, following a meeting of the JCC board in December, 1976 to draw up a plan for implementation during the 1980s.

U.S. personnel and institutional support for these two GARP objectives should come through augmentation of existing research efforts.

Profile 2

PREDICTIONS OF CLIMATE (ADAPTIVE EMPHASIS)

The ultimate goal of much of the current research on climate is to obtain more accurate predictions. If this proves impossible, attention must focus on measures of climatic variability and the probability of future variations.

Progress toward a quantitative theory of climate has been made. A combination of mathematical models of climate and observational studies of the physical processes believed to govern the overall behavior of the atmosphere gives the theory promise. Furthermore, fairly realistic simulations of the atmospheric part of the climatic system are now possible. As yet, however, they are not adequate enough to explain many important aspects of climatic variability.

RESEARCH NEEDS

The purpose of the research proposed here is to move from success in short-term prediction (a few days) to success in predicting weather over longer periods. The basic theory and much of the model design for these long-range predictions will come from the research proposed in Profile 1. The essential emphasis in this profile is on model extension, adaptation, and implementation that recognize the multitude of uses for such predictions. This research would concentrate on developing the methodology needed to predict weather and climate for three time spans: one month to a season, 1 to 10 years, and 10 to 100 years.

This research must take into account analyses of historical variability in climate. Analyses of this type help to orient research on prediction and provide knowledge of the limits of climatic expectations.

A range of modeling approaches must be examined. For example, the process of relating solar activity as reflected in sun spots is undergoing renewed interest. Adaptations of many models used for other purposes need

to be considered for possible use with meteorological data.

IMPACT

The fact that weather and climate have a dominant influence on agricultural production, marketing, water supply, and energy use has long been recognized. However, a comprehensive assessment of the impact of better climatic predictions on human affairs is lacking. Previous assessments of impact have been highly fractionated. Maunder (1970) has made a comprehensive study of this problem, but most studies have narrowly concentrated on the economic impacts of weather variation and inadequate predictions.

It would be misleading to suggest that better predictions would eliminate the economic costs attributable to weather variations. However, estimates of expected variation can set some estimates of the possible costs of decisions made without climatic information.

The Institute of Ecology (1976) has shown that climatic variations have a profound effect on crop production. For example, their studies show that if 1936 weather conditions had prevailed in 1975 (assuming 1975 crop area and 1973 level of technology), the United States crop would have been reduced 27 percent, or 71 million tons, for the combined production of corn, wheat, sorghum, and soybeans. The effects would be less severe in other years, but the results dramatize the costs of climatic variation and serve as some indication of the potential that could be reached through improved availability and use of climatic information.

Stated differently, the weather of 1972 resulted in a halving of global food reserves (abetted in particular by the shortfall in Russian grain production) and more than a doubling of some food prices. Similarly, weather problems in 1974, which took the form of excessive rains which delayed spring planting and a dry summer and early frosts, caused crop losses that greatly reduced U.S. corn and soybean production at a time when world needs were urgent. This urgency was accentuated by insufficient rain during the monsoon period in some parts of southeast Asia and India and floods in other parts of the world. Meanwhile, the Sahel drought, which lasted for approximately five years, was bringing famine to millions and death to untold thousands. In addition, the collapse of the Peruvian anchovy harvest in 1972 and 1974 (apparently related to fluctuations in ocean currents and atmospheric circulation) had a profound

impact on world fertilizer supplies and the price of soybeans and other protein feed stocks.

One must caution that we do not know how effectively improved weather predictions or other weather and climate research would be used. For example, the Sahel drought awakened the world's conscience to the human suffering that is possible, and many researchers want to contribute to its alleviation. But more research will be needed even to evaluate the potential use of such research itself. Recent literature cautions about expecting complete acceptance of such research results and their use (Glantz 1976, Glantz 1977, Katz and Glantz 1977). Also, education and development programs must accompany research programs on weather and climate.

Yet, examples of the effectiveness of weather and climate research are available. Pearce (1976) states:

> If...weather predictions, particularly of the onset or recession of rains, were sufficiently accurate, operations planned on the basis of these predictions would result in considerable savings, for instance by planting or harvesting crops at the times giving optimum yields. This would be true even allowing for occasional losses due to badly timed operations arising from misleading predictions. A specific example relates to the onset of the Indian summer monsoon. Sowing operations can commence only after the monsoon sets in; however, the showers usually assumed to represent its onset sometimes prove to be a false alarm and may be followed by several dry days during which many of the seedlings which germinate die off due to water stress. Many of the poorer farmers do not have sufficient reserves of seed to resow. Clearly with a reliable forecast of such conditions planting could be delayed with considerable consequent savings.
>
> At present, in some developed countries, forecasts of up to a few days ahead are now issued by weather services for the benefit of farmers and have been found to be of considerable value. In India medium range forecasts are issued 2 months ahead of the NE and SW monsoons; in East Africa special agricultural forecasts are issued in connection with cotton spraying in Tanzania. In Nigeria the meteorological service publishes routinely agrometeorological bulletins containing climatological data. The use of weather forecasts for agricultural

planning is still, however, either non-existent or at a minimal level in most countries, both developed and under-developed....

Another example of economic benefit from more accurate weather forecasts is that to be derived in many areas from water resource management operations, such as by optimizing water conservation, e.g., in irrigation schemes through controlling reservoir releases. Forecasts on all time-scales from a few hours to a year or so ahead are of considerable value to water engineers:

(i) forecasts of heavy rain for up to a few hours enable emergency measures to be taken to minimize flood damage and, in semi-arid areas, conserve as much as possible of the rainfall;
(ii) forecasts of up to a few days ahead of 'rain' or 'no rain' are highly valuable for reservoir operation and irrigation control;
(iii) accurate long-period (i.e. 2 weeks to several months) forecasts of drought would again be highly valuable for reservoir operation and groundwater extraction.

IMPLEMENTATION

The prospects for obtaining answers to some of the scientific questions about weather prediction are good. The techniques are complicated and have been partially discussed in Profile 1.

Enough is known about the physics, chemistry, and biology of atmospheric processes that many climate relationships can be specified in classical mathematical terms. The result is a set of nonlinear, partial differential equations expressing in mathematical form the conservation of momentum, the conservation of energy, the conservation of mass (including water vapor), and the equation of state. When developed for the globe, this set of equations is known as a General Circulation Model (GCM). Although not amenable to analytical solution, the set can be integrated by finite difference techniques (time steps between 5 and 10 minutes) on a high-speed computer.

Both theoretical considerations and practical computational constraints make it unlikely that successive states of the atmosphere can be predicted and then averaged in time to obtain the climate, but

the methodology employed in statistical mechanics appears to offer a viable approach. By analogy to thermodynamics, the detailed motions of gas molecules correspond to the weather and the statistical properties of a gas (e.g., temperature and pressure) correspond to climate. Statistical sampling and estimation theory are involved in deducing the relevant properties of a nonlinear dynamic system so that the amount of climatic "noise" can be identified and internal behavior distinguished from external forcing functions. Research utilizing this approach is just now getting underway. The magnitude of the task is impressive, but not overwhelming (Leith 1976).

The introduction of electronic computers has enormously simplified handling the complex sets of differential equations that have been identified and the ensemble averages now being explored. The result is a powerful tool for testing theory against observation, since general circulation models of the global atmosphere run between 10 and 100 times faster than real time.

Measurements of atmospheric factors have been dramatically improved as a result of technological innovations. Polar-orbiting and geosynchronous satellites provide global overviews, while microwave sensing systems detect the three-dimensional field of motion within a single convective cloud or tornado.

Impressive steps toward an understanding of climatic change have come from reconstructing climate conditions over wide ranges of time and space from proxy data (CLIMAP 1976). Biological transfer functions and oxygen isotope stratigraphy, extended locally by standard micropaleontological and lithological techniques, provide a way to compile global charts of sea surface temperatures, ice extent, and continental albedo as far back as 18,000 years ago. This information is not only of great interest and usefulness in its own right, but it also provides an essential set of boundaries for analysis, by means of three-dimensional models of the conditions required to maintain a particular climate in equilibrium. Modeling and climate reconstruction are proceeding in concert, and it is clear that a powerful new tool for understanding climatic change is at hand.

Implementation of this research still means theory development, but emphasis will be on adaptation model design and testing. Developing country adaptations, and agricultural adaptations particularly, will need to be considered. Developing country and regional centers should be encouraged since they are logical organizations to adapt this work.

Profile 3

INADVERTENT CLIMATE MODIFICATION RESULTING FROM HUMAN ACTIVITIES

INTRODUCTION

The possibility that certain widespread human activities such as the burning of fossil fuels may have profound effects on the earth's atmosphere has drawn considerable attention in recent years. While no scientific evidence exists to prove that inadvertent but substantial changes in the atmosphere will lead to harmful modification of the climate everywhere on earth, some data do exist showing local climatic variations that are the result of human actions. These data have produced a deepening concern within the scientific community and a widespread reflection of this concern in the news media. The problem has been summed up in the following statement by a panel of experts to the Executive Committee of the World Meteorological Organization (1977):

> Other than on local scale, there is no unequivocal observational evidence that human influences of any kind have yet been the origin of unusual climatic behavior anywhere in the world. This is not altogether surprising in view of the high natural variability of climate which makes detection of human impacts difficult. Nevertheless there is no justification on these grounds for complacency about the potentially serious effects that man's activities could have in the future.

At the present time, most of the concern about inadvertent modification of the atmosphere is focused on the five following human activities.

The Emission of Carbon Dioxide (CO_2)

The burning of fossil fuels for energy purposes results in the release of carbon dioxide into the atmosphere, where its residence time is about 1,000 years. Thus for all practical purposes, CO_2 is released into the atmosphere permanently. It is estimated that the CO_2 content of the atmosphere is now 12 percent above the level of the preindustrial era.

It has been estimated, but still not proven, that this increase in atmospheric CO_2 could, if it acted alone, raise the average world temperature an estimated 0.5°C, with an increase of possibly 1°C in the polar regions. If the use of fossil fuels continues to increase, within 50 years the atmosphere might contain twice as much carbon dioxide as it did during the preindustrial era.

Preliminary sensitivity tests on global models suggest that this amount of CO_2 might be accompanied by an increase of approximately 2°C in the average global temperature, with several times this temperature rise in the polar regions. Predictions of CO_2 concentrations beyond 50 years are complicated by the fact that energy constraints will influence the rate at which fossil fuels will be used. Such offsetting factors must be part of any future analysis of this problem. However, it appears that atmospheric CO_2 late in the twenty-first century or early in the twenty-second century may be much more than it is now. This could increase world temperatures with catastrophic consequences.

Some hint of the agricultural implications of this problem can be obtained by examining Figure 1 which was assembled by Kellogg (1977) from the distribution of fossil organisms in ocean sediments and of pollens in lake sediments and other proxy data. It shows schematically the precipitation relative to the present. But, a caveat must be kept in mind: the processes that produced the climate of 4,000 to 8,000 years ago were different from those postulated to occur in the future. Given that caveat, one can note the striking differences between northern and eastern Africa, the Mideast and India, Europe and western Eurasia, and in the breadbasket of North America.

The Emission of Trace Gases

In addition to carbon dioxide, other gases are released into the atmosphere as the results of human activities. The most important of these are N_2O (nitrous oxide), a by-product of fertilizer use; CH_4 (methane), a by-product of both fossil fuel combustion

and fertilizer use; and chlorofluorocarbons, mainly CCL_3F and CCL_2F_2 (commonly called freons), typically released into the atmosphere by aerosol spray cans. All these gases have strong absorption bans in the infrared portion of the energy spectrum, and increased atmospheric concentrations of them also are likely to raise surface temperatures. Preliminary calculations by Wang, et al. (1976) suggest that a doubling of the present amounts of N_2O, CH_3 (carbon tetrahydrate), and NH_3 (ammonia) in the atmosphere would cause increases in the earth's surface temperature of 0.7°C, 0.3°C, and 0.1°C, respectively.

It also has been estimated (NRC 1976b) that continued production of CCL_3F and CCL_2F_2 at 1973 levels would produce a temperature rise of about one-half that likely to be caused by the increase in atmospheric carbon dioxide during the remainder of this century. Of course, it is improbable that we will allow such a temperature rise to occur.

The Emission of Particulates

Various human activities lead to the emission of particulates, or minute solid particles, into the atmosphere. Among the major sources of particulates are the industrial processing of materials, the combustion of fossil fuels, and the clearing of land for agricultural purposes, which results in large quantities of wind-raised dust. Just how much particulate matter in the atmosphere is due to human activities (as opposed to natural processes, such as erosion and volcanic eruption) is uncertain. Bolin (1975) states that estimates of the percentage of atmospheric particulate matter due to human activities cover a wide range.

Whatever their source, particulates in the atmosphere have the effect of altering the reflection and absorption of reflected energy. They also alter cloud formations, to some extent. It is also well known that sulphur dioxide emission leads to the formation of sulfuric acid particles in the atmosphere. These particles are one of the chief sources of local air pollution and may be of regional importance in determining the acidity of rainfall.

Radiation

Charney, et al. (1975) make a persuasive case that excessive grazing by animals on the vegetative covering of the earth has serious effects on the atmosphere because it increases the reflective power of the

earth's surface. With the help of atmospheric models, Charney and his associates concluded that overgrazing, by raising surface reflectivity by a factor of two, led to cloud suppression and a consequent lack of rainfall, perpetuating the serious drought in the Sahel.

Since the reflection of solar radiation is very much influenced by whether it falls on snow and ice or on water, there is a potential for a pronounced positive feedback mechanism to operate near the boundary between ice sheets and open water.

The Transfer of Sensible Heat to the Atmosphere

The release of sensible heat from such sources as power plants into the atmosphere appears to be a lesser problem than those listed above. In the high-income countries, the anthropogenic heat released on a regional scale is only a small fraction of that received on a bright day, although it is higher in heavily populated areas such as New York City and in heavily industrialized areas such as the Ruhr Valley in Germany. A tenfold increase would be required to bring this heat to a point where it would be large enough to influence normal atmospheric processes.

RESEARCH NEEDS

Since current models of atmospheric processes suggest that certain human activities may produce significant changes in climate, the chief research need appears to be the development of models that would permit precise estimates of the ultimate consequences of gaseous and particulate emission into the atmosphere. Current climatic models are not realistic enough to make these estimates. For example, current estimates of the climatic effects of increases in carbon dioxide are made with models that exclude many feedback mechanisms, particularly atmosphere-ocean interactions.

Another research need is to resolve the major uncertainties that still exist in determining global trends of turbidity and the optical properties of assemblies of particles. While arguments have been advanced for global climatic effects from anthropogenic particle emission, the matter still remains open (Mason 1976).

With regard to overgrazing and the possible effects of changes in solar reflection, one research need is to monitor changes in extraterrestrial solar radiation, mainly in the visible and near ultraviolet range of the energy spectrum (World Meteorology Organization 1976).

The chief research need in regard to sensible heat is the development of more refined models to carry out sensitivity studies. Since lead times of 50 to 70 years are desirable in assessing the potential impact of energy-generating and distribution systems, this work should commence soon.

We must have more knowledge about humankind's place in this vastly complex climate picture. Do the earth's living matter, atmosphere, oceans, ice sheets, and land surface constitute a single organism that is capable of responding to external stimuli in a manner that controls the earth's surface temperature, the composition of the atmosphere, the pH of the soil, and so on, within limits that are conducive to an orderly evolution to successively higher forms of life? In philosophical terms, this implies that this complex organism has a central nervous system and an awareness of itself and the rest of the universe. An alternative would be that it is only by random chance that the physical conditions on earth have been suitable for a feat of natural biochemical engineering that is mind-boggling in its complexity.

IMPACT

An increased ability to assess the possible consequences of human activities on the atmosphere would have important implications for the entire world. If it were known without doubt that certain human activities, if continued, would lead to a significant increase in deserts or ice sheets, modifications of our energy and industrial systems would be in order. Given the potential effects of widespread climatic changes, the proposed research cannot be judged by the usual techniques. What we may confront are two alternatives, survival and no survival.

Unfortunately, actual examples to put these problems in perspective are scarce. There is an example, however, of the effects of increased amounts of air particulates from urbanization on rainfall and hail with subsequent effects on agriculture. Braham, et al. (1976) report increases of from 10 to 30 percent above the normal summer level of rainfall in a 2,000-square-mile area in southern Illinois east of St. Louis between 1971 and 1974. The increased rainfall resulted in annual gains of 400,000 bushels of corn and 130,000 bushels of soybeans, as well as an average streamflow increase of 15 percent. Offsetting these, however, was a threefold greater crop loss due to hail in the same areas.

IMPLEMENTATION

Profiles 1 and 2 discussed the implementation of programs for the collection of climatic and weather data and the design of atmospheric models. Inadvertent modification of the climate due to human endeavors has strong similarities to natural atmospheric processes and must be approached in the same way. It is by means of the same data collection processes and atmospheric models that it will be possible to simulate and assess the various anthropogenic variables. The GARP program can make a contribution to such problem analysis whether such analysis involves natural processes or human activities.

Profile 4

PROSPECTS FOR WEATHER MODIFICATION

Improved agricultural practices and modern technology have led to great increases in food production in the high-income nations. So far, however, relatively little has been done to reduce the variability of the weather, particularly during the growing season. Wet springs still cause serious delays in planting, and short seasons due to frosts still diminish crop yields. Hail and strong winds still destroy vegetation, and droughts continue to have devastating effects.

During the last three decades, scientists have been investigating ways in which to modify the weather. The dispersion or, "seeding," of particular substances into certain types of clouds sometimes affects yields of snow and rain. The seeding of certain types of thunderstorms apparently affects the size and therefore destructiveness of hailstones. Theoretical and experimental work also suggests that the destructive power of hurricanes might be reduced by seeding, which reduces peak windspeeds. Some limited evidence suggests that cloud seeding might reduce lightning and the resulting forest fires. Finally, it has been shown that by means of cloud seeding some types of fogs can be dispersed.

RESEARCH NEEDS

Major contributions to increased food production expected from this research are (1) augmentation of rain and snow, (2) reduction of hail, and (3) reduction of hurricane winds. These lines of research can be couched in these objectives:

-- Development of better precipitation augmentation technology. This would require precise identification of the meteorological conditions

under which precipitation can be increased (or decreased) by means of cloud seeding.

-- Development of better technology for reducing hail damage.
-- Development of the technology for reducing the peak windspeeds of hurricanes.
-- Development of reliable knowledge on the social costs and benefits of weather modification technologies so that appropriate legislation and legal interpretations can be made.

To achieve these objectives, basic research in cloud physics must be expanded to supplement the technology. Inadequate understanding of many aspects of cloud and storm behavior has seriously hindered progress in weather modification. A great deal still must be learned, for example, about the interaction of clouds with the air around them and about the formation and growth of ice crystals.

IMPACT

Augmenting Rain and Snow

The seeding of supercooled clouds (clouds composed of water droplets so pure that they remain in a liquid state even at temperatures as low as -20° or -30°C) can cause alterations in the cloud structure and influence the precipitation formation mechanisms.

Evidence exists that appropriate seeding of certain types of winter clouds over mountainous terrain causes significant increases in snowfall. In an analysis of the possible effects of seeding winter storms over the Colorado River Basin, Weisbecker (1974) estimates that in an average year, 2.3 million acre-feet of water could be produced. Weisbecker also states that the cost of this endeavor is significantly cheaper than the cost of water produced by other augmentation methods, such as desalination of saltwater or condensation of geothermal steam.

Recent studies of silver iodide seeding of clouds over a two-winter period in the Ukraine has led to claims that the average snowfall was increased by about 35 percent above the amount of nearby areas that were not seeded. Such results call for additional research to establish firmly the potential advantages of seeding winter storm clouds in agricultural regions with fairly flat terrain.

Rain is of paramount importance in grain-producing areas during the growing season, but the effectiveness of cloud seeding of summer rainstorms has yet to be established. Some cloud seeding has been followed by

increased rainfall, but at other times there has been less rain than expected. Some seeding has had no discernable effect at all.

Nonetheless, recent research indicates that it is sometimes possible to identify cumuliform summer clouds which will produce additional rain if seeded in an appropriate fashion. There is little doubt that individual clouds can be modified; the question is whether rainfall can be increased 10 to 30 percent over an agricultural region some tens of miles in diameter. Experiments in the United States, U.S.S.R., and other countries suggest that a summer rainfall augmentation technology is a distinct possibility.

A major new research program to be conducted over the Great Plains of the United States, currently in the planning stage, should contribute substantially toward improved understanding of the potential effects of summer cloud seeding. The same can be expected from an ambitious international research program on rain augmentation being sponsored and planned by the World Meteorological Organization.

The value of increased precipitation over the grain-growing areas of the United States has been estimated in a recent report by the National Academy of Sciences (NRC 1976a). Mathematical models were used to calculate the effects of a 10 percent increase in rainfall on principal grain crop yields (Table 1).

Hail Suppression

If hailstones falling from thunderstorms were only a few millimeters in diameter, melting would cause them to reach the ground as raindrops or tiny, harmless ice grains. The object of most hail suppression programs is to bring about such a result. Thunderstorms thought to be prospective hail producers have been seeded with ice-crystal nuclei in the hope of producing a great many small hailstones instead of the smaller number of large hailstones that would have occurred naturally.

Scientific evidence demonstrating the effectiveness of these efforts is meager. Major research programs in the United States, Canada, Argentina, and Switzerland have yielded ambiguous answers. On the other hand, research in the U.S.S.R. during the early 1960s convinced scientists in that country that it was possible to reduce hail significantly by firing ice nuclei into clouds with cannons or rockets. Operational hail suppression programs have been carried out over many parts of the U.S.S.R. since 1963, and the program has grown steadily. About 12 million acres of farmlands were under so-called "hail protection" in 1976 (see Sedunov 1974 and Battan 1977). And, it is

Table 1. Calculated increases in average annual production in the United States

Crops	Additional production (million bushels)
Corn	
With a 10 percent increase in average rainfall	38
With a 30 percent reduction in hail damage*	8
Wheat	
With a 10 percent increase in average rainfall	34
With a 30 percent reduction in hail damage*	31
Soybeans	
With a 10 percent increase in average rainfall	18
With a 30 percent reduction in hail damage*	5

*During critical periods of production.

Source: NRC (1976).

claimed that hail damage has been reduced by 60 to 90 percent with very high benefit-cost ratios. The validity of these claims is difficult to assess without an independent test. Such a test is to be conducted in Switzerland over the next few years in a joint Swiss-Italian project using seeding rockets and launchers purchased from the Soviet Union.

Estimates of the value of hail suppression in the United States have been made (Table 1).

Hurricane Modification

It has been hypothesized that it is possible to change the pressure pattern in a hurricane so as to reduce peak windspeeds by heavily seeding the towering clouds outside the peak wind ring. Mathematical models of hurricanes suggest that such a hypothesis is plausible. Two tests conducted by federal government scientists during a hurricane in August 1969 gave encouraging results: peak winds decreased by about 15 and 30 percent for a few hours following seeding. Additional experiments are needed, but a relative scarcity of hurricanes and the need to conduct experiments over ocean areas have restricted seeding opportunities. Prospective tests in the western

Pacific, where hurricanes are relatively frequent, were aborted because of objections from the governments of Japan and the People's Republic of China. More recent plans call for tests over the western Atlantic or eastern Pacific areas.

An analysis of the benefits, costs, and risks associated with hurricane seeding has been made by Howard, et al. (1972). They concluded, on the basis of available knowledge, that hurricane seeding is likely to reduce peak winds by about 15 percent and that this would reduce the probability of severe land damage from hurricanes. They state: "The expected loss in terms of property damage appears to be about 20 percent less if the hurricane is seeded." This conclusion has been questioned by many scientists on the grounds that it is based on inadequate scientific data. Nevertheless, it has served to encourage further research on hurricane modification.

Social and Environmental Impacts

It is difficult to make a complete assessment of the costs and benefits that might result from weather modification. An increase of rainfall in one area may lead to more or less rainfall downwind, with detrimental consequences. Or there may be competing interests in the same general area. Grain and fruit growers may want hail suppressed, while cattle growers may want augmented rain or ice particles on their pastures. Hydrologists might want more snow over western mountains, while environmentalists might be concerned about the increased prospect of avalanches and ensuing harm to humans and animals. Many economic, legal, and social problems are involved in weather modification. If a useful technology is to emerge, it is important that the social aspects be studied and that appropriate legislation be adopted to protect both individual and public interests.

IMPLEMENTATION

The three-decade history of weather modification has been a period of conflict, a blend of unwarranted optimism and unfounded pessimism, and inadequate basic research and field experiments required to illuminate the complex issues that are involved.

Recent literature includes a historical overview (Hess 1974), a state-of-the-art assessment (NRC 1973), an outline of the federal role in weather modification (Domestic Council 1975), a penetrating analysis of institutional deficiencies at the federal level (NOAA

1976), and a discussion of agricultural implications (NRC 1976a). The National Weather Modification Policy Act of 1976 (Public Law 94-490) directed the Secretary of Commerce "to prepare a study on the state of scientific knowledge and technology concerning weather modification as well as the problems impeding the effective implementation of such technology." Included in the terms of the study is a directive to develop a comprehensive and coordinated national weather modification policy based on legal, social, ecological, legislative, and international considerations as well as on scientific and technical factors.

Meanwhile, WMO is undertaking a systematic research and development program intended to assess the feasibility of significantly augmenting precipitation. Designated the Precipitation Enhancement Project (PEP), the program has identified six potential operational sites, and studies are now underway to select a region for the main PEP experiment and to establish a sound experimental design.

Although a great deal has been learned about the scientific aspects of weather modification over the last three decades, progress in the development of reliable technology has not lived up to expectations. A national commitment has not been made to such a goal, and inadequate financial resources have been distributed among many government agencies, including NOAA, the Bureau of Reclamation, the National Science Foundation, the Department of Defense, the Department of Transportation, and the Department of Agriculture. Between fiscal years 1973 and 1976, total federal support for weather modification research decreased from about $20 million to $15 million, which is inadequate both in terms of the difficulty of the task and the potential benefits. Funding for weather modification research will have to be enlarged substantially if significant advances are to be achieved in the next 5 to 10 years.

In order to centralize responsibility for progress in research and application of weather modification techniques, one government agency should be assigned principal administrative responsibility. This agency should work with other government and state agencies, the academic community, and private interests in planning a research program aimed at achieving the research objectives listed earlier. Coordination should be improved, but strong competing programs should be retained.

Profile 5

ADAPTATION OF AGRICULTURAL PRACTICES TO WEATHER AND CLIMATIC VARIABILITY

Successful farming is, to a large degree, the result of appropriate adaptation to weather and climatic variability. Any attempt to improve the world's agriculture must include improved understanding of plant and animal responses to weather and climate. Basic research recommended in the Supporting Papers of the World Food and Nutrition Study on breeding crops and animals to withstand greater meteorological stresses will help farmers adapt their farming systems to weather and climate (see specifically the reports of Study Teams 1 and 2). Unfortunately, previous adaptations to weather and climate have come indirectly or by happenstance since the basic research objectives were directed at other problems. The research recommended in this profile concentrates directly on the adaptation of agriculture to weather and climatic variables.

Climate and weather research interacts with agricultural research at several points. To date, however, very little of either research has concentrated on these interactions which are significant in reducing world hunger and malnutrition. A recent NAS study, Climate and Food, Climatic Fluctuation and U.S. Agricultural Production (NRC 1976a) speaks directly to this issue. The detailed research recommendations in that study should be strongly supported, and this profile and the next one are in some ways merely illustrations of the research needs documented in that study.

RESEARCH NEEDS

What is needed is fundamental research on biological responses to variable weather conditions. This research would establish the basic relationships affecting the adaptation of specific crops and farm

animals to heat or moisture stresses, risk, and management of resources. Much of this research would be an expansion of ongoing biological or related research covered by the other study teams, but some of it will be unique, dealing with environmental stresses caused by climatic conditions alone.

This research would be based on bioresponse models which provide vital input for the management information system discussed in Profile 6. However, the two topics can be developed separately, with work on each proceeding concurrently.

IMPACT

Perhaps farm management would have the greatest use for these models in decisions such as time of planting, rate of planting, water management, fertilization levels, weed control, and harvest activities. Estimates of yield derived from the models would be particularly useful to government officials.

The probability of success in determining the biometeorological responses of plants and animals is high, since the disciplines of plant and animal physiology are already well established. It is anticipated that imaginative interdisciplinary research aimed at closing present gaps in our knowledge of biological response would result in substantial model improvements within three to five years. Complex multilevel models that would adequately simulate plant and animal responses could be developed in about a decade. This could be done in either of two ways: (1) by dedicating present controlled-environment research facilities to the study of a single crop or animal for extended periods of time in order to obtain a wide range of biological responses, or (2) by building new specialized controlled-environment facilities.

These complex models could be used to define areas where further research in biometeorological response is needed. They also could be used to indicate ways to maximize (or minimize) the interactions between meteorological factors and biological responses.

IMPLEMENTATION

Such variable meteorological factors as temperature, precipitation, and solar radiation are necessary to perpetuate biological systems. Until now, research has concentrated on statistical links between meteorological variables, usually averaged over monthly periods, and biological responses, generally in terms of marketable yield. While this approach provides a

first-order approximation between weather and yield, the model's applicability is nearly always limited to the geographical area for which the relationships were developed.

Research has begun on the development of strategic multiple regression simulation and individual farm models. These models simulate plant growth and development and estimate production by determining biological responses to inputs of meteorological factors on a daily, and in a few cases, hourly or shorter time basis. They place the biological, meteorological, and economic variables in appropriate relation to each other. This adaptative research should be encouraged. The necessity for improved data gives urgency to the backup analysis for data, as well as for the model building.

These simulation models permit the assessment of short-term environmental stresses so their application will not be geographically limited. In addition, they are very useful for research on both basic and applied problems, as well as in farm management and governmental policymaking.

Construction of these physiological models has been hampered, however, by a lack of suitable biological response information and of weather and climatic variation data. For the most part, model builders have had to rely on empirical generalizations drawn from field responses which have not only been averaged over individual crops or animals but also integrated over time, and thus under varying weather conditions. Since the studies were designed for purposes other than weather adaptation, little or no attention has been paid to the mechanism or environmental factor that produced the response.

Dynamic simulation models of crop growth and animal development, therefore, need to be improved through better understanding of the relationships among basic biochemical, physiological, and morphological processes as well as meteorological and other environmental factors. Among the more important research areas for establishing biological response functions are canopy radiation interception, photosynthesis, translocation, respiration, morphology, phenology, root development, soil-plant atmosphere dynamics, water status, and nutrient intake.

Profile 6

USE OF WEATHER AND CLIMATE INFORMATION BY AGRICULTURE

The need for research on plant and animal response to stress, especially the stresses of weather and climate, was established in Profile 5. This profile outlines research on the problems of adapting and implementing bioresponse information and other data such as weather predictions, for practical purposes. Prospective users of this information, such as farmers and agribusiness managers, need to know how to incorporate this information into their decision making.

RESEARCH NEEDS

Research is needed to achieve three goals: (1) better use of statistical techniques for estimating geographic patterns of weather and climate changes, (2) the development of more sophisticated models which incorporate bioresponse information for handling risks of farmers or managers of marketing organizations (these models would use probability distributions of the cost and benefit factors with respect to the bioresponse information), and (3) increased use of information from models in the management area, as emphasized in Climate and Food (NRC 1976a).

Much of the historical information available deals with weather and climate variability over land, but many weather patterns form over water. The means for studying weather movements and the ability to relate weather probabilities in one area to weather probabilities in another have improved, making such research more feasible.

Research undertaken by other industries has shown how weather predictions can be used in models of risks. Risk models have been used in agriculture, but generally indirectly, through farm planning or in inventory control in marketing organizations. The

dominant emphasis in applying them to farms has been on the level of farm specialization, thus many other variables have come into play. The research proposed here would approach the problem of risk by emphasizing the weather variable directly, rather than using it indirectly.

Research into management methods can reduce both weather-related risks and damage caused by meteorological disturbances. This research would concentrate on how to improve farm management decisions on crop and acreage mixes, pest control, seedbed preparation and planting, interseeding, timing and amount of fertilizer application, and harvesting. Also, it could be used in agribusiness management decisions concerning transportation plans, selling dates, inventory policy, raw material procurement, and pricing policies. Animal production also can be improved by paying more attention to the weather. While most farmers revise their day-to-day management practices according to their expectations of weather changes, better biological response information and more reliable weather forecasting would improve their management practices.

The leadership in this research will rest with the high-income countries, but the results should aid in the management of crops in the developing countries where such crops are highly weather-sensitive. It is esimtated that three-fourths of the potentially arable land in the tropics have limited capacity due to insufficient moisture. This research could aid in the management of these short supplies. Use of meteorology in the tropics has been hampered by insufficient data and research.

IMPACT

The impact of the research proposed in this profile is difficult to estimate. Much of this research deals with how to make use of the basic biological and meteorological research discussed in earlier profiles. Thus how research proposed in other profiles is applied will often depend upon the research recommended and accomplished here. This research also will activate research in other areas.

As mentioned earlier, the potential impact of this research is not limited to farms and marketing institutions in the high-income nations. For example, identification of the "lead indicators" of climate, such as the timing and strength of monsoons, would facilitate many management decisions. The Indian Meteorological Service has made notable progress in monsoon forecasting. It has discovered, for example,

that the mean surface pressure over tropical South America during April and snow accumulation in the Himalayas during May appear to be correlated with summer monsoon rainfall distribution in India (Pearce 1976).

The principal beneficiaries of the proposed research would be farmers and ranchers, those whc supply goods or services to farmers, and those in government agencies concerned with policy decisions. Benefits also would accrue to those engaged in transportation, distribution, and processing of agricultural products. Ultimately, the consumer should benefit, directly and indirectly, as increased efficiency in farm management results in steadier food supplies and greater stability in food prices.

Meteorological services can open up many supporting lines of research, as outlined in Climate and Food (NRC 1976a):

> Meteorological services in less developed countries are not now as important as they might be, but they have great potential importance for specific areas such as (1) determining the proper climatic profile for plant breeders to breed varieties adapted tc the longer-term climate rather than to the time scale of plant development; (2) delineating potential new areas for production adapted for specific crops (phytoclimatology); (3) managing diseases and insects by both institutional and individual methods such as seeding and harvest dates or denial of pest habitat; (4) determining the appropriate carrying capacity for native ranges and forests; and (5) perhaps most importantly, improving forecasts of the beginning and ending of dry and wet seasons that determine seeding and harvesting dates in many of the less developed countries.

According to Pearce (1976):

> In nearly all tropical countries the main justification for the existence of a meteorological service has been the suppcrt cf aviation needs in compliance with international air transport regulations and this has determined the level and direction of funding of the service and, indirectly, the level of meteorological research in universities and other institutions. Developments in agriculture and water resource planning do not, in general, result in an

appropriate increase in funding of the meteorological service and other institutions to support research (or even operational requirements) into the use of meteorological information in these activities.

In general, there seems to be too little local effort devoted to:

(a) studies of water vapour budgets, water balance and soil water regimes and their integration into local agricultural and hydrological programmes, analysis of rainfall distribution in space and time;

(b) the structure of the main rain-producing systems affecting particular areas and their relationship to larger scale systems such as monsoon circulations and subtropical anticyclones (this could yield information of value in medium range prediction); and

(c) forecasting techniques, particularly beyond a day or two.

The areas (a), (b) and (c) listed above are those in which research is most appropriately carried out by individual countries; indeed it would not be practicable to carry out this research in any other way, since the kinds of investigations involved often require access to and maintenance of field facilities and coordination with local agencies. Research techniques are available which can usefully be applied without access to very large computer facilities so that large capital investments in such facilities in each individual country are not necessary.

The overall level of impact during the next 15 years is difficult to determine because of the complexity of this research and the difficulties in adapting the research. Even if improved and more timely information and guidance are made available, farmers and other users may choose not to use it for a variety of reasons. Certainly a strong educational program for farmers and other users is necessary. Considering the complexity of this research as opposed to its potential benefits for agriculture, the net effect during the next 15 years can be expected to be at least moderate. This research may help get research that is already underway adapted, which could make the research extremely high in returns.

IMPLEMENTATION

The proposed research is mainly an extension of the research on models for prediction and measurement of historical variability of climate discussed in Profiles 1 and 2. That research includes the collection of a backlog of data, both actual current weather and climatic variations and proxy data from sediments and glacier cores. After data are collected, probability distributions of expected climatic variations in one area can be joined with those from another area to predict events either in the second area or in a third area.

Simulation techniques and lag models enhance the use of the techniques used to measure historical variability of climate. The improved seasonal prediction models should directly aid research on the more comprehensive prediction models.

Research into risk management could be patterned after that used in other industries. Risk models could be incorporated into linear programming and budgeting models used for overall farm and agribusiness planning and special emphasis would be placed on the level of farm specialization. However, the complexity of agriculture will make this work more complicated than in most other industries.

J. C. Thompson has developed a well-known model which illustrates the risk management approach used in the construction industry (Godske et al. 1957) (see Appendix B). By using weather probability data for a series of concrete pours, a hypothetical company was able to reduce costs of protection against rain by more than 70 percent. The analysis used assumptions such as: $400 would be needed to protect the concrete against damage from rain on any given day; and (2) rain of more than 4mm in a given 36-hour period would result in $5,000 damage to the concrete.

The proposed research on management has been detailed in Climate and Food (NRC 1976a) and the implementation procedures given there are endorsed. This research would be primarily adaptive, an extension of the large amounts of ongoing and planned research, especially on crop stress. Models for applying probability statements to management problems are available, although substantial "fine tuning" of plant and animal response to weather and climate is required.

This research must be coordinated with research on water management such as publicly financed irrigation projects, water catchment projects, wells, and desalination efforts. (See NRC 1976a and the report of Study Team 4). This research also must be coordinated with research on expanding both central and local on-site food storage facilities to reduce waste in food

marketing and distribution. (See NRC 1976a and the reports of Subgroup A, Study Team 1, and of Study Team 6.)

To support this overall research program, it is essential that national and worldwide climatological data bases be further developed. Weather information needed for various agricultural areas should be identified. We recommend:

-- nationwide U.S. information programs, including an expanded National Weather Service Program, to provide farmers with better weather and climate information;
-- assignment of a meteorological specialist to the staff of the U.S. Cooperative Extension Service office in each state.

For each farming system and for each crop, timely weather and climatic data should be made available in concise and easily comprehensible form. Measures of variability and contingency tables also should be presented. Examples of useful agricultural weather information not now generally available include soil moisture levels, soil and air temperatures and precipitation distribution and intensity, as well as "normal" data, whether it is daily, weekly, monthly, or annually.

This entire program must be well coordinated among all agencies and must keep the final users in mind. Training programs must be expanded to provide adequate numbers of highly qualified people to carry out these activities in both the high-income and the developing countries. The training needs of agricultural meteorologists in the tropics is critical since most available meteorologists have been trained for other purposes. Some training programs are evolving such as the East African Institute for Training and Research in Nairobi, but others are needed. Some of this may be special training needed for particular adaptative research as noted in Climate and Food (NRC 1976a).

Resources needed for successful implementation of better meteorological services include the education of agrometeorologists and climatologists to the use of satellite and synoptic data available from nonnational sources to help determine certain critical changes such as the beginning and end of the dry season. The capital required to use this information is relatively small and is mostly for radio transmission or reception equipment from existing meteorological networks.

An improved information system in the United States would have worldwide benefits and would be based on existing facilities, personnel, and communication networks. This basic framework must, however, be expanded and regionalized for maximum impact.

The development period would have three phases. The first phase (two to three years) would emphasize collection and consolidation of existing meteorological data, analysis and establishment of benchmark statistics, and creation of interactive interpretive models. Existing facilities, such as those of NOAA, state, and academic institutions, would be used. The second phase (one to two years) would include a pilot project covering one agricultural region. In the third, or operational, phase, 12 regional centers would be established in the United States.

Appendix A

SOME CHARACTERISTICS OF THE CLIMATIC SYSTEM

Here we include precise definitions of several terms used in this report.

Weather:

The definition used for weather in this report is consistent with ones used in prior NAS reports. Another definition is found in The Glossary of Meteorology of the American Meteorological Society (1959):

> The state of the atmosphere, mainly with respect to its effects upon life and human activities. As distinguished from "climate," weather consists of the short-term (minutes to months) variations of the atmosphere. Popularly, weather is thought of in terms of temperature, humidity, precipitation, cloudiness, brightness, visibility, and wind.

Climate:

> ...the average (together with the variability and other statistics) of the complete set of atmospheric, hydrospheric, and cryospheric variables over a specified period of time in a specified domain of the earth-atmosphere system. The time interval is understood to be considerably longer than the life span of individual synoptic weather systems (of the order of several days) and longer than the theoretical time limit over which the behavior of the atmosphere can be locally predicted (of the order of several weeks). We may thus speak for example, of monthly, seasonal, yearly, or decadal climatic states (NRC 1975).

Climatic Variation:

> ...the difference between climatic states of the same kind, as between two decades. We may thus speak, for example, of monthly, season, yearly, or decadal climatic variations in a precise way. The phrase "climatic change" is used in a more general fashion but is generally synonymous with this definition (NRC 1975).

Climatic Anomaly:

> ...the deviation of a particular climatic state from the average of a (relatively) large number of climatic states of the same kind. We may thus speak, for example, of the climatic anomaly represented by a particular January or by a particular year (NRC 1975).

One climatic state is related to another climatic state through climatic processes. Climatic processes can be described in terms of interactions (including positive, or reinforcing, and negative, or dampening, feedback mechanisms) among components of a climatic system consisting of the atmosphere, the oceans, the cryosphere (land ice and snow, sea ice), the biosphere, and the lithosphere. These components form the "internal" climatic system, which is global in extent. Thus changes in one part of the globe are almost always related to changes in other parts, but they are not necessarily of the same type, magnitude, or direction. Climate also may be influenced by "external" forces, such as (1) variations in the characteristics of the sun, the primary energy source; (2) orbital parameters of the earth, including the tilt of the earth's axis; (3) volcanic eruptions; and (4) orographic features. Human activities that may produce climatic changes, such as the emission of gases, particulates, and sensible heat, can often be treated, at least initially, as external modifications.

Three additional characteristics of the climatic system may also be of interest. "Adjustment time" describes the rate at which an adjustment occurs to changing external forces. For the atmosphere, adjustment time is about one month. This is inferred from the march of the annual temperature cycle, which is delayed about one month relative to the annual march of solar radiation. For the surface layer of the ocean, adjustment time varies from a few months to a year, while the adjustment time of the deepest layer of the ocean (below the thermocline) is on the order of

1,000 years. For the surface of the earth (i.e., vegetation and surface characteristics), as well as for sea ice, adjustment time varies between years and decades. For ice sheets, such as those found in Greenland, adjustment time varies from 1,000 to several thousand years.

A second characteristic, the geographic extent of climatic changes, falls into three categories: (1) local climatic changes, which extend over an area of from 10 to 100 kilometers; (2) regional climatic changes, which have dimensions of up to several thousand kilometers; and (3) global climatic changes (which may be the sum of diverse regional changes), embracing the entire earth's surface.

Finally, the scale of climatic variation, if one uses temperature as a key indicator of climate, falls into three categories: (1) those that occur over an interval of about 100 years--tenths of a degree Centigrade; (2) those that occur over thousands of years--1° to 3°C; and (3) those that occur over intervals that vary from tens of thousands to hundreds of thousands of years--5° to 10°C.

Appendix B

EXAMPLE OF THE USE OF WEATHER DATA IN A RISK MANAGEMENT MODEL FOR THE CONSTRUCTION INDUSTRY

A hypothetical Los Angeles construction company is engaged in making a series of concrete pours during the winter months. The company finds that it will cost about $400 to protect the concrete each time, but that damage of $5,000 will result if rainfall exceeding 4 mm occurs within 36 hours of the time of pouring. Accordingly, to minimize the cost of the entire operation, the concrete should be protected if the ratio of the cost C to the contingent loss L is less than the probability P of rainfall greater than 4 mm.... Thus, in this case,

(1) $P(\text{rain} > 4\text{mm}) > C/L = 400/5000 = 0.08.$

The usual categorical forecast is therefore not the most valuable prediction in this case, since it is aimed at providing a forecast to suit the "average" user and is quite properly based (either subjectively or objectively) on P's being greater than 0.50, or

(2) $P(\text{rain} > 4\text{mm}) > 0.50.$

Consulting [prior data,] we see that the inequalities (1) and (2) are satisfied for any value of W_f exceeding 4.1 and 6.6 respectively. If the contractor should accept (2), the categorical forecast, as a basis for his decision whether to protect his work or not, it is evident that he would not protect his concrete often enough. In order to make this point clear, the following comparison has been made of the cost of carrying on this operation throughout the winter season (1

October 1949-31 March 1950) by several alternative procedures. Here, for the sake of simplicity, it is assumed that the contractor operates every day during the period (182 days).

	Total cost plus loss
Case I. The contractor obtains no forecast at all and takes protective measures. Since there are 17 days with rain exceeding 4 mm during the period, the loss is $5,000 per day for 17 days.....	$ 85,000
Case II. The contractor obtains no forecast but takes protective measures every day. He thus sustains no loss, but the cost is $400 per day for 182 days.....	$ 72,800
Case III. The contractor uses climatological expectancy as a basis for the operation, taking protective measures only during the period when this expectancy is greater than 0.08. For Los Angeles this requires protection from 11 December to 25 March and no protection before or after that period. In this case the contractor would take protective measures on 105 days and would sustain a loss on 4 days.....	$ 62,000
Case IV. The contractor uses "persistence," i.e., takes protective measures on all days following a day with measurable rainfall, and provides no protection on other days. This would require protection on 31 days and the contractor would suffer loss on 5 days.....	$ 37,400
Case V. The contractor obtains a categorical forecast designed for the "average" user and thus based on equation (2) above. Using the objective probability	

estimates, this would require protective measures on 19 days, and the contractor would suffer loss on 5 days.....$ 32,600

Case VI. The contractor obtains a forecast designed for his particular operation and thus based on equation (1). Using the objective probability estimates, this would require protective measure on 35 days, and the contractor would suffer loss on 2 days.....$ 24,400

An inspection of these figures reveals that, although the contractor using the "average" forecast, Case V, would reduce the total cost of the operation below that for an operator using no forecast at all, or using climatological expectancy or persistence, the least total expenditure would result from the use of the probability forecast. This illustrates the advantage of the probability estimate and reveals the danger in any categorical forecast where the user is not provided with, or does not make use of, a measure of the reliability of the prediction. In the above example, only the objective probability forecasts have been considered, although probability estimates might also be made by the forecaster from a subjective evaluation of the meteorological situation. For some purposes where adequate numerical techniques are not available, such subjective probability forecasts undoubtedly could be used to good advantage (Thompson 1950).

NOTE

1. This report emphasizes general climatic research and related implications of such research for agriculture. The report of the National Academy of Sciences, Climate and Food, Climatic Fluctuation and U.S. Agricultural Production (NRC 1976a), discusses in more depth applied weather and climate research directly affecting agriculture. These two reports complement each other as background for the research recommended in World Food and Nutrition Study: The Potential Contributions of Research (NRC 1977).

REFERENCES

American Meteorological Society (1959) The Glossary of Meteorology. Boston: AMS.

Baes, C. F., Jr., H. E. Goeller, J. S. Olson, and R. M. Rotty (1976) The Global Carbon Dioxide Problem. ORNL-5194, Oak Ridge National Laboratory, Oak Ridge, Tenn., 37830.

Battan, L. J. (1977) Weather modification in the Soviet Union--1976. Bulletin of the American Meteorological Society 58:4-19.

Beltzner, K., ed. (1976) Living with Climatic Change. Conference sponsored by the American Meteorological Society, Canadian Meteorological Society, Geophysical Institute of the National Autonomous University of Mexico, Mexican Geophysical Union, and Science Council of Canada, Toronto, Canada, Nov. 17-22, 1975. (Available from Science Council of Canada, 150 Kent Street, Ottawa, Ontario, K1P5P4, Canada).

Bolin, B. (1975) Energy and Climate. Secretariat for Future Studies, Stockholm, Sweden.

Braham, R., et al. (1976) METROMEX update. Bulletin of the American Meteorological Society 57(3):304-308.

Charney, J., P. H. Stone, and W. J. Quirk (1975) Drought in the Saharas: a biophysical feedback mechanism. Science 187:434-435.

CLIMAP Project Members (1976) The surface of the Ice-Age earth. Science 191:1131-1137.

Dickson, R. R. and J. Namias (1976) North American influences on the circulation and climate of the North Atlantic sector. Monthly Weather Review 104:1255-1265.

Domestic Council (1975) The Federal Role in Weather Modification. A report from the Subcommittee on Climatic Change, Environmental Resources Committee, Domestic Council, Executive Office of the President, Washington, D.C.

Glantz, M. H., ed. (1976) The Politics of Natural Disaster--The Case of the Sahel Drought. New York: Praeger Publishers.

Glantz, M. H. (1977) Value of a long-range weather forecast for the West African Sahel. Bulletin of the American Meteorological Society 58:2.
Hammond, A. (1975) Probing the tropical firebox: international atmospheric science. Science 188:1195-1198.
Hays, J. D., J. Imbrie, and N. J. Shackleton (1976) Variations in the earth's orbit: pacemaker of the Ice Ages. Science 194:1121-1132.
Hess, W. N., ed. (1974) Weather and Climate Modification. New York: John Wiley and Sons.
Howard, R. A., J. E. Matheson, and D. W. North (1972) The decision to seed hurricanes. Science 176:1191-1202.
Institute of Ecology (1976) Impact of Climatic Fluctuation on Major North American Food Crops. (Available from Institute of Ecology, 1315 16th Street, N.W., Washington, D.C.)
International Council of Scientific Unions (1965) First Report of the International Union of Geodesy and Geophysics, Committee on Atmospheric Sciences.
Katz, R. W. and M. H. Glantz (1977) Rainfall statistics, droughts, and desertification. Desertification, edited by R. W. Katz. Boulder, Colo.: Westview Press.
Kellogg, W. W. (1977) Global influences of mankind on the climate. Climatic Change, edited by J. Gribbin. To be published in 1978 by Cambridge University Press, Cambridge, England.
Leith, C. E. (1976) Some Objectives for Climate Research. Report submitted to the Climate Board of the Joint Organizing Committee for GARP of the International Council of Scientific Unions and the World Meteorological Organization.
Mason, B. J. (1976) Towards the understanding and prediction of climatic change. Quarterly Journal of the Royal Meteorological Society 102:433-478.
Maunder, W. T. (1970) The Value of Weather. London: Methuen and Co.
Mayer, J. (1975) Management of famine relief. Science 188:571-577.
Namias, J. (1976) Negative ocean-air feedback systems over the North Pacific in the transition from warm to cold seasons. Monthly Weather Review 104:1107-1121.
National Oceanic and Atmospheric Administration (1976) Fifth Annual Report to the President and the Congress by the National Advisory Committee on Oceans and Atmosphere. Washington, D.C.
National Research Council (1973) Weather and Climate Modification: Problems and Progress. Washington, D.C.: National Academy of Sciences.

National Research Council (1975) Understanding Climatic Change: A Program for Action. Washington, D.C.: National Academy of Sciences.
National Research Council (1976a) Climate and Food: Climatic Fluctuation and U.S. Agricultural Production. Washington, D.C.: National Academy of Sciences.
National Research Council (1976b) Halocarbons: Environmental Effects of Chlorofluormethane Release. Washington, D.C.: National Academy of Sciences.
National Research Council (1977) World Food and Nutrition Study: The Potential Contributions of Research. Washington, D.C.: National Academy of Sciences.
Pearce, R. P. (1976) Research Priorities and Institutional Requirements in Tropical Meteorology Related to Agriculture and Hydrology. Special Consultative Report to the World Bank.
Sedunov, Y. S. (1974) Weather by order. Izvestiia, Feb. 23, 1974, p. 5.
Thompson, J. C. (1950) A numerical method for forecasting rainfall in the Los Angeles area. Monthly Weather Review 73(7):113-124.
U.S. Senate (1976) Report from the Committee on Commerce Accompanying S. 3383, the National Weather Modification Policy Act of 1976. Submitted May 13 as Report No. 94-859.
Wang, W. C., Y. L. Yung, A. A. Lacis, T. Mo, and J. E. Hansen (1976) Greenhouse effects due to man-made perturbations of trace gases. Science 194:685-690.
Weisbecker, L. W. (1974) Snowpack, Cloud-Seeding, and the Colorado River: A Technology Assessment of Weather Modification. Norman, Okla.: University of Oklahoma Press.
Wilson, C. L., W. H. Mathews, W. Kellogg, and G. D. Robinson (1971) Inadvertent Climate Modification: Report of the Study of Man's Impact on Climate (SMIC). Sponsored by the Massachusetts Institute of Technology, The Royal Swedish Academy, and the Royal Swedish Academy of Engineering Sciences. Cambridge, Mass.: MIT Press.
Von Neumann, J. (1960) Some remarks on the problem of forecasting climatic fluctuations. Pages 9-11, Dynamics of Climate, edited by R. L. Pfieffer. New York: Pergamon Press.
World Meteorological Organization (1976) Weather Modification Programme. WMO Bulletin 25:103-104.
World Meteorological Organization (1977) Technical report by the World Meteorological Organization Executive Committee Panel on Climatic Change. WMO Bulletin 26:50-55.

BIBLIOGRAPHY

Advisory Committee on Weather Control (1957) Final Report of the Advisory Committee on Weather Control. Washington, D.C.: U.S. Government Printing Office.

Bryson, R. (1976) Statement to the Subcommittee on the Environment and Atmosphere. Committee on Science and Technology, U.S. House of Representatives, May 26, Washington, D.C.

Domestic Council (1974) A United States Climate Program. A report from the Subcommittee on Climate Change, Environmental Resources Committee, Domestic Council, Executive Office of the President, Washington, D.C.

Gasser, W. D. (1976) World Climate Changes and Agriculture--The Issues. Presented at a Symposium on Living with Climate Change, sponsored by the Mitre Corporation, Aspen Institute for Humanistic Studies, and American Meteorological Society, Nov. 9-11, 1976, Reston, Va.

Hare, F. K. (1976) Climate Change, Food Production, and Interstate Conflict. A report of a conference held at the Bellagio Study and Conference Center, June 4-8, 1975, sponsored by the Rockefeller Foundation, New York.

International Council of Scientific Unions (1967) The Global Atmospheric Research Programme (GARP), Report of the Study Conference held at Stockholm, Sweden, June 20-July 11, 1967, sponsored by the ICSU/IUGG Committee on Atmospheric Sciences, the ICSU Committee on Space Research, and the World Meteorological Organization.

Joint Organizing Committee of WMO-ICSU (1973) Report of the Eighth Session of the Joint Organizing Committee for GARP, March 14-19, 1973, London. (Available from the World Meteorological Organization, Geneva, Switzerland.)

Joint Organizing Committee of WMO-ICSU (1975) The Physical Basis of Climate and Climate Modeling. Report of the International Study Conference, July 19-August 10, 1974, Stockholm, Sweden. GARP Publication Series No. 16 (Available from the World Meteorological Organization, Geneva, Switzerland).

Joint Organizing Committee of WMO-ICSU (1976) Report of the Twelfth Session of the Joint Organizing Committee for GARP, June 24-30, 1976, Nairobi, Kenya. (Available from the World Meteorological Organization, Geneva, Switzerland).

Manabe, S. and R. T. Wetherald (1975) The effects of doubling the CO_2 concentration on the climate of a general circulation model. Journal of Atmospheric Sciences 31:3-15.

National Oceanic and Atmospheric Administration (1976) Weather Modification Activities for Calendar Year 1975. U.S. Department of Commerce, Rockville, Md.

National Research Council (1971) Atmospheric Science and Man's Needs: Priorities for the Future. Washington, D.C.: National Academy of Sciences.

Thompson, L. M. (1976) Weather variability, climatic change, and grain production. Science 188:535-541.

Upensky, J. (1937) Introduction to Mathematical Probability. New York: McGraw-Hill.

U.S. Congress, House (1976) Weather Modification Policy Act, Public Law 94-490. Congressional Record, September 20. Washington, D.C.: U.S. Government Printing Office.

Digitized by Google

ISBN 0-309-02726-8