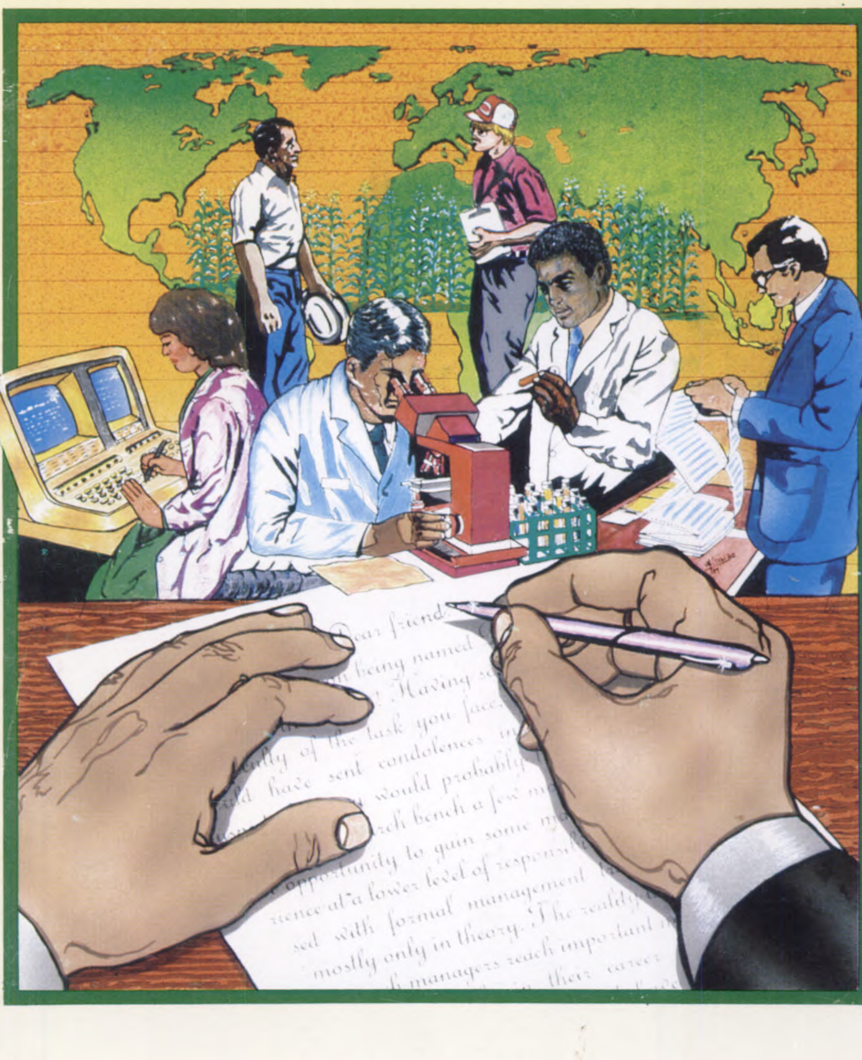
FOR DEVELOPMENT

OPEN LETTER TO A NEW AGRICULTURAL RESEARCH DIRECTOR



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TO EVY
Understanding wife; helpful partner
in our life and work around
the world

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PART TWO: A RESEARCH STRATEGY FOR SUSTAINABLE DEVELOPMENT WITH EQUITY

FOREWORD BY AMIR MUHAMMED*

I take great pleasure in writing this foreword, as the subject of agricultural research management in developing countries is so dear to me. Dr. Nickel's book is in the form of a long, friendly letter written by an experienced research manager to a would-be research director in a Third World country. The material is presented in an organized manner to aid research managers in the developing countries. Having been involved in developing research institutions and the national agricultural research system in Pakistan and in grooming upcoming research directors, I can immediately appreciate the value of this book. It is timely, perceptive and lucid, aimed to address the complex issues facing new research managers who often lack exposure and experience. The material is especially designed to help the newcomer. With the sensitivity and perception shown by the author, it has equal value for more experienced managers and is certainly is a valuable addition to the scarce literature on agricultural research management in the Third World.

An important outcome of the "Green Revolution" was the respectability it gave to applied agricultural research among government leaders in the developing countries and international organizations. The dramatic success of the seed-fertilizer technology helped in developing confidence in the ability of agricultural scientists to solve national and global food shortage crises. Many developing countries, especially in Asia and Latin America, were able to increase food production and reduce reliance on imports through use of improved technology. Simultaneous emphasis on improving food security management has resulted in reduction of widespread food shortages and famines.

The success of the sixties attracted attention from leaders of developing countries to develop agriculture research on a priority

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basis. This also made careers in agriculture research attractive to talented young scientists. The donor community and international agencies like FAO, UNDP, and the World Bank also began giving priority to investment in agricultural research. Strengthening of agricultural research systems in the developing world is now considered an integral and important part of the total development strategy. The payoffs from such an investment are becoming evident as countries move toward self sufficiency through better resource management and well-conceived research and development programs.

The international community and national agencies have focused on the development of National Agricultural Research Systems (NARS). Development of infrastructure such as new laboratories, field equipment, library facilities and support services has received priority. Training of younger scientists is a major activity of all research systems. Besides formal graduate training, several international centers like the International Rice Research Institute (IRRI) and the International Wheat and Maize improvement Center (CIMMYT) have pioneered much of the informal on-the-job training. The other international centers developed after IRRI and CIMMYT also include this as an important component of their activities. Judicious government policy, research support and dedication of researchers and farmers in many countries have resulted in major production increases. These economic benefits have often triggered development in other sectors. In particular, the agribusiness sector has largely developed because of the productivity gains captured through research-led technology development.

Dynamic changes experienced in the sixties and seventies also resulted in rapid expansion of research facilities, at times with little forward planning. Concern is now expressed about the total impact of investment in agricultural research. Some feel the payoffs have not been commensurate with the investment. Policy makers in developing countries feel a key element overlooked in research planning has been the uplift of management capabilities in agricultural research. Despite adequate research facilities, the lack of trained research managers has resulted in low research output, ill-conceived research plans and reduced credibility of research input to development. Often senior scientists close to retirement age with little or no experience in management are appointed as Research Directors. Seniority and research accomplishment are rarely the sole attributes of a successful research manager, especially if the person lacks the experience and training in research management.

Steps have been taken to upgrade research management training. The Consultative Group for International Agricultural Research (CGIAR) established a new institution ISNAR –International Service for National Agricultural Research– to help strengthen research management in developing countries. Countries with more developed research systems have also contributed by providing training facilities and sharing experience. Despite such efforts, the manpower trained abroad is poorly equipped to handle management problems at home.

Research administrators frequently complain that although scientists sent abroad for training in management are reasonably well trained in technical aspects of research management, they often lack the sensitivity and leadership skills essential to head national institutions and programs. Often those who serve as trainers are strongly biased to developed country situations. Even the course material is designed around case studies alien to the students' environment back home. The trainers are usually unaware of the attitudes of administrators in the trainee's country, knowledge about financial and administrative procedures and, above all, the role of culture in shaping the value system that determines sustained intellectual development and research efforts. Thus, managers trained abroad through formal courses and special assignments often produce less than desired results on jobs back home.

Dr. Nickel's book is sensitive to the special needs of the research managers in developing countries. It deals with aspects not adequately covered in conventional text books on research management. While numerous books and training courses are available on personnel management, financial management, procurement of scientific stores and research station management, they are not suited to the cultural environments and value systems in developing countries. This book has the special merit of addressing at length research management issues related to the value system of a research institution, since research productivity is closely related to the culture of the institute. The author also stresses the need to differentiate people based on occupation, for example, research scientists, administrators and support staff, since no single role model can be applied to all staff categories.

I believe that an important aspect of successfully organizing research in developing countries is the ability to manage the political system. Unfortunately, the priority of the typical politician is to use the research system to strengthen his/her own constituency, often by lobbying for jobs in the research system. Their interests are limited to getting relatives or friends appointed to the scarce jobs, regardless of the qualifications. Few political leaders understand or appreciate the need for agricultural research and its specialized requirements. If the research manager is poorly prepared to handle the political maneuvering common to many research systems, such a result is a tremendous set-back to the institution, with consequent low morale among scientists and other staff.

The author has dedicated this book to the newly appointed research director with little training or experience in research management. However, there are several levels of research organization in a country, from research policy formulation to actual carrying-out of research at institutes and experiment stations and utilization of the research results by farmers. Research policy is formulated in the government ministry of agriculture or science and technology, or in the national agricultural research council. Effective participation at the policy formulation levels requires different management skills than customarily needed at the research institute. While the management principles outlined in the book are intended for directors of research institutions, individuals concerned with research policy formulation and administration at the national level may also find the material useful.

Research management is a dynamic discipline, responding rapidly to changes in the social, economic and political environments. The role of information management is becoming increasingly important. Therefore, the modern research manager must acquire the necessary computer skills to effectively do the job.

The author has done a commendable job of effectively communicating his ideas to the reader. Dr. Nickel conveys lessons learned from twenty-two years of research management, hoping that many common pitfalls can be avoided by sharing his own mistakes. His purpose is to inspire confidence in the new research leader by facing challenges and assuming responsibility. He realizes that while skills in several components of research management, for example, personnel, finance and procurement, are easy to grasp, developing a management philosophy is where most young managers have to struggle. Nickel's book has plenty of recipes to help both the novice and experienced learn the tricks of the trade. I found the quotations selected from great leaders appropriate and inspiring.

The book is highly educative and informative. I strongly recommend its use in formal training and orientation courses for research managers. This book should also find space in the shelves of experienced managers as a handy reference.

ACKNOWLEDGMENTS

This book is a personal view of research management drawn from my own experiences and observations during fifteen years as a practicing scientist and twenty-one years as a scientific administrator. Thus, grateful acknowledgments are due to the various people under whom and with whom I have worked, whose management practices taught me many lessons. Special thanks are due to those who put up with the many mistakes I made, both as a young scientist and as an inexperienced research manager. One of the principal points made in this book is that good leaders regard mistakes as an opportunity for learning rather than a failure. In this regard it is most appropriate here to give special recognition to my first employer, the late E. J. Peters, President and owner of Maple Leaf Enterprises, Inc.. As an entomologist, fresh from the university and filled with enthusiasm for the concepts of integrated pest management, I opposed the common practice of spraying potato fields beginning at a certain date to protect them from tuber moth. Instead I recommended that no insecticide be applied until my observations demonstrated that the adults and/or eggs of the pest were present. The idea was sound, but the necessary technology for detection and information on economic population levels was not adequately developed. As a result, forty acres of potatoes had to be plowed under. Rather than fire me for this costly error, Mr. Peters soon afterward promoted me. It was an important step in my learning process for which I have always been grateful.

I also wish to acknowledge the vision and encouragement of the Late Dr. Sterling Wortman, who first launched me into a management career. It was Dr. Wortman who recommended me as Dean of Agriculture in the University of East Africa when that University sought help from the Rockefeller Foundation to fill that post. At that time I was a thirty-six-year-old Assistant Entomologist in the University of California with no experience or formal training in academic or research management. As the Foundation's Director for

Agricultural Sciences, Dr. Wortman offered me an appointment with the Foundation and accompanied me on a successful interview visit with University officials in Uganda. He encouraged me to "work myself out of a job" by identifying and training appropriate local staff and, when that was accomplished, transferred me to another management position in the newly-founded International Institute of Tropical Agriculture in Nigeria. Now, with hindsight and long experience in recruiting staff, I marvel at the boldness of his action in identifying someone he felt had the potential of becoming a good administrator and giving him the opportunity to develop that potential. It is the type of bold risk-taking that characterizes the best leaders.

Although the views expressed here were formed over many years, this book would probably not have been written if I had not had the opportunity to read and reflect on the subject. This opportunity was made available when the Board of Trustees of CIAT generously gave me a six-month study leave, for which I am deeply grateful. The last month of this period was spent as a Resident Fellow at the International Study and Conference Center of The Rockefeller Foundation at Bellagio, Italy. It was during this month that the first draft of this book was written. I am indebted to the Foundation for this fellowship and to the staff of the Villa Serbelloni who did so much to make my stay there so productive and enjoyable.

I am also grateful to the many colleagues who provided information and advice during the preparatory phases, and who commented on all or parts of the first draft of the manuscript. Special thanks are due to Fritz Kramer for his helpful advice and encouragement throughout the project; and to Trudy Brekelbaum, Nyle Brady, James Cock, Matthew Dagg, Dana Dalrymple, Lowell Hardin, Amir Muhammed, Gustavo Nores, Alexander von der Osten,, Joaquin Paez, Aart von Schoonhoven and Fred Wangati, whose written comments on the first draft were very helpful in revising the manuscript. While I benefitted greatly by their criticism, comments and suggestions, the decision as to which suggestions to accept was mine; so I take full responsibility for the contents of the book, including the errors which no doubt remain and the improvements which could have been made had time permitted expansion of certain portions. Unfortunately, the study leave is over; and if there is one thing certain about full-time research managers, it is that they don't have time to write books. Finally, I wish to thank Gloria de Escobar for her patient efforts in the re-processing of the successive revisions.

PART ONE

RESEARCH MANAGEMENT

CHAPTER I

INTRODUCTION

Dear friend:

Congratulations on being named Director of your research institution! Having some idea of the difficulty of the task you face, perhaps I should have sent condolences instead. I do suspect that you would probably have liked to stay at the research bench a few more years, and an opportunity to gain some managerial experience at a lower level of responsibility, or at least take a management course would have been desirable. But there it is; if we accept, as I certainly do, that the heads of research institutions should be scientists, than we must also accept the fact that at some time in their career some persons will have to give up their career as scientists and take up a new career as research managers. The problem is that the ideal career path, with the future manager exposed to a series of management circumstances of increasing responsibility, interspersed with formal management training, exists mostly only in theory. The reality is that many research managers reach important management positions early in their career without such experience or training. I have made this an "open" letter in order to share it with the many who find themselves this position.

The promotion of young, relatively inexperienced scientists to management positions is particularly common in developing country institutions, where shortages of highly trained scientists result in the first persons in the country with a doctorate finding themselves named to important executive positions soon after returning home. But they are not alone; scientists around the world have to learn management by "on-the-job-training." It happened to me twenty-two years ago. The purpose of this letter is to pass on to you some of the lessons I have learned along the way. Many of these lessons were learned by making mistakes; hopefully, this letter can help you avoid some of them. I also learned a lot by observing "good" and "poor" research leaders and management practices along the way. The worst examples were certainly not limited to the Third World-some of the best lessons of how *not to manage* were learned from observing my own countrymen–but the Third World is where the need for effective research management is most urgent. It is in the agricultural research institutions of the developing countries where the responsibilities for excellent research to contribute to the alleviation of hunger and poverty lie most heavily. Because of the imperative urgency of effective research management in the Third World, and because that is where most of my experience lies, this letter is directed chiefly to the specific conditions of developing countries. However, the management principles are universal; consequently I trust that the views expressed and experiences recounted in this letter will also be of use to research managers in developed countries. Certainly agricultural research management around the world needs to be made more effective.

On Being an Agricultural Research Director

The Importance of the Task

A dynamic agricultural sector is central to social and economic growth. This is particularly true in developing countries where the general welfare and farming fortunes are so closely linked. Firstly, in many of these countries a large portion of the population, including some of the most needy, are still rural; so any attempt to improve the income of the poor cannot neglect this sector. Secondly, raising the income of the rural sector is one of the best ways to generate an expanded market for the kind of goods that are produced by local, labor-intensive industries; thus, raising the income of farmers and rural, landless laborers is one of the most important steps toward industrialization and urban employment enhancement. Thirdly, the urban poor, when provided with employment and increased income, will greatly expand the demand for food and fiber; thus, unless agricultural productivity improves, the result will be inflation and increased use of scarce foreign reserves for food imports. Finally, agricultural exports represent one of the most important sources of foreign exchange needed to import the capital goods required for development. Consequently, agricultural development is key to the process of economic development.

While agricultural development is the motor of economic development, new agricultural production technology is the fuel for that engine. In most countries increased production will have to come from increased productivity on existing lands. Until recently, increased production has come from expansion of land under cultivation. That option has been largely exhausted; and where such expansion takes place it is often onto marginal lands, frequently in fragile ecosystems. Intensification of production on existing land through the increased use of purchased inputs is an option frequently unavailable to farmers with limited resources, and can have negative environmental repercussions. The increase in agricultural production so critically and urgently required to meet increasing demands cannot be achieved by a combination of traditional practices and folk wisdom. Traditional systems, while stable, are not sufficiently productive. Thus, the goal of a rapid, sustainable increase in agricultural production depends on technical progress.

In turn, technical progress depends on effective, relevant, local agricultural research. There is a worldwide trend towards science-based agriculture. Several decades ago, it was generally assumed that new agricultural production technology developed for the industrialized, largely temperate agriculture of the developed countries could be transferred to the largely tropical, developing countries. This concept has been shown to be erroneous. On the other hand, it has been amply demonstrated that new production technology developed in and for the developing countries can have a major and highly beneficial impact. To be effective, however, the research aimed at the generation of such technology must be sharply focused on the most urgent, high-priority national needs. At the institutional level, the production problems must be identified and translated into relevant, researchable questions; and the research aimed at answering these questions must be organized into coherent and realistic programs. This will be your task as a research leader.

But the new technology alone will not be enough. It will have to be made available to farmer; the farmers must adopt the new technology; and they must do so within a propitious policy environment. As a national research leader you will be an a good position to influence the national priorities and government policies.

The Nature of the Task

Business Management and Research Management. Like you, I made the change from a hands-on scientist to scientist-administrator at a young age. At that time I was an Assistant Entomologist at the University of California, Berkeley, and was offered and accepted the post of Dean of the Faculty of Agriculture of the the University of East Africa. Having had no previous experience or training in management, I went to the Dean of Agriculture at my university before leaving and asked his advice. One of the things he told me was that being a university dean was different from running General Motors. I found that advice very helpful, because it underlined the need to recognize that one cannot deal effectively with academics and scientists by issuing orders; rather than "directing" one must manage through persuasion and consent. Over time, however, I have come to realize that the similarities between managing a business enterprise and an academic or scientific institution are greater than the differences.

The differences are more a matter of degree than of substance. In all human enterprises–be they in the public or private sector, factory, or university or research institution–one must deal effectively with people. What is different is that in academic and scientific institutions the output is produced by people; in a factory the output is produced by machines run by people. Besides, scientists tend to be more independent and allergic to excessive controls and formal structures, and the nurturing of an environment conducive to creativity takes on greater importance in scientific and academic organizations. The research manager must also learn to deal with a wider range of disciplines.

But there the differences end. Many of the principles of management developed over time for business are relevant and useful; and this has become more so as the nature of business management has changed, and new schools of management have evolved. For example, the much-used concept of "management by objectives," or MBO, stresses participatory management, a concept I have found very useful in managing scientific and academic institutions. The Naisbitt Group (1986), in their book entitled "New Executive Skills for the Information Economy," point out that the nature of leadership in business enterprises is changing. They state that "in the industrial economy, leaders gave orders. In the information economy, leaders are facilitators." Thus, you will find that books on management aimed chiefly at businesses will be very useful to you. Since these are much more numerous than publications about research management, that's a good thing. I have personally found the popular management book, "In Search of Excellence," by Peters and Waterman (1982) very helpful, and recently wrote a paper (Nickel 1988) translating the principles they espouse into the language of agricultural research management.

Of course, there has always been a great overlap between management of the research and development (R & D) aspects of a commercial firm and management of public sector research. Both deal with the management of scientists, and both have to do with producing new products which must eventually be "sold" to end users. In this field, I am sure you will find the excellent handbook, "R & D Productivity" put out by Hughes Aircraft (Ranftl 1978) and the subsequent commentary on this, "Training Managers for High Productivity" by Ranftl (1984), particularly helpful.

A Rewarding Career. There are many scientists who find the whole idea of being an administrator repugnant; they wish to, and should, devote their entire career to doing research. However, those who are inclined to management should, when given the opportunity to serve in this capacity, do so enthusiastically; without feeling that they are forced to do a necessary or unpleasant duty, or regretting that they have left the field for which they studied. They will find research management to be a most rewarding and exciting career. I have found it so, and I trust you will as well.

Several years before I made the switch from scientist to administrator, while I was a Visiting Scientist entomologist at the International Rice Research Institute in the Philippines, the late Dr. Sterling Wortman, then Associate Director of the institute, asked me about my career goals. I told him that some day I would like to become a research administrator. When he asked why, I replied that I thought it would be exciting to be an orchestra conductor; to direct the various instruments to come in at the right moment and have the thrill of "making the music." His response was: "but did you ever think about all the sour notes?" Many years later, when I read various articles about management, I found that the analogy between conducting an orchestra and managing an organization was not original with me; it has been used many times in the management literature. In fact, some have pointed out that the analogy is not quite exact. Leonard Sales (quoted by Mintzberg 1980) perceptively elaborated on this theme: "The manager is like a symphony orchestra conductor endeavoring to maintain a melodious performance in which the contributions of various instruments are coordinated while the orchestra members are having various personal difficulties; stage hands are moving the music stands; alternating excessive heat and cold are creating instrument and audience problems; and the sponsor of the concert is insisting on irrational changes in the programme." I'm sure this rings all too true to research managers; yet, despite these problems and the occasional "sour notes," I have found that under the right conditions teams of scientists produce beautiful "music" in the form of exciting new technology; and to be playing a key role in creating these conditions is truly rewarding. This letter is mostly about how to wave the baton.

Research management is a full-time job. I have known many scientists who, when appointed to an administrative post, tried to keep one foot in each field by attempting to continue to do research at the same time. Very few can pull it off. Usually they end up being either poor researchers or poor managers, or both. I suggest you give your important new responsibilities as a research director all you have got. If it turns out you are not successful, or don't like it, then return to being a full-time scientist. If you do find it something you enjoy and are good at, then decide to make research administration a career. Science is moving at such a fast pace that it is hard to maintain excellence in a discipline on a part-time basis. A good research manager will instead need to learn all he can about a wide range of disciplines, and invest his efforts fully into sharpening his management and leadership skills.

Leadership, Management and Politics. As a research director, you will need to be both a good manager and an inspiring leader. While these tasks are closely related, they are not the same. A leader has vision; a manager gets things done. A leader sees to it that the right things are done; a manager sees to it that they are done right. A leader will see what changes are needed; a manager will see to it that the changes are made. Intellectual leadership is essential to being a good scientist, but many creative people are not very good at getting things done. Organizational leadership involves both vision and execution. The head of a research team must be a good leader and must combine these attributes with good organizational and management skills. It is this combination which I shall have in mind as I discuss "research management" in this letter.

Early business management literature dealt chiefly with what a manager, as I have described above, does. As early as 1918, Fayol listed the five basic functions of management as: planning, organization, command, coordination and control. This is still a quite good description of the purely management functions, but it does not describe the leadership role so essential to inducing the creative environment and motivating highly skilled and sensitive scientific personnel. More recent business management literature, however, does recognize the importance of leadership skills.

Political dexterity will also be important in your role. Coulter (Elz 1983) pointed out that "research managers have many pressures and are pulled in many directions when deciding on priorities: their research workers want to do things that interest them and naturally wish to further their careers; politicians want them to solve the problems of their particular areas, urban dwellers want cheaper food, and the minister of finance wants more export crops or wants new crops to open up a new market or substitute for imports such as bread wheat." You will also, no doubt, be required to juggle the special interests of various donors. Thus, the director of an agricultural research institute needs to combine management, leadership and political skills.

The Challenge to Better Management

In Developing Country Institutions. Your task will be even more complex and demanding because the institution you direct is in a developing country. By definition, this means that you will have to operate in an institutional environment characterized by scarcity of human skills and material resources, with little or no cushion to absorb unsound decisions on priorities or inefficient management. You may also have to deal within a fragile economic structure and weak public administration. The purpose of this letter is to help you improve the management of your institution; but it would be unrealistic to imagine that this can be done in isolation from the overall institutional structure, administrative culture and political system of your country.

Many countries have taken important and useful action to partially buffer agricultural research institutions from institutional weaknesses and political pressures by establishing semi-autonomous institutes or placing the research activities under the umbrella of relatively independent agricultural research councils. The International Service for National Agricultural Research (ISNAR) is working actively to advise national agricultural systems on such valuable organizational mechanisms and has produced useful publications on this subject; I recommend that you contact that organization on this, if you have not already done so, and won't go into further detail in this letter.

Good management and organizational improvements can go a long way towards overcoming the institutional factors which impinge on agricultural research institutions in developing countries; but they won't make them go away completely. I mention this here because I want you to know that I have tried to keep the institutional environment in mind in this letter. I recognize that most of my experience has been with international institutions, which has made my job easier than yours. I know that the real world of developing country institutions is that adequately trained technicians are not always available, the telephones frequently don't work, the electrical power source is irregular, and the funds for the approved budget usually arrive late or are suddenly reduced. I recognize the difficulty of such problems; but I also feel strongly that good management can help overcome their impact. A lot can be done to insure effective and efficient management within your institution; that is what this letter is about.

In National Agricultural Research Systems. By national agricultural research systems (NARSs), I refer to the entire complex of institutions engaged in research of relevance to agriculture. These usually include: a national agricultural research institution and one or more universities in which members of the Faculty of Agriculture and the Faculty of Science are engaged in research. These are supported chiefly from the national budget, usually through the ministries of agriculture and education, or a ministry of science and technology. In addition, many countries have specialized commodity research centers dealing with a single export crop, such as sugarcane, rubber, cacao, and coffee, usually supported by levies imposed on the sale/export of these commodities. Also agricultural research may be conducted by private companies dealing with agricultural chemicals or seeds. Some countries also have basic research institutions doing fundamental research that may have relevance to agricultural science. It is essential that these various efforts be coordinated in a manner which insures maximum complementarity. Such coordination is frequently performed by an agricultural research council, or an agricultural component of a national research council. How these function, and how they can work better in a harmonious manner to contribute to the priority needs of the country cannot be dealt with in detail here because this depends on the specific circumstances of each country. However, I do wish to emphasize one point; that is the importance of better integrating the large, highly skilled, valuable, and frequently underutilized human resources found in the universities.

Too often, there is little or no linkage between the research activities of university staff and national research programs, yet some of the best scientific talent resides in the universities. Because of their prestige, and the need for highly trained specialists to adequately perform the teaching function, a large share of the scientists with advanced degrees are in university faculties. Their talents are often not applied towards solving high priority agricultural production constraints for one or more of the following reasons: 1) they have little or no funds nor adequate facilities for research; 2) they consider research directly aimed at solving practical problems as inappropriate to university staff; and 3) their abilities and advice are not taken into account in the development of national agricultural research plans.

The separation of the universities from the mainstream of applied agricultural research in developing countries is usually a vestige of the colonial history; the colonies inherited from their respective metropolitan countries the concept that applied research is conducted by research institutions under the ministry of agriculture, while universities restricted themselves to basic, or fundamental research. One example of this was the Faculty of Agriculture of the University of East Africa. Makerere College (later to become Makerere University, when the University of East Africa was broken up and each of the three countries of East Africa –Uganda, Kenya and Tanzania– developed its own, separate university), formerly part of the University of London, and was one of the oldest and most prestigious faculties of agriculture in Africa. It had a highly trained staff devoted chiefly to teaching; but they also conducted research, supported very modestly by a small research fund administered by the University Grants Committee. In the British tradition, they took pride in limiting their research to strictly fundamental topics. As an extreme example, I was told, when I arrived there as the first full-time Dean in 1966, that when the new Professor of Agricultural Biology had arrived a few years previously, he had brought with him his own collection of turtle eyes, since his main research area was in their anatomy.

Efforts at Makerere to redirect such talent towards problem-solving research more directly related to the needs of the region were hindered by two factors: 1) the legitimate concerns of "academic freedom," and 2) the fact that it was more expensive to do the kind of field and laboratory research of a more applied nature. There was a growing realization that many research topics could be identified which were sufficiently sophisticated to be of the highest academic value, but which would at the same time generate knowledge of direct relevance to high-priority national needs. This research would, however, cost more than the modest funds available.

It was not difficult to overcome these obstacles. Through a grant from the Rockefeller Foundation, a special fund was established to support appropriate research projects. A Research Committee was established within the Faculty of Agriculture, which reviewed project proposals for the proper combination of relevance to urgent agricultural production problems and scientific excellence. Thus staff were not forced to redirect their research activities. They maintained their academic freedom to do what they wanted with the time they had available for research; but if they wanted financial support to purchase the equipment or hire the technicians and labor needed to do more costly research, they had to demonstrate that the research was relevant. Soon the University Farm and the Faculty laboratories were transformed into one of the most effective research stations in tropical Africa.

But a mechanism had to be found to institutionalize this trend beyond the time when external funds were so readily available. This was possible because at the same time the Uganda Government established an Agricultural Research Council, which had authority over a modest fund, from national resources, to give grants to support worthy projects. Some of these grants were made to University Faculty projects; thus a start was made towards developing a mechanism whereby the human and physical resources of the University could be applied more effectively toward solving important national problems, and the activities of the University were better integrated with those of the Ministry's research institutions, with some division of labor. Unfortunately, subsequent political developments interrupted this progress, but a model was established.

A more recent, and currently operative example of such integration, on a much larger scale, is the Philippine Council for Agricultural and Resource Research Development (PCARRD). This Council does not manage research stations or conduct research; rather it coordinates and funds most of the agricultural research in the Philippines. Individual institutions, including in many cases universities, are given national responsibilities for specific crops or research topics and are provided funds to carry out these projects. Thus the universities are fully integrated into the national research program. For a fuller description of this model I refer you to the publication: "The Making of the Philippine Agriculture and Resources Research System: A Case for the Developing World" (Valmayor 1985). There are other variations on this theme, chiefly in Asia. A good description of the various models can be found in "Agricultural Research Organization in the Developing World: Diversity and Evolution" (ISNAR 1986). Whatever the organizational model might be, it is essential that effective mechanisms be found to assure the most efficient utilization of the available human resources in the various institutions.

In Individual Agricultural Research Institutions. Making NARSs more effective involves not only the organizational issues described above, but also the important issues of establishing national priorities and resource allocations. ISNAR has been established to work with individual countries that request its assistance on these important matters. Furthermore, the agricultural research management literature is replete with learned papers, chiefly by economists, on resource allocation. Therefore, I feel that I have no comparative advantage in dealing with these broad issues. Where I think I may have something to offer, as a practitioner of agricultural research management, is in the "nuts and bolts" of leading and managing an individual research institution. Several years ago, when I started reading in the area of research management, I discovered that there were two kinds of people: those who write about research management and those who do it, with little overlap between the two. The purpose of this "open letter" is to begin to fill this gap. In what follows, therefore, I hope to give some practical advice, based chiefly on my personal experience and observations, of the elements of the day-to-day management of a research institution. The type of institution I have chiefly in mind is the national research institute of a small-to-medium sized country, which usually has a national center and several research stations. These comments should also apply to the management of major research centers in larger countries which have divided their research responsibilities along commodity, ecological or geographic/political lines.

Rising to the Challenge-A Summary

Being the director of a national agricultural research institution in a developing country is at the same time one of the most important and one of the most demanding tasks. The economic development of the country and the ability of that country to provide food and fiber for its growing population depend on technological innovations; the generation and adaptation of such innovations depends on effective national research. Financial and manpower resources are limited, and the research organization must operate within a deficient institutional environment. Yet the persons called on to take on the daunting task of leading such research institutions are often lacking in experience and formal training in management. To do their job effectively, they need to exercise sound management practices and display outstanding leadership skills. The chapters that follow give some personal views and experiences on how that can be done.

CHAPTER II

MANAGING THE VALUE SYSTEM

The Importance and Role of Value Systems

Generally, when people think of research management, they have in mind planning, evaluation, human and financial resource management, and organization and structure. These are important, and we shall come to them later. I begin, however, with value systems, because the single most important task of the chief executive of any organization is to create and nurture the corporate culture of the institution. Every institution takes on a distinctive character, which can be called its culture. One of the definitions (Websters *New Collegiate Dictionary* 19) for the word "culture" is: "the integrated pattern of human behavior that includes thought, speech, action, and artifacts and depends on man's capacity for learning and transmitting knowledge to succeeding generations; the customary beliefs, social forms, and material traits of a racial, religious, or social group." An institution's culture determines "how we do things around here" as well as "why we do them." While strategic planning involves a vision of the future, it is the corporate culture which shapes that vision. And although the key to research management is the management of human resources, the way people in an institution are thought of and treated depends on the value systems imbedded in its corporate culture. Thus, the first priority for a research leader is to pay attention to the basic philosophy or "ideology" of the institution.

Establishing an Institutional Philosophy

Every institution has a culture made up of a system of values. Such a culture can develop haphazardly or it can be purposefully established. An effective leader will give careful thought to what value system is right for his institution and set about to developing and nurturing it. This is not done overnight; rather it is built up over time. It is not instilled by edict, but is cultivated by consistent actions. It should not require a long document to describe such a philosophy; rather it should be enshrined within a few, simple statements which all members of the corporate family can embrace as their own.

Some examples of components of a value system appropriate to an agricultural research institution might include:

- Concern for the welfare of the rural and urban poor.
- A strong client orientation.
- A commitment to excellence, to high standards of performance.
- A commitment to relevance of research.
- A sense of urgency.
- Mutual respect and trust among all levels of employees.
- Cost consciousness.

Unless it is a completely new institution, a new research director will find that the organization he leads already has an established culture; and that that culture will not necessarily be what is written down or publicly espoused as the institution's philosophy. For the true set of values of a group is not defined by rhetoric, but by how its members behave. An institution may say it emphasizes excellence; but this is meaningless if mediocrity or shoddy work is tolerated and commonplace. A motto that the institution is client-oriented is only that if quality and service are second-nature to every employee. Who will believe that a research institution is developing new production technology for the many poor people who urgently need it when it is common to see people reading the newspaper during working hours, or if is a common practice to arrive at work late and/or leave early? So before embarking on the task of establishing a set of guiding values a leader must first take stock of the current value system as actually practiced, so that these can be compared with the future core values the institution should have, and develop a plan to, over time, make the changes required. Conducting such a "culture audit" involves interviews with people in the institution to identify and analyze behavioral patterns and those things that promote or inhibit such behavior. Observing what people do–and why–will reveal the value systems in place in the culture of the organization. From this it will be possible to extract a statement of values which are *practiced* and compare this to the values which are *espoused.*

Determining what should be the core value system of an institution, and what should be the strategy for achieving desirable changes, should not be done by the chief executive in splendid isolation. An effective leader will normally engage several trusted, key colleagues to assist him in this important task. In doing so, he will not only benefit from their valuable advice, but will also be enlisting partners in the vital but exacting process that lies ahead of "selling" the value system and applying it in daily actions. It will be extremely difficult to permeate an institution with a set of guiding values if the top management team is not already convinced of their legitimacy and importance and committed to a common purpose.

Reinforcement of the Value System

Culture building is not a one-time event; it is a constant endeavor. Maintenance and strengthening of the set of core values require constant reinforcement. And although the employees must be frequently reminded of these values, mere pronouncements from the top will not suffice. Reward systems are an important component of cultural reinforcement, as are various events in which these values, and those who contribute to them, are recognized and celebrated. For example, if teamwork is an important facet of the value system, those who work together harmoniously with others must be given special recognition when merit awards are allocated –and must realize why they have been selected for such recognition. Teamwork can also be celebrated in various functions demonstrating a spirit of working or playing together, such as sports events, musical and drama groups. Those who best exemplify basic values in practice should be celebrated as heroes in various ways. The top management of the institution must also serve as role models in application of values in their daily activities.

Once a set of core values has become an integral part of the institution's culture, new employees will be helped in adhering to them by their colleagues, in the same say as children growing up in a home acquire the value systems of the family. Such a passive mechanism of acquiring the corporate culture by "osmosis"must be supplemented with more active measures. The institution's leaders must use every opportunity which presents itself, and create additional ones, to spell out and promote the elements of this culture. For example, the institution I now serve has as one of its key values the alleviation of hunger and poverty through its efforts. Recently, a new audio-visual package was developed which depicted how this was being achieved. It was made for visitors; but we realized it would help persons working in the Center to also understand how their efforts contributed to this goal, so we arranged to have all employees see the show. Since this was done in small groups, it involved many showings. I chose to personally introduce each session, and used this opportunity to repeat the message that every employee at every level was important to the enterprise and was making a difference in people's lives. The fact that I gave priority in my schedule to make these introductions gave a message that these considerations were important. The deliberate constitution of each group to insure a blend of employees covering the range of salary scales from senior scientist to laborer helped reinforce the message that everyone counts.

While the processes of passive diffusion and active promotion described above will result in most employees taking on the corporate culture as their own, there are aways some who don't fit. They will eventually sense that they are "out of step" and leave the institution. Sometimes this process may have to be accelerated by those hopelessly out of tune with core values. If this is done right, that is, by applying core, people-oriented values in the personnel action, and letting it be known that the most important limits in personal behavior are those related to the key corporate values, the value system is strengthened by such action. On a more positive note, recruitment is an important reinforcement mechanism, as new employees most likely to fit into the corporate culture are selected.

So, you see, it takes time to build a corporate culture, and a process of selecting, motivating, rewarding, retaining and unifying good employees to reinforce it. From time to time, it will be necessary to evaluate the commitment to this common purpose. Hickman and Silva (1984: 65-69) have developed an excellent "cultural awareness checklist" which is useful for this purpose.

The ICTA Example

The Guatemalan Institute of Agricultural Sciences and Technology (ICTA: Instituto de Ciencia y Tecnología Agrícolas) was created in 1973 as the national institution responsible for generating technology appropriate to Guatemalan agriculture and promoting the use of this technology. From the start, it was evident that ICTA was something different from traditional agricultural research institutions. It had a strong orientation to working with and learning from farmers; and to the generation of innovations that would be useful under the real conditions and constraints of small farmers. It emphasized practical, field research; rather than devote major efforts to sophisticated laboratory research or making its own crosses to develop new varieties, it stressed adapting materials and technology that could be obtained from various regional and international organizations whenever possible. It placed most of its scientists in regional field teams rather than at headquarters in the capital city. It also incorporated a strong socioeconomic component into its research structure.

In my frequent visits to ICTA over the years, I have been immensely impressed by the consistency of its policies despite the many changes in leadership that have taken place due to changes in governments in that country. I have also been impressed by the consistently high morale of its staff and their continued dedication to the principles described above. Most important is the fact that ICTA has produced a great deal of useful production technology, which has been adopted by farmers and is making an important difference in the well-being of the people of Guatemala. I do not intend to go into detail about ICTA's programs here. Rather, I wish to identify some key elements in its corporate culture that have made it successful where many agricultural institutions with much greater human and financial resources have failed.

To start with, the founders of ICTA created a clear and unique set of core values. These included: attention to the needs of small farmers; concentration on basic food crops and animals; relevance of all research activities; and increased production by farmers as a measure of success. This is not such a novel list. What was different about ICTA was the various ways in which this culture was reinforced by concrete actions.

One of the most basic reinforcement mechanisms was the creation and consistent adherence to a new terminology. There was no reference to research or extension, only "production." The experiments carried out by the multidisciplinary teams on farmers' fields were called "farm experiments" *(ensayos de finca)*. The second-stage trials to validate technology developed in the farm trials, and involving the farmer and his normal practices other than the new technology, were referred to as "farm trials" *(parcelas de prueba)*. This insured that the cooperating farmers understood that what was being tested was still in a trial stage, so that their confidence would not be destroyed when a practice failed; it also made the farmer, a partner in the process. The word "demonstration," traditionally used for plots meant to validate and demonstrate new technology to farmers was assiduously avoided –indeed forbidden– as the founders of ICTA believed that one of the reasons for failure of apparently good technology in the past, or lack of adoption by farmers, was that the word "demonstration" implied that the scientists were sure they had something of value to demonstrate. To make sure the innovations being demonstrated would not, to their embarrassment, be worse or no better than the farmers' practices under real conditions, they did too much to make sure the "demonstrations" gave superior results. While such rigorous adherence to a special terminology may seem simplistic, I believe it has served as an important element in ICTA's success.

But how are you going to "keep them down on the farm?" The ICTA founders purposely kept the regional research stations small and unattractive, and limited work on them to only essential activities that could not be done on farmers' fields. The national headquarters was also kept small, in a rented office building. Only now, fifteen years after ICTA was founded, is a national headquarters building, complete with laboratories, being finished. They started in farmers fields, then developed modest regional stations, and only when the concept of decentralized, on-farm work had been thoroughly institutionalized, did they develop more sophisticated infrastructure; quite the reverse of the normal pattern!

Indoctrination was another essential element in the implementation of a value system in which regional, on-farm work the norm, rather than something assigned to "second-rate" staff. An important reason why the institute's core values have remained relatively constant in spite of leadership changes (some of which involved political appointments of people who might have had quite different ideas from the founders), is the training given to all new technical staff. All new "Ingenieros Agrícolas" who joined ICTA began their service with a ten-month course. These courses were conducted in the provinces, and included at least 50% field work in addition to indoctrination in ICTA's philosophy.

This is not an exhaustive list of how ICTA's strong culture contributed to success. I am sure that others closer to ICTA could add other examples, and probably temper any excessive enthusiasm I have displayed in this summary. For more details I refer you to the various documents written about ICTA, such as those by Gostyla and Whyte (1980), Castillo and Juarez (1985), Fumagalli, Ortiz and Castillo (1985), and Waugh (1975).

ICTA is now entering a new phase in this process: building stronger linkages for the diffusion of promising innovations. True to its traditions, ICTA has developed a special terminology to describe this activity, and a new type of infrastructure to involve farmers in the process. They call it "FSR/E:" Farming Systems Research with Extension. It is intended that ICTA will work closely with the official Ministry of Agriculture institutions responsible for extension (DIGESA and DIGESEPE) in an integrated effort of technology transfer, in which rural leaders *(líderes agrícolas)* will work with organized farmer groups in "transference plots" (not "demonstrations") to provide the necessary multiplier effect. For more details on those new developments, see papers by Fumagalli *et al.* (1985a) and Fumagalli *et al.* (1985b).

Creating and Managing a Corporate Culture–A Summary

The strength of an institution is its culture. The heart of that culture is the system of values it espouses and practices. Creating and nurturing the core value system of the institution, and exemplifying it in his own personal behavior, is one of the most important tasks of a research leader. To do this, he must clearly understand what that value system is, articulate it in simple, easy-to-grasp terms, and permeate the institution with it, using every opportunity to espouse the culture and celebrate its heroes. An institution that has a clear value system thoroughly engrained within its corporate culture, and whose employees at all levels understand and adhere to those values, will be much more likely to remain effective in the face of adverse circumstances such as resource constraints and changes in leadership which so often afflict Third World research organizations.

CHAPTER III

MANAGING PEOPLE

The late industrial giant, John D. Rockefeller, was heard to have said: "I will pay more for the ability to deal with people than any other ability under the sun" (Bergnen and Honey 1966). Management of human resources is important in any enterprise; it is fundamental in a research organization. The end product –new knowledge and technology– is produced by people, not by machines. Most research institutions devote seventy percent or more of their budgets to personnel. Thus, one of the most important tasks of a research leader is to develop a cadre of experienced research personnel of the proper size, mix and educational level, and to inspire them to give outstanding performance.

Planning Human Resource Needs

A common plea by research managers is for more, and more highly trained, staff. But an overly ambitious wish list of personnel requirements is not only unrealistic, it is erroneous and may even be dangerous. More important than the total number of staff is the proper mix of disciplines and of levels of training and experience. Careful planning of human resource requirements is a key element of research management. I place this subject here because of its fundamental importance; however, chronologically this exercise comes after the research planning discussed below; for it is only after research priorities and programs have been determined that the planning of the numbers and mix of different kinds of personnel can be established.

Many Third World agricultural research organizations have too many people rather than too few, at least for the level of financial resources available. As a consequence, most of the budget (often reaching 90% or more) is used up for personnel costs, with the resultant shortage of operational funds for equipment, supplies and travel, and consequent low levels of productivity. Overstaffing is the result of a combination of external and internal pressures and, in many cases, poor management. It is common to attempt to do too much (e.g., too many commodities in too many locations) for the amount of resources available. This may be the result of external or internal pressures, but can be alleviated by better prioritization in the use of limited resources. We will come back to this later when we discuss planning. Another cause of overstaffing, resulting chiefly from internal pressures, and which is more directly related to the subject of human resource management, is the very common human tendency of scientists and middle management staff to ask for more people to work under them. When such requests are granted in times of adequate funding, it is very difficult, if not impossible, to reduce the staff in years of financial difficulties. Another reason for taking on more staff than the institution needs or can afford is the strong pressure research institutions are under in some countries to absorb the growing number of graduates in agriculture and science coming out of the universities.

The same pressure to absorb new university graduates, and in some cases a lack of understanding of what kind of person it takes to conduct research, along with lack of scholarship opportunities for higher degree studies, has resulted in the staffing of some research institutions with underqualified "scientists." A myth seems to have developed in some circles that one can do science without scientists. A fresh university graduate, whether from a science or agricultural college, is not yet a scientist. The first degree lays a solid foundation onto which the additional knowledge, skills, and discipline required to do research must be built. While it is true that some first degrees (e.g., the Ingeniero Agrónomo degree common in Latin American universities, and the Ingenieur Agronome degree given in some francophone African universities) include a research thesis among the requirements for graduation, an additional, advanced degree, with the specialized, disciplinary knowledge and the scientific rigor acquired by this experience, is highly desirable, if not essential, for high-quality research performance. Thus the entry point for scientific research staff should normally be a Master's degree or equivalent.

There should also be a proper balance between holders of M.Sc. and Ph.D. degrees. While the value of the additional skills and knowledge achieved through doctoral studies is obvious, and the importance of having some scientists with these qualifications on the research staff is clear, this does not mean that institutions with limited resources should strive to have all, or even a majority, of their scientists with Ph.D or equivalent degrees. The costs, both for the training and subsequent personnel costs may be too high for many institutions. At a symposium on Planning and Management of Agricultural Research held at the World Bank (Elz 1984), the author of the lead paper on "Manpower and Financial Constraints" warned against recruiting at too high a level because recruits with greater qualifications command higher salaries. If such people are hired in substantial numbers, salaries and benefits will tie up increasing portions of available funds; because the more highly qualified recruits are placed in higher job levels, they also reach various salary and job ceilings faster, increasing the probability of early stagnation and, therefore, the likelihood of lower motivation.

In the discussion that followed this paper, Dr. Amir Muhammed, Chairman of the Pakistan Agricultural Research Council, stated that most research directors have been conditioned to think that the number of staff with the Ph.D. degree determines the quality of the research system, but that he had found that those trained to the M.Sc. level worked on applied problems better, since staff with Ph.D.s often wanted to do research that was too sophisticated for local circumstances. On the other hand, Dr. Eliseu de Andrade Alves, President of the Brazilian National Agricultural Research Organization (EMBRAPA), supported the training of a large number of persons from his institution to the Ph.D. level. He made the interesting point that there are two ways to produce good scientists. One is to let them get a M.Sc. and train them to become good researchers. This takes time, and they will make some mistakes; but in the end, they may become very good scientists. The alternative is to pay for them to get a Ph.D. abroad. The costs will be obvious, but they may be lower than the hidden costs of on-the-job training implicit in the first scheme. There is clearly no easy answer which will apply to all countries and circumstances, but it is clear that research leaders must give this matter careful thought and work out the best balance for their particular situation.

Finally, in planning the human resource requirements, the appropriate mix of support staff undergirding the work of each scientist should not be neglected. This means insuring that the proper number and mix of technicians and skilled laborers is included in the staffing plan. Well-trained scientists are expensive. It is essential that this scarce and valuable resource be fully utilized; this means giving them the support staff to maximize their productivity. Sometimes research managers deny scientists field laborers with the laudable goal of insuring that the scientists themselves get out in the field. I agree with that goal, as no scientist can adequately plan the work or interpret the results without himself being personally involved in the field work; but there are other ways to make this happen, which we shall come to later. All I want to point out here is that it is foolish to use the precious resource of a highly skilled scientist to do things a less-expensive person could perform; it is a question, once more, of balance.

The matter of technicians is too often neglected in the manpower planning exercise. One problem is that developing countries are often short of well-trained, non-professional, technical staff. When this is the case, the research institutions themselves may have to embark on special training programs to prepare or improve the skills of such staff. The CIAT Regional Bean Program scientists working in Africa recently assisted the national research programs of Rwanda, Uganda and Tanzania to carry out training workshops for field technicians. This was perceived by the national research leaders as making an important improvement in the effectiveness of their research programs.

Personnel Selection and Evaluation

Even if you have all the financial resources needed, excellent facilities, and well-formulated plans, you can't have an effective research institution with mediocre staff. Careful selection of personnel, therefore, requires the best efforts of a research manager. This is not something that can be done haphazardly or quickly. There never seems to be enough time available for all the tasks required of a research manager, but this is an area for which plenty of time must be allocated.

Like manpower resources planning, this is an activity that begins with program planning. In an earlier paper (Nickel 1983), I recounted a discussion I once had with the late Dr. Sterling Wortman on this subject. I will repeat the advice he gave me: before recruiting for any position, be very clear about the precise type of work you expect him to do. Many organizations do it the other way around; they have a vacancy for a particular field and then try to fill that position with the best scientist of that discipline. The trouble is that there are often different types of specialists or experience within the same, general disciplinary area; so once the person has been selected, managers attempt to change the person to fit the job, or alter the research program to fit the person. Knowing clearly what role the person will be expected to play in the research agenda is a much better way to begin the selection process.

Creativity is essential in a research operation. We shall later go into how the right kind of environment can be created to foster creativity. But this is useful only if the institution has creative scientists. Thus creativity is an important factor to strive for in selecting research staff. While this quality may not be found for all positions, it is important that the institution at least have a core of some highly creative scientists who will stimulate their colleagues. Such persons may not always have the kinds of personalities that appeal to management as being easy to handle; but to select only scientists who won't cause problems is to rob the institution of some of the best minds. To select only those who don't have obvious weaknesses, rather than look for strengths, is to settle for mediocrity.

It is of special importance to select well-experienced scientists to lead the respective programs. In the case of multidisciplinary teams, it is particularly important that such leaders are capable of identifying key production constraints, translate them into coherent, relevant and realistic research plans, and forge a group of diverse scientists to execute a program to carry out such plans effectively. If these programs are to be given the autonomy and flexibility essential for creativity, yet focus their research on agreed, high-priority objectives, the program leaders will be pivotal to the success of the research institution. You will do well to give special attention to selecting the most capable persons to fill these key positions.

Several studies have shown that the overall productivity of a research organization is heavily dependent on the performance of its leaders and the top five percent of its technical staff. If these positions are filled with outstanding persons, other competent people will be drawn to the institution. Program leaders should be unusually competent, creative and innovative; they should be highly motivated, self-confident, self-directing and willing to work harder than the other members of the team. They should be extremely dedicated and possess high integrity, values and standards.

Evaluation of staff is also an important responsibility of a research manager. This is important in order to identify and reinforce strengths as well as to identify and minimize the effect of individual weaknesses. Many managers find this to be one of the most difficult and onerous of their tasks. Furthermore, investigators have found little relationship between ratings generated by evaluators and actual on-the-job performance; and, when the same individual is evaluated by two or more evaluators, assessments vary widely. One reason for this is that the role of "judge" and "counselor" are viewed as incompatible. "Coaching" or "mentoring" would probably be better terms to describe this function; and this points out the need to consider staff evaluation a continuous, day-to-day activity rather than a once-a-year event. One of the most common management errors is to save up all complaints for the end-of-year evaluation, rather than to try to correct weaknesses as they become evident. Another common fault is failing to let employees know by what criteria they will be evaluated. Letting people know what is expected of them and giving direct feedback in a timely manner is one of the most important leadership functions. It is also one of the most difficult things to do right. I find giving negative feedback particularly awkward. The danger of this attitude is that one tends to neglect criticism until the problems build up to a crisis. Negative feedback should not be given in a crisis situation or in front of others. It is much better to do it in a planned and calm manner, and to provide ample opportunity for the person being evaluated to explain his point of view. In this way misunderstandings of what was expected can be clarified and reasons for actions can be uncovered in a way that the situation can be corrected in a constructive way. It is essential that such feedback be nonevaluative; that is, attention must be directed at performance, not the person. Most people tend to interpret the message as a characterization of themselves, rather than their performance. This should be avoided. The point is to challenge a person's mind, not his ego. The message that should be conveyed is "I respect you as a person, but we need to mutually find a way to improve your performance."

In evaluating a person's performance, it is important to distinguish between accomplishments and activity; between working efficiently and working effectively; between work and important work; between working hard and working smart. A person may work long hours, but be using his time inefficiently. He may work efficiently and do things right, but be doing the wrong things.

A particularly thorny problem is whether or not to discuss the formal evaluation, required as a bureaucratic procedure in many institutions, with the person in question. Drucker (1966) indicates that while most organizations have in place a system that requires discussing the evaluation with the staff member, such interviews in practice almost never take place. This is because the evaluations tend to emphasize the person's weaknesses; and it is too painful to discuss a person's defects. He points out that, ever since Hippocrates, it has been known that the association between healer and patient is incompatible with the authority relationship; thus doctors have always considered the patient/healer relationship as privileged. It is best, therefore, for the evaluation to be based on the major contributions expected of the person and the discussion to center on the person's performance against these goals. This underlines one of the most common weaknesses of staff evaluation, whether it be on a continuing or periodic basis; too often the specific tasks and expected performance of a particular job are not clearly spelled out from the outset. When this is done, the mentoring and evaluation tasks of the supervisor will be more pleasant and effective. For further assistance in the important task of staff evaluation, I refer you to a useful, brief paper by Len Grensing (1985) which describes common rating errors and spells out helpful "steps to error-free evaluations."

Finally, I cannot leave the discussion on staff selection and evaluation without mentioning the unpleasant, but vital truth that some people have to be selected out. Any institution that does not prune out dead wood is doomed to mediocrity. I know this presents a particularly difficult problem to managers of national research institutions, as they are usually bound by government regulations or labor laws which make it practically impossible to rid an organization of unwanted or unproductive employees. I can prescribe no easy solution, but national policymakers and legislators will have to "bite the bullet" by amending such rules if they expect their national institutions to perform effectively.

Motivation for Excellence

Having selected the best people available, it is essential to create the right environment to stimulate creativity and productivity. Peter Drucker has correctly stated: "the test of organization is not genius. It is its capacity to make common people achieve uncommon performance" (Drucker 1966). Research managers do not create a product; their main task is to create the right conditions so that the scientists in their organization will be most effective in generating the product of the institution: knowledge and technology. The Hughes study on R & D productivity (Ranftl 1985) showed that personal productivity does not correlate significantly with such factors as I.Q., excellence of education, schools attended, curricula pursued, grades achieved, or specialized courses taken. Such factors are of great significance when hiring someone into the organization, but differences in productivity primarily depend on two key factors: attitude and motivation. I shall, therefore, devote a considerable portion of this letter to this key subject.

One reason I believe this topic is of such special significance is that this is an area in great need of improvement in developing countries. Unlike salary levels and the broader institutional context, this is something a research director can influence very positively. It is generally recognized that the productivity of agricultural researchers in many Third World institutions is very low. In the course of my work, I have had the privilege of personally visiting such institutions in over fifty developing countries in Asia, Africa and Latin America; in all continents I have seen the same syndrome of low productivity symptoms. Yet, I have observed much higher productivity in international Centers, and some national research institutions. Being a highly skilled and productive scientist very much depends on the kind of support and motivation such a scientist receives. It depends as much on the working environment as on the training and experience of the persons involved. While the creation of such an environment is certainly easier in an international organization, I am convinced that there are many things that can be done to improve these factors in developing country institutions.

Frederick J. Herzberg (1974), in his motivational theory, isolates two groups of needs, motivators and dissatisfiers. The motivators include interesting work, challenging work, personal achievement, recognition, and the opportunity for increased responsibility. The dissatisfiers are organizational policies, working relationships and conditions, supervision, and compensation. He has labelled the management actions to reduce the dissatisfiers as "hygiene factors." I shall use this dichotomous division to structure the comments I wish to make about key management principles as they apply to a research institution, taking up the latter set first.

Hygiene Factors

Salaries. Many may be surprised to find compensation among the dissatisfiers rather than the motivators; I consider it correct. In my experience, I have not found salary levels to be the most important factors affecting staff morale or productivity; that is, if they are at a sufficient level to meet the essential needs of a scientist and his family. That is an important qualification, as no one can be satisfied or concentrate on his work if his income is insufficient to meet the basic needs to feed, clothe, house and educate his family, or if he considers himself to be unfairly treated *vis-a-vis* his colleagues. Therefore, it is essential to ensure that adequate compensation is given and that financial rewards are equitably allocated. But there are limits to what a publicly funded institution can pay; and if adequate levels are assured, other factors become more important in the motivation of staff.

Much attention has been given to the need to develop semiautonomous research institutions in order to remove them from the often woefully inadequate salary structures of civil service scales in many developing countries. This is important; but it can be carried only to a certain limit. Amir Mohammed (Elz 1984) has wisely pointed out that when the head of state earns only the equivalent of US$1000 per month, it is not possible to offer scientists salaries that are internationally competitive; thus scientists need to possess an element of patriotism, idealism and concern for humanitarian issues.

Operational Funds, Equipment and Facilities. One of the greatest sources of low morale, low productivity and "brain drain" in developing country agricultural research institutions is the lack of adequate funds for supplies, travel, and temporary labor; and the lack of modern, well maintained equipment and facilities. Scientists are often faulted for staying at their desks rather than getting out in the field and for carrying out routine, pedestrian research; when the truth is that they have no alternative, given the shortage of operational funds. It is all too common to find that 90 percent or more of the research budget is taken up by personnel costs; under these conditions it is next to impossible to perform effectively. I have also observed (in the case of EMBRAPA, for example) what a tremendous boost in morale a new facility with modern equipment can give to a research team. Many times capital funds for such modernization are not available; but they often do not cost more than a year's operating budget, so it doesn't make sense to continue year after year wasting that budget in facilities that do not stimulate productive work. On the other hand, good facilities don't do much good if there are not adequate funds for operations. Obviously a proper balance must be achieved between personnel costs and operating budgets, and between capital and recurrent costs. We shall come back to this again when discussing the resources mix in the section on budgets.

Research Support. Another important counterproductivity factor for agricultural research is poorly managed or nonexistent research support. By this I mean the experiment station, greenhouses and service laboratories. Among these none is more important than research station management. By definition, much of agricultural research must take place in the field. Unless the research fields are properly managed the results will be useless and the scientists frustrated. Uneven or weedy fields are often the cause of high coefficients of variability which make the results difficult to interpret or statistically insignificant. Good experiment station management includes such basic operations as proper land preparation, timely irrigation and weed control. Many experimental station fields have been rendered useless by a gradual build up of nutgrass. Others have become so acid through decades of fertilization that they are no longer representative. Often no systematic record has been kept of previous fertilizer or herbicide treatments, making results hard to interpret.

A good station manager must be a good farmer; because management and maintenance of the farm machinery is essential to timely operations, specialized training in agricultural engineering is also important. Research institutions too often do not give enough attention, or do not budget a sufficiently senior post, to attract and keep highly skilled and experienced experimental farm managers, and as a result the good scientists they have are less productive than they could be. Specialized, in-service training should be given to farm managers lacking the required skills and experience. One source of such training of which I am aware is the International Maize and Wheat Improvement Center (CIMMYT), in Mexico. A Master's degree course on Farm Management is given at the University of Monterrey, Mexico. I'm sure there must be others, and you would be well-advised to send your manager in charge of station operations to obtain such training.

Another, often neglected, area of research support is that of information services. Scientists who do not keep up with what is going on in their field may use outmoded methods or waste their efforts repeating experiments already done elsewhere; yet maintaining a large, current library of scientific journals is very expensive and well beyond the resources available to many research institutions, especially in smaller countries. And even the best libraries are hard pressed to keep up with the rapidly increasing flow of information. It is estimated that more than 15,000 scientific and technical articles are published daily worldwide! Furthermore, busy scientists often do not go to the library or take the time to dig out the relevant information they need. What is needed is an instant awareness service in which the specialized information needed by each scientist is made directly available to him. Such specialized services are now available for specific commodities or research areas from several of the international agricultural research centers, FAO's AGRIS, the Commonwealth Agricultural Bureau, and others. You will want to subscribe to such services. You should also keep aware of the rapidly developing area of electronic information services which provide instant access to some of the world's largest reference collection. These still involve relatively expensive communications costs; but it will not be long before entire data banks can be available to any institution with a microcomputer and an inexpensive compact laser disc reader.

Administrative Support. Every institution needs efficient administrative units, such as accounting, purchasing, personnel, and maintenance. These are absolutely essential, and the persons working in these sections should be encouraged to take pride in being full partners in the activities of the institution. There is a natural tendency, however, for such activities, because of the vital functions they perform, and the controls they must exercise, to take on a life of their own. Sometimes scientists get the impression that they are working for the administrators, filling out the various forms and reports required, and following the procedures essential to good control. What must be emphasized is that neither is working for the other, but all are working together to make the institution efficient yet effective. The support nature of the administration must therefore be emphasized, and a service attitude engendered, without in any way belittling the importance of those working in these units.

Positive Motivation

Having gone over some of the principal management components which if not done right can act against motivation of staff, let us now consider some actions that can contribute in a very positive manner to improve motivation.

Meaning. All of us want to lead a meaningful life. Shaw, in "Man and Superman," stated that the true joy in life is being used for a purpose recognized by yourself as a mighty one. Nietzche is reported to have said: "he who has a why to live can bear almost any how." A research leader has two vital functions in this regard. One is to permeate the organization with the realization that what the institution as a whole is doing is deeply meaningful. This task is largely accomplished by creating the right value system within the corporate culture and constantly reinforcing these values, as I have discussed earlier. A work environment in which all are aware that the products of the institution are going to make an important difference in the lives of millions of poor people is a tremendous stimulus. The second task is to take various actions that will make each individual feel that he or she is making an important contribution to this noble goal. Employees should not work under the illusion that only the scientists are creating something of value, but that each one of them is a soldier in the battle against hunger and poverty. When employees have the sense that what they are doing is much more than a job and that they are partners in a worthy cause, they will be motivated to make a maximum effort.

Recognition. All people desire and work for recognition. Giving positive reinforcement is one of the most powerful tools of the research leader. I have already mentioned the importance of giving timely positive and negative feedback as an essential component of the evaluation process. It is also a key element in motivation. The right to criticize when criticism is needed is earned by praising whenever praise is due. The effective research leader will, therefore, actively seek out opportunities to complement people. This does not mean empty flattery, which is easily recognized for what it is, but genuinely recognizing good performance and verbalizing such recognition. I have learned that the most productive and creative people are often those who have the greatest need for recognition; the research leader who overlooks this fact foregoes a great deal of benefits to his institution. Bennis and Nanus (1986) state that people love others not for what they are, but for how they make them feel. Thus the business of making another person feel good in the unspectacular course of his daily work is the very essence of leadership. In addition to more formal recognition, I have found it useful to keep a store of recent accomplishments in mind for individuals so I can mention them when passing them in the hallway or meeting them in the snack bar. One of the important benefits of "management by walking around" is to encounter people doing their job and to let them know you appreciate how well they are doing it.

One tool for active recognition which I have found particularly useful, and appreciated, is to respond personally to the writers of the many reports which cross my desk. As the institution I currently direct is an international one, these take the form chiefly of trip reports. Including in my response enough statements about the contents of the report so that the writer knows I have read it serves the valuable purpose of letting the individual know that management is aware of and interested in what has been accomplished and what problems have been encountered.

A Creative Environment. Creativity is what research is about. Thus a research institution must not only have creative people but also an open, creative, professional work environment. Fostering such an atmosphere means minimizing bureaucratic procedures; encouraging innovation and the taking of calculated risks; putting up with innocent foibles and eccentric mannerisms; and making arrangements to stimulate informal communication.

Highly educated, creative people are put off by bureaucratic procedures and rigid, formal hierarchy. Thus a careful balance between necessary controls and authority and an open, informal atmosphere must be sought.

Informal communication among scientists must be fostered; it is how many new ideas are generated. Within disciplines this takes place naturally when scientists are organized into disciplinary departments. However, as I shall later explain, I favor organization into multidisciplinary programs. When this is done, a special effort has to be made to foster intradisciplinary communication. One way to accomplish this is to physically place scientists from the same discipline, but belonging to different programs, in the same building or laboratory. Another is to promote interprogram, disciplinary seminars. Probably of even greater importance is the stimulation and idea generation that results from informal discussion with persons from different fields of specialization. Such cross fertilization can be promoted both by physical arrangements and management attitudes. Physical arrangements include such simple mechanisms as having plenty of blackboards or flip charts around where people meet; and placing many small tables in snack bars or cafeterias where people from different departments or programs can meet over meals or refreshments. Managers must also avoid the temptation to think that when scientists are chatting "too long" at coffee breaks or lunches that they are wasting time when they should be "working." Some of the most important ideas may be generated in such informal interaction. Of course, if they are only talking about yesterday's football game, that is something else; but this is a risk worth taking to stimulate productive communication.

Scientific research is an adventure into the unknown; therefore it means taking risks. I have learned that a thin line (often seen only with the benefit of hindsight) separates a "bold innovation" from a "dumb mistake." A climate in which mistakes are looked upon as learning opportunities rather than classified as "failures" is thus essential to a creative environment. Bennis and Nanus (1985) cite a striking example of how successful companies tolerate mistakes as learning opportunities. They record how a promising junior executive at IBM was involved in a risky venture for the company and managed to lose over $10 million in the process. When IBM's founder, Tom Watson, Sr. called the nervous executive into his office, the young man blurted out, "I guess you want my resignation?" Watson's response was, "You can't be serious. We've just spent $10 million educating you."

Creative people are not all poured from the same mold; and surely not the same mold as may characterize their managers. An effective research leader must, therefore, be generous in tolerating differences in behavior, and even what seem to be odd or eccentric behavior, as long as these do not seriously detract from the work or image of the institution. In this regard you may find it singularly useful to become familiar with the type indicators developed by Isabel Myers and Katherine Briggs (1962). This system classifies all people into sixteen types on the basis of differences among them in their preferences for:

finding out through sensing or intuition; deciding by thinking or feeling; relating to the outer or inner world as extroverts or introverts; and using perception or judging in dealing with the outer world. None of these combinations is considered to be either "good" or "bad," but different. Understanding what type you are and what type your colleagues are can be very helpful in understanding why people react differently to various situations and how opposite preference can complement and reinforce each other. MBTI (Myers-Briggs Type Indicator) testing is increasingly becoming an important component of recruitment and management training, and should be conducted by persons trained in this area. For more information on this subject, a good contact would be Consulting Psychologists Press, Inc., Palo Alto, California.

Caring, Listening, Trusting. I have already alluded to the fact that, like you, I was thrust into full-time administration early in my professional career. In preparation for this task (in 1966) I sought a book I could read on academic/research management but found none. So the only reading I did was the well-known book by Dale Carnegie, "How to Win Friends and Influence People." With hindsight, that was probably one of the best books I could have read at the time, since most of management has do do with dealing with people; and whereas all people are alike in needing personal attention and recognition, all people are also different. Each is an individual with his or her own set of problems, concerns, ambitions and pride. Caring about these individual, personal needs is one of the most important motivational responsibilities of a leader. It has become one of the main themes of more recent management literature; but it has long been recognized by effective leaders. One of the best pieces of advice I received on becoming a Dean came from Dean Umali, one of the great leaders of the University of the Philippines at Los Banos. He advised me that in matters of discipline related to work performance to be firm (if my memory serves me, he may even have used the word "ruthless"), but in matters of personal needs, to be compassionate.

Respect means recognizing each person's individuality, and taking time to listen to their personal goals, joys and sorrows. Becoming a good listener is a leadership skill that can be learned and sharpened. Active listening involves using the right body language; following the other person's agenda rather than reverting to one's own; verbalizing understanding of what the person is saying; and probing to clarify understanding of the other's thoughts and feelings. Probing to get a clear idea of the other person's point of view is a skill that involves asking open-ended questions; asking only one question at a time; giving the person time to answer; and avoidance of criticism. Such empathetic listening can only be done by devoting sufficient time to the exercise; and that time should be quality time, that is, the listener must be disciplined not to let his mind wander to the many other things on any manager's agenda, but to concentrate on what the other has to say.

Giving enough quality time is difficult for overworked executives, but it is a task that must be given priority. A practice I have found useful is to schedule periodic, personal interviews with each of the senior staff in my institution; letting the individual talk about anything he wishes, such as accomplishments, plans, career goals, constraints to productivity, complaints about the Administration, and personal problems and concerns. Such a process is very costly in terms of scarce senior management time, but well worth the investment.

Respect and trust are two sides of the same coin. It does little good to hire the best staff possible and then to second-guess them all the time. Research leaders must, therefore, avoid the temptation of "micromanagement." Managers are too often inclined to consider each travel request as a "junket," and each equipment request as another "sophisticated toy." Yet we must recognize that if we have selected people who ask for unreasonable or worthless things or cheat on their expense accounts, we have made a much larger mistake than these items represent; for we have entrusted to them the planning and carrying out of very costly research projects of great social value. If we trust them to exercise mature judgment in the latter, we must, within reasonable limits, trust them in the former. It has been my motto to hire the best scientists available, give them whatever they request, and then expect miracles from them, so they will be deprived of any excuse not to perform. Of course, budget realities make this a utopian model, but it is very important to create conditions which foster the attitude by staff that the greatest limitation on their productivity is themselves.

Attitudes for Effectiveness

Having carefully selected the staff and taken various actions to motivate them, research leaders should expect certain attitudes from such staff, and stimulate such attitudes to ensure maximum, effective productivity.

Relevance

Insuring relevance of the research programs is part of the research planning process dealt with in Chapter 4. But it is also a product of the attitudes of the scientists; for regardless of how carefully the research is planned, the actions of the individual scientists, in how they plan and carry out the separate experiments within the overall plan, will be key to whether or not the planned objectives are achieved. It is the duty of the research leader, therefore, to constantly insist that researchers concentrate on the important rather than the interesting; and that they give priority to the goals of the institution and their respective programs over personal interests. Institutional and career goals need not be incompatible, since problem-solving research can, and should be, of the highest quality, which can be reported in respectable, refereed scientific journals and contribute to the scientists' professional reputations. It must also be recognized that permitting scientists some flexibility to venture into the unknown is an important component of a creative atmosphere, and that what appears only "interesting" today may become very important tomorrow. But solving problems to overcome priority problems must always be the objective of the work of each member of an agricultural research institution's staff.

Excellence

The crystal-clear ring of excellence should permeate every part of an agricultural research institution! The task of creating new technology to fuel increased, sustainable agricultural development is simply too important to settle for mediocrity. And the commitment to excellence must not be restricted to how the research is done, but how neatly the letters are typed, how well the hallways are cleaned, how well the equipment and vehicles are maintained, and everything else that is done in the institution; for excellence is contagious, and one cannot expect high-quality research to be performed in a place where other things are done in a slipshod manner. Unfortunately, excellence in the appearance and maintenance of facilities is too often equated with affluence. Of course it does cost a bit more to buy paint and spare parts, or to do good cleaning, but that is small compared with the waste of resources seen in so many poorly managed institutions. The most important ingredient for excellence is not money but the attitudes of the employees.

Striving for excellence should be one of the important components of the corporate culture, so that it becomes the natural way "things are done around here." At CIAT, it has become a key element in what we call "The CIAT Spirit," so that even the institute's choral ensemble, dance group, and sports teams are frequently winning prizes in the region, because employees have got into the habit of doing things well. It is the responsibility of a research leader to insist on excellence, and to promote the attitudes basic to it.

Hands-on Productivity

Not only should staff in an agricultural research institution work well, they must work hard, and with a sense of urgency. Too many people depend on the results of its work to permit a leisurely pace or complacent attitudes. Whenever possible, merit increases or bonuses should be incorporated into the compensation system to reward such actions. Probably of even greater importance in stimulating the right attitudes is the example given by research leaders and senior scientists. If the boss regularly turns up late for work, it is hard to convince subordinates of the urgency of what they are doing.

I recall the first time I visited IRRI in 1963 and saw the world-class scientists who were at that time beginning to develop what would later be called the "miracle" rice varieties regularly going into the knee-deep mud of the rice fields to conduct and supervise the work personally. I was thinking that this attitude, rather than specific technical skills, would be the most important thing the young scientists from many Asian countries participating in training courses in that institute would take home with them.

One of the most common complaints by managers and observers of Third World agricultural research institutions is that the scientists prefer to stay in their offices, sending subordinates out in the field to do the hard work. A common myth has arisen that the educated people in some cultures have a strong aversion to "getting their hands dirty," or engaging in hands-on field work. I believe that this is usually not so much a matter of strong local cultural norms as it is the result of other underlying factors which are easier to overcome. Allow me to give two simple, personal examples. When I served as a USAID Entomology "Advisor" in Kampuchea (then called Cambodia) in the early 1960s, I did a great deal of field work to survey the severity of pests of various crops and conduct experiments to develop control practices. At first when I ventured into a rice paddy, my Cambodian "counterpart" stayed in the car, parked in the shade. Later, when we had to spray some experimental plots with a backpack sprayer, it was I who had to mix and apply the insecticide while my Cambodian colleague stood by. At first glance, these experiences would seem to confirm the stereotype of the Asian professional not wanting to dirty his hands. But it was not that simple. Later when he became more confident in the survey techniques, and in how to calculate the spray mixture and make the application, this same person insisted that he do all the work and that I stand by. My interpretation of this is that the person in question, a recent university graduate, was not adverse to such work, but was afraid that his lack of practical experience (usually not part of the university curriculum) might make him look foolish. This example demonstrates that educational and training programs that include practical field work are more important than cultural differences.

Another example comes from Africa. When we first introduced training in some practical field skills into the curriculum at Makerere, a few of the first students who were sent out for their field practice sessions wore their academic gowns, to indicate their belief that such work was not appropriate to a university education. However, they soon became very enthusiastic about the skills they were acquiring and asked for more of this experience. That part of the curriculum has not only been retained to this day, but has been expanded.

The tendency not to get involved in field work is probably due largely to tradition combined with peer pressure. Few have the courage to break from the established norms. A single individual returning from higher degree studies or in-service training where he has learned the skills and appreciated the satisfaction of personal involvement in field research feels isolated and afraid to stand out as different. This underlines the importance of creating a critical mass of persons trained in these methods, and the importance of research leaders who will set the attitudinal pattern.

The Power of People–A Summary

The essence of research management is the art and science of dealing with people. Without good and well-motivated staff, the best facilities and most adequate financial resources remain unproductive. With people who have been carefully selected to do specific tasks; who have been well informed as to what is expected of them; who are skillfully coached do do that task; and, above all, who are highly motivated, there is almost no limit to what can be accomplished, even with less-than-ideal facilities and budgets. Motivation involves making people know they are doing something important; treating them as individuals who can be trusted to perform responsibly; recognizing their achievements; and surrounding them with an environment conducive to creativity and productivity. A research leader must devote a large portion of his time to this task and constantly strive to improve his competence in this vital management skill.

CHAPTER IV

MANAGING THE RESEARCH

Planning

"A well-managed organization is a 'dull' organization. The 'dramatic' things in such an organization are basic decisions that make the future, rather than heroics in mopping up yesterday" (Drucker 1966). Such basic decision-making about the future is the essence of research planning. An important component of research management is deciding what is going to be done. This involves the setting of priorities (national and institution levels), determination of the constraints to be overcome by research (institution and program levels), and the development of a strategic plan (institution level), and long-range and annual work plans (institution, program and individual scientist levels).

As you will be managing the national agricultural research institution, I will confine my discussion of research planning in this letter chiefly to planning at the institution and program levels. However, as you are likely to be a member of whatever group is given responsibility for overall planning for the national agricultural research system, I will make a few remarks regarding this level of planning as well; much more information regarding the determination of national research priorities, organization of national agricultural research systems, and allocation of resources at the national level can be obtained from the many publications on resource allocation and the good work of ISNAR. The discussion of planning in this chapter will deal chiefly with the planning *process,* or how to go about the business of planning at various organizational levels. The *content* of the plans, i.e., some thoughts about nature of the strategy that I consider most appropriate for agricultural research institutions in developing countries, is dealt with in Part II (Chapters 8 and 9).

At the National Level

Those planning agricultural research at the national level should take into account all institutions in the country engaged in research of importance to agriculture. Besides the national institution specifically charged with agricultural research, this should include universities, specialized commodity research institutes, and private sector research. Every effort should be made to allocate responsibilities among all of these organizations and coordinate their activities so as to avoid unnecessary duplication of effort and insure complementarity. Some overlap in responsibilities is, however, not always a bad thing. Especially in larger countries, some competition may be healthy. A classic example of such duplication is the U.S. system of 50 state experiment stations and the USDA Agricultural Research Service, which work on similar problems, plus a myriad of private sector research efforts. One is tempted to wonder whether the productive agriculture that has arisen as a result of their research would have come about if there had been a stronger national planning and coordination effort. But for smaller, developing countries, resources are too scarce to countenance wasteful duplication.

At the national level, it is important to insure a congruence between the national development objectives and the research plan. An essential element of national planning is to set priorities for various commodities and agroecological regions. One of the greatest weaknesses of national agricultural research systems in developing countries is that the scarce resources are spread too thinly over too broad a range of commodities. Every country produces a wide array of agricultural products, some obviously more important than others. National planners must have the courage to decide which will receive attention, what proportion of available resources will be allocated to each of those selected, and which will, at least for the time being, be neglected. There are often pressures to cover every geographic and ecological region as well. Most countries have more research stations than can be effectively supported, manned and managed. The political will to give priority to selected regions is sorely needed.

Economists are important players in the planning exercise. Biologically-oriented scientists can best indicate where the scientific progress to overcome production constraints is most needed and what are the probabilities of success, but economists are best equipped to estimate what the social benefits will be if they succeed. While the role of economists on research teams is very important, they are also key players in the setting of national priorities and allocation of national resources. The former will often need to be most skilled in microeconomics; the latter must be able to deal with broader, macroeconomic issues. Economists, working closely with biological scientists, should identify commodities that account for a large share of output or commodities where large gains can be made in a short time; they should also estimate the anticipated cost/benefit ratios for alternative future research strategies in order to help establish priorities. The methodology for such *ex ante* analyses is not as well developed as for *ex post* analysis of the benefits of agricultural research; but economists should be pressed to make the best estimates they can, based on the anticipated production increases, estimated rates of adoption, and estimated costs, in order to give planners a rough idea of the expected returns on investment.

At the Institutional Level

On the basis of the national plan for commodity and regional priorities, and for a division of labor between various organizations, the national agricultural research institution will need to develop its own plan, taking into account the mandate assigned to it. This assumes that the priority commodities and regional emphasis have already been determined as part of the national plan. If this has not been done, the institution will have to do its own analysis to determine which commodities and ecological/geographic regions to focus on. Even when the national exercise has been done, the commodity priorities may only be spelled out in general terms (e.g., grain legumes or livestock production), in which case these will need to be further refined at the institution level, using the same interaction between economists and biological scientists referred to above.

The institutional planning process should not be an extrapolation of previous strategies and activities, as is commonly done, but should take a fresh look at what needs to be done most and how. In this exercise, it is important to distinguish between strategic planning and operational planning. Strategic planning describes the institution's vision of the future and the operational principles that will get it there. Operational planning defines the strategy and work plan for accomplishing this over a given period of time. While strategic planning should be "top down," operational planning should be "bottom up."

For both strategic and operational planning, it is clear that a plan will not succeed unless it is "owned" by those whose responsibility it will be to carry it out. These then are the persons who must do the planning, rather than assigning this task to a special "Planning" unit in the institution. In recognition of the importance of individual managers at various levels in the organization to be involved in the planning process, there is a growing trend in business corporations to eliminate planning departments, or at least greatly reduce their power. This should be done in agricultural research institutions, many of which still have large and powerful planning units.

Strategic Planning The starting point for strategic planning is the set of value systems embodied in the corporate culture (see Chapter 2). If this has not already been done, it should be the first item of business in the strategic planning exercise. If it has, these values may need to be re-examined, and a plan developed to incorporate any changes indicated. To do this, and to consider possible new general directions, the head of the institution, with a small team of top management and program/department leaders, should start out with a general "brainstorming" session. This will probably take several days, and may best be done in a "retreat," isolated from the normal pressures and interruptions. In brainstorming, it is important to include a non-evaluative phase of idea generation in order to let concepts flow freely without anyone feeling threatened that his idea will be considered stupid or inappropriate. From this shower of "wild" ideas, the elements of a strategy can begin to emerge, which can then be molded and honed into a plan. The reason this begins from the top down is that the possibilities of embarking on new directions or initiating new programs, sometimes at the expense of phasing out current programs, must be considered among the options; a bottom-up process is most likely to result in a plan that calls for "more of the same," since individual scientists will quite naturally consider what they are currently doing, and the strategies currently employed as being the most appropriate. Once a general mission statement has been decided upon, it will, of course, need to be communicated to the entire staff and clients and a consensus built around it. This is an important task of the research leaders, and the process is a reiterative one, as the consensus-building exercise may well produce desirable modifications to the institutional strategy. A strategy can be effectively implemented only if all players are convinced of its merits, but they cannot all be involved in the early steps of its development.

An important component of strategic planning is to define clearly what are to be the products of the institution and who are the clients, end users and beneficiaries of these products. There is often a lot of fuzzy thinking on this subject. Some people think that (of course) the clients for and beneficiaries of the new technology are farmers; a more accurate analysis will reveal that the client is the extension service (or other organization responsible for diffusion of technology), the end users are the farmers, and the beneficiaries are both farmers and urban consumers. Sometimes the product is not new technology at all, but new methodology, and the clients are other scientists, inside or outside of the institution. And the term "farmers" may well be too general; the value system of the institution may specify certain user groups and beneficiaries, such as small farmers, or women, or the urban poor as deserving of special attention. This will affect the strategy (and those advocating attention to special user groups should also recognize that it will probably be more costly).

As part of the strategic plan, the strengths and weaknesses of the institution should be assessed, and guidelines laid down as to what it will do and what it will leave to others. The latter process has both "upstream" and "downstream" dimensions. On the downstream or upstream side, a clear division of labor and a plan to strengthen the interfaces must be envisaged with respect to the research institution and extension and development agencies. On the upstream side, there must be a clear idea as to what type of fundamental knowledge, methodology or genetic materials can be obtained from other institutions dedicated to working towards the more basic end of the research continuum. There is probably also a "sidestream" consideration, as horizontal transfer of technology and division of labor within regional networks become increasingly viable options. Ruttan (1982) has pointed out that rarely does a research institution have access to all the resources it needs. Decisions must be guided by what the institutions can and must do for themselves and what they can borrow or adapt from elsewhere. A research policy that decides to do everything is unlikely to succeed in doing anything.

Research managers are always faced with the question of whether to do "basic" or "applied" research; and politicians (who usually have great influence on the resources) are often inclined to make simplistic, uninformed statements accusing research institutions of being "ivory towers," insisting they should do only "applied" work. It is not that simple. Wortman and Cummings (1978) have described a more useful classification. They refer to *basic research* as that which develops knowledge with no predetermined use necessarily in mind; *supporting research* as investigations whose usefulness can be only partially foreseen, such as work on nitrogen fixation and its potential application to grasses; *strategic research* as biological, chemical, physical, or social science research aimed at solving major problems affecting several areas of a country or the world; *tactical research* as work at experiment stations in support of on-farm research; and *operational* *on-farm-level research* as the identification, through experimentation on farms, of the combinations of crop and animal production practices that will provide higher productivity and profitability on those farms. I, personally, prefer to refer to "problem-solving," or "mission-oriented" research, combining all of the Wortman/Cummings categories in whatever combination is needed to overcome the highest priority production constraints.

Busch (1983) contrasts two historical models of scientific research: the "Cartesian" and "Baconian" models. In the former, the scientist was seen as a hard-working individual working in isolation, composing grand and elegant theories and only later, with the help of a staff of technicians and apprentices, testing them by observation and experimentation. In the Baconian model, described in "New Atlantis," a scientific community that Bacon called the "House of Salomon" worked together to produce results immediately applicable to the state and the larger society. A somewhat similar dichotomy is described by Pastore and Alves (1980) in which they contrast the "diffuse" model, which characterized earlier agricultural research in Brazil, with the "concentrated" model adopted with the creation of EMBRAPA. In the diffuse model, each research unit tries to diversify its activities, researching many different products and attempting to generate a wide array of technologies. The role of making the proper choice of technology as well as determining optimum systems is left completely to the farmer. This model generates a large amount of information with a low probability of crystallizing it into a new technology. For this reason, the authors considered this model as extremely expensive and possible only in rich societies. The concentrated system, on the other hand, concentrates funds and talents on a few relevant products and specific physiogeographic regions. Clearly the Baconian and concentrated models are the ones most applicable to developing country agricultural research institutions.

Once these basic issues have been decided, a "mission statement," summarizing the work the institution will do, and a statement of working principles, spelling out how it will function, can be drawn up as the institute's strategic plan. This will then be the doctrine within which the working plans can be developed. To be most useful, such a strategic statement should not be too long. The statement of the main business areas of the institution, and of its guiding philosophy and operational principles can probably be set forth on two to three pages. The statement of the general set of strategies which will be followed to accomplish the mission will vary in length depending on the complexity of the institution's programs, but it should usually be less than 10 pages in length.

Operational Plans. An operational plan represents a coherent strategy for how the institution will accomplish its mission within a fixed period of time. Unlike the strategic plan, it must also take into account a realistic estimate of what can be done with the resources likely to be available. Operational plans are usually short-term (annual), and medium- or long-term (5 to 10 years). Given the uncertainty surrounding the outcome of the research process combined with funding prospects, the longer the time horizon of the plan, the more general and subject to later modification it must be. Both annual and medium- or long-term plans are needed, since it is within the framework of the longer range plan that the annual plans are developed. The annual plans are the basis for the preparation of the annual budget, and are thus tied to the budgeting cycle. Following the bottom-up principle, each research section (e.g., grain legume pathology) develops its own operational plan; these are then consolidated into program or departmental plans. In addition, the various support units must develop their own plans, based on the types of support which will be required as indicated by the research units, in an interactive process. Then the total institution plan is developed, based on the individual unit plans, and budgetary implications are calculated. If, as is usually the case, the total plan adds up to more than the institution is likely to receive, appropriate modifications must be made. Thus it is important that plans be quantified and priorities established to the extent possible, and that management is involved at various stages of the process. When, in the final stages of the planning process, or due to shortage of funds during the execution phase, cutbacks are required, the overall priorities and principles of the institution, as spelled out in the national plan and the institution's strategic plan, will serve as valuable guides.

In preparation of the plans, the possibility of phasing out some activities which have fulfilled their objectives, or turned out to be less useful than new activities which could be initiated, should be carefully considered. A good criterion in considering whether or not to continue an activity is to ask: "if we were not already doing this, would we get into it?"

Operational plans for each program/department should be composed of several elements. One is a goal statement, stating, in a brief paragraph, the overall purpose of the research unit. This should be followed by a list of operational objectives. These should be brief, single sentence statements about what is expected to be achieved within the planning time horizon. As far as possible, these should be quantifiable objectives. An example of such a set of objective statements –say for a grain legume program– would be:

- Develop a bean variety with resistance to anthracnose and bacterial blight for region X, and a cowpea variety with resistance to pod borers and yellow mosaic disease for region Z.
- Determine the consumer preferences for cowpea quality in five cities.
- Test the combination of variety A and mulching, in association with maize in 25 farmers' fields, in each of regions Y and Z.

And so on. Once all the objectives have been listed, a program strategy to achieve these objectives, and an indication of the resources required to carry out that strategy, are spelled out.

At the Individual Scientist Level

Once the institutional and program operational research plans have been developed and approved, it still falls on each senior scientist to choose from a wide array of possible experiments those which will form a technically sound and relevant research program in order to meet the stated objectives according to the agreed-upon strategy. Here is where there is still plenty of room for things to go right or wrong. Scientists may, instead of adhering to the national or institutional priorities, respond to the signals of the reward system. Too often these are shaped by developed country standards which tend to cause researchers to design experiments that lend themselves to publication in international journals. This is where a clear set of values established for the institution and adopted by individual scientists as their own, as well as careful staff selection, evaluation and monitoring, play important roles.

Farmer Participation

In discussions regarding research planning, the statement is frequently made that the farmer should be more involved in the planning process. This is true for some but not all stages of research planning. In my view, priority-setting with regard to commodities and regions must be done at the national level, with strong involvement of economists. Problem identification determining which of the many production constraints for the commodities and regions selected should be given greatest attention –should be done by scientists in the field, and in close collaboration with farmers. These should be converted into operational objectives, putting together the operational plans for each program and institution. Once this has been done, each scientist determines what methodology and tactics he will employ in carrying out the specific experiments to execute the research plan. To the extent that these involve on-farm research, the farmer is again an important partner. Thus, farmers are usefully involved in steps two and four, but cannot be helpful in steps one and three. This refers to farmers acting as individuals. When farmers are organized into commodity associations, and especially when such associations help fund some of the research, they can also have a useful influence in the priority-setting process. This would also be true if farmers serve on the governing board.

Monitoring and Evaluation

Monitoring and periodic evaluation of research activities and results represent an important component of research management; they are among the most valuable tools the research leader has to measure accomplishments, detect the need for mid-course adjustments in the plan or execution of it, and provide clues to changes that should be considered in future plans. In this area of management, the research leader has a more complex task than his business executive counterpart. The "bottom line" of profits or loss, and the measurable parameters of productivity and costs, are easier to evaluate in profit-oriented enterprises producing tangible objects than in assessing the efficiency and effectiveness of a research institution. Even within the research enterprise, efficiency (e.g., meeting budget guidelines, carrying out the number of experiments planned) is easier to gauge than effectiveness. While efficiency relates to doing things right, effectiveness relates more to doing the right things, and doing them in a manner that will achieve results, which can be measured only from outside the institution. Although this is not an easy task, it deserves careful attention by research leaders; and although the nature of monitoring and evaluation will be somewhat different depending on the organizational structure and mission of each institution, there are some guidelines that should be useful in carrying out this important responsibility.

Monitoring is a continuous, day-to-day activity. It is done chiefly by the program leaders/department heads who are in daily contact with the scientists. The institution's top management plays its role in this function chiefly by its contact with these supervisors and by reading the various reports generated by the scientists. An important complement to this more formal monitoring, however, is for top management to walk around the institution frequently, stopping to talk with scientists and technicians, giving them an opportunity to talk about what they are doing. This "management by walking about" serves the multiple purpose of monitoring, staff evaluation and staff motivation. The research manager will soon be able to detect whether experiments seem to be outside the agreed strategy; he will also get a good idea as to who is staying in the office and who is in the field or laboratory. And the staff will know he knows. There are also few things that better serve to motivate staff than to see that top management is interested in what they are doing.

Evaluation is done periodically. The entire national agricultural research system and the individual institutions should be evaluated regularly, in addition to the evaluation of programs and individual scientists within the institution. The first two should be evaluated at intervals of five to ten years, and the last two on an annual basis. The system and institution evaluations should be done by teams of external reviewers, while the program and scientist evaluation should be done chiefly within the institution. As in the case of planning, evaluation should be done by those closest to the action, rather than by a special "planning and evaluation" unit. Since this document deals with management of the institution, it is the evaluation of the individual scientists and programs that is treated here.

We have already dealt with some aspects of individual assessment as part of the discussion on human resource management; but that had to do chiefly with personnel matters like motivation and rewards. What is treated here is the evaluation of the research activities and progress as they relate to the overall goals and strategy of the institution. As I advocate interdisciplinary, team research, it is most appropriate to emphasize evaluation of programs; consideration of individual performance must be seen in the light of contribution to the entire team effort.

What criteria should be used for such evaluation? The spectrum includes: amount and quality of new knowledge created; number and quality of publications produced; innovations developed; innovations adopted by farmers; increases in productivity by individual farmers; and national increases in production. When I first joined CIAT, I informed the staff, in a meeting on the day of my arrival, that the success or failure of the center and their individual efforts must in the end be judged by whether or not productivity of the commodities in

CIAT's mandate increased at national and continental levels. Some considered this grossly unfair (and it was), because such increases in productivity would depend on many factors well outside their control. It is also somewhat unfair, although somewhat less so, as a criterion for national research institutions, given the many factors which can hinder adoption of technology and production increases even when the research institution and scientists do their jobs well. These include lack of effective extension services, lack of credit to farmers, nonavailabilty of essential inputs, and inappropriate government policies. Furthermore, when the value system and national goals include such elements as equity, sustainability, and attention to special user groups, increased productivity and increased production are not in themselves sufficient goals. Nevertheless these represent a target that should not be lost sight of, as a mere increase in knowledge or elegant scientific publications, important as they must be, are not adequate measures of success. The nature of the technology produced will to a large measure determine whether or not it is eventually adopted, and whether it will have the desired effects on sustainable increases in production with equity. Evaluation will need to focus on the quality of the research and the measurement of results against the objectives listed in the operational plan; but the overall social objectives of the entire enterprise must always be kept in mind.

How and by whom should the annual evaluations be done? I believe these should be in the form of internal reviews done largely by peers. Such internal reviews fall into two basic categories: program and institutional. The program reviews should be attended by all professional members of the program (or department). There should be intense reviews of the accomplishments and plans of each individual scientist, with hard-hitting questions about methodology, focus and scientific integrity. The institutional reviews should be attended by all the senior scientists in the institution and chaired by one or more of the senior research managers. They should focus on the accomplishments and plans of each program and deal with whether or not the programs are adhering to the established priorities and meeting the agreed-on objectives. The relevance of the work of various components to the agreed strategy should, when necessary, be challenged. Each scientist should be expected to produce an annual report of the results of his work; and these should form an important part of the documentation for the review. The reviews should not be mere "show and tell" performances, but leave ample time for discussions. The discussions should be constructively critical; that is, questions and comments should be frank and incisive, with the goal of highlighting areas needing improvement in methodology or focus, but should not be used for destructive attacks or personal vendettas. It is essential that scientists from all other programs be present for the presentations and discussions for any specific program; this serves not only to insure that staff of the institution are familiar and supportive of the institution-wide program, but also that scientists of the same discipline from other programs are present, since they represent the most important element of peer review. Each of the program and institutional internal reviews should probably be about a week in length. This represents a large cost, but a very good investment.

Of course, the key to effective evaluation and monitoring is the existence of clearly stated objectives and strategies against which the accomplishments and future plans can be measured. This underlines the importance of the previous discussion on research planning. Effective reviews are also the key to the flexible research management and the nature of research organization advocated throughout this document. Without good evaluation procedures, the relatively self-governing, unimpeded carrying out of individual research activities, so essential to creativity, could result in scientific anarchy. Thus this, like other sections of this letter, is part of a piece; just one essential component in an effective research management style.

Organization

ISNAR (1986) reports that organizational issues have figured prominently on the agenda of nearly every country that has requested that Service's advice. Organization of agricultural research pertains to how a national agricultural research system is organized as well as to organization issues within an institution. Here I will deal only with this question as it relates to a single institution with national research responsibilities.

Research Structure

Multidisciplinarity. Before embarking on this important subject, I wish to clarify the terminology. You may have noted that throughout this document I have referred to the need for interdisciplinary research and the use of multidisciplinary teams. This represents a departure from my earlier publications (Nickel 1982, 1988) in which I used the term interdisciplinary in both instances. In doing so, I was following the usage I had found to be most common in the literature (Payne and Pearson 1979; Boger and Boyd 1982; Russell 1982; Rossini and Porter

1979). In this usage, the term "interdisciplinary" was used for both research activities and research teams when the research is done by a group of scientists from several disciplines, mutually planned and executed under single leadership; and the word "multidisciplinary" was used to describe loosely coordinated research conducted by scientists from several disciplines and belonging to distinct departments. Recently, however, a friend and colleague, Dr. Michael Arnold, a better student of the English language than I, pointed out (Arnold 1986) that the prefix "multi-," meaning many, should refer to the structure, and "inter," meaning between, should refer to the process. As an example, he stated that "a multicellular organism achieves functional coherence through intercellular processes for the movement of metabolites." I accept that correction, and thus use the word "multidisciplinary" to refer to the makeup of teams working together in whatever form, "interdisciplinary" to describe how they work together, and "matrix" to describe when the scientists working together in an interdisciplinary manner are each responsible to their disciplinary department as well as to a designated leader of a coordinated activity.

Program Organization. All this is more than mere semantics, because I feel strongly that the best way to do agricultural research is in an interdisciplinary fashion; and the best organization, when it can be achieved, is around multidisciplinary programs. Such programs, whenever possible, should be organized by product rather than by function. In agricultural research, this usually means by commodities or groupings of similar commodities rather than by disciplines. Thus, a rice or grain legume program would be composed of scientists from such disciplines as breeding (or genetics), plant pathology, entomology, physiology, agronomy and economics. A pasture team, because pastures are not the end product, would also need to include some of the animal science disciplines. This is not the way most agricultural research programs are organized. The most common structure is for the scientists to be placed in disciplinary departments and assigned to work as members of interdisciplinary commodity programs or projects, with budgets assigned chiefly or entirely to the departments and the coordination assigned to "leaders" in a matrix-type organization. The greatest weakness of such a structure is that the scientists end up being responsible to two or more "bosses." This is more than academic, since important personnel matters like evaluation and recommendations for salary increases, as well as budget control, are often assigned to the department head. Such division of supervision and funding sources from program responsibility can easily result in divided loyalty, fragmented responsibilities and budgetary gamesmanship.

When considering the organization of agricultural research programs, special mention of agricultural economists is in order. Some research institutions don't even have any; those that do usually place them in a separate economics unit. I believe economists represent an essential discipline, and that they should be fully integrated into the multidisciplinary research teams. It is not their role to merely pass judgment on the value of technology after it has been developed, or to study why such technology was or was not economically viable, or adopted by farmers; rather, they should be full partners in the technology generation process to insure that their biological science colleagues develop the kind of technology most appropriate for the real socioeconomic conditions of the farmers and the market.

When I arrived at CIAT, most of the scientists were already integrated into multidisciplinary commodity programs; but the economists were assembled in a separate economics program. When we disbanded that program, placing at least one economist into each commodity team, there was considerable consternation, mostly on the part of economists outside of the institution. But this arrangement has stood the test of time. Biological scientists have learned much from their social science colleagues, and vice versa; and both types of scientists are enthusiastic about the arrangement. Furthermore, the economists still work together, as needed, when dealing with center-wide and inter-program issues.

Structuring the research so as to have strong, multidisciplinary teams, with well-led commodity programs as the basic organizational unit, has the advantages of intense interdisciplinary cooperation, clarity of purpose, loyalty, and *espirit de corps* not possible with a matrix arrangement. Rossini and Porter (1979) correctly characterize the matrix type of research as a "patchwork quilt" and the interdisciplinary type as a "tapestry." One reason this simple, effective organizational mechanism is not used more is that, for budgetary reasons, many research institutions do not have the requisite number of scientists in the indicated disciplines so as to assign at least one to each of the commodity groups for which they have responsibility. An obvious solution is to reduce the number of commodities for which the institution is responsible. However, even with the planning processes described above, this may not always be possible. In that case, the pragmatic solution is to divide the time of the individual scientist between several programs, but with that scientist assigned, for administrative purposes, to the program in which his services are most needed, and "lent" to the other programs. In an institution with a clear value system understood by all –and effective leadership– such inter-program cooperation should be possible. If, in such an arrangement scientists are also organized into disciplinary units, as well as being assigned to work as part of one or more commodity programs, most of the budget allocations should go to the programs.

Another solution, which I consider less desirable, but which may sometimes be necessary, is to establish one or more "scientific support" units, containing the more "rare" disciplines which each program cannot afford (e.g., virology, nematology, molecular biology) and have these units serve all the programs, but with most of the scientists assigned to multidisciplinary commodity teams. If this solution is chosen, care must be taken so that the real needs of the programs are met by the centrally-organized scientists, and that the latter don't consider themselves as more "scientific" than their commodity program colleagues.

You will have noted that any mention of projects, one of the most common components of the research organization of many institutions, has been conspicuous by its absence from these discussions. The development of projects –with a clear description of the objectives, planned activities for a specified time period, inputs and outputs expected, and an indication of what elements from various organizational units will be involved– is an essential tool when dealing with matrices. Such organization of research by projects is especially important when cooperation among several institutions, or between departments in a university, is envisaged, or when special funds outside the normal institutional sources are being sought. Projects can also be used to describe activities of subsets of programs or interprogram cooperation in the type of commodity program organization espoused here; but I consider them unnecessary, and perhaps even counterproductive when the type of planning and evaluation and monitoring we have discussed is utilized. When the objectives, strategies and annual work plan are clearly delineated for each program, and the methods described above are in place to insure that individual scientists are adhering to that plan and working within the specified strategies, there is little value in asking each scientist to present a group of projects describing the experiments he plans to do. Furthermore, the development of project proposals, the review and approval procedures involved, and the writing-up and reading of project reports take up a lot of researcher and management time that could better be used getting on with the task. Organization of research by projects may cause scientists to do humdrum research that is sure to give reportable results. It also limits the flexibility essential to something as serendipitous as scientific research. However, as long as research institutions are organized along disciplinary department lines, such a project model may be needed.

The IAR Samaru Case. The commodity program model I recommend for research institutions is clearly not applicable to research in universities which, owing to their primary didactic function, are organized by disciplinary departments. Thus research in universities, when interdisciplinary, is usually organized in a matrix form. In a case study used by ISNAR in their management training program (Mckenzie 1983), an interesting example is given of the conflict which can arise when these two models are juxtaposed through the laudable integration of staff between a research institution and a university.

The Institute for Agricultural Research at Samaru began in 1922 as the research and training arm of the Department of Agriculture for the Northern Province of Nigeria. In 1962, when Amadu Bello University was created, the Institute was attached to the University; but it remained semi-autonomous, with its own budget and director, and its own Board of Governors. While the University obtained its funds from the Federal Ministry of Education, the Institute received its funding from the Federal Ministry of Science and Technology. There were, however, strong linkages between the Faculty of Agriculture of the University and the Institute. Institute staff were allowed to spend part of their time teaching and Faculty staff part of their time on research. University postgraduate students frequently did their thesis research in the Institute.

Initially, there was also a close linkage in the organizational structure and management. The 13 sections of the Institute were organized along disciplinary lines, and made up parts of the six Departments of the Faculty. For example, the University Department of Plant Science was directly linked to the Institute's Sections of Plant Breeding, Fiber Breeding and Horticulture. The Institute Heads of Sections were nominally subordinate to the respective Heads of the Faculty Departments, which were joint Faculty/Institute appointments. Under this arrangement, the research activities were totally managed within a single disciplinary structure.

In 1972, a new organization of research programs was initiated. Ten programs, in line with government agricultural research priorities, were established. These were multidisciplinary programs organized along commodity lines. A committee, under a chairman, was set up for each program. Program planning and review was conducted in open sessions of these committees attended by section heads and representatives of the Extension Service. Each program was made up of a number of separate research projects, which were developed at the initiative of individual scientists. Funding for projects approved by the committees as being of high priority within their respective programs was provided directly to the disciplinary section of the scientist in charge of that project. An Institute-wide advisory body to the Director of the Institute, called The Professional and Academic Board, was responsible for seeing to it that the research programs drawn up by the program committees were translated into programs of the research sections. They referred to this multidisciplinary matrix as a "systems approach."

In 1974, the third major change took place when the Institute initiated a new process of preparing its budget proposal by programs. They felt that the government officials could better identify national priorities with commodity programs than with disciplinary sections. Once the budget was approved, however, it was still translated into the requirements of each disciplinary research section and managed by the sections. In 1981, the Director proposed that the research Sections should be disbanded and the Institute be reorganized along program lines. This met with considerable opposition; consequently, a working group was appointed to suggest a suitable organizational structure. The compromise that the group recommended, and that was eventually proposed to the Governing Board by the Director, was that the Institute would be organized by programs (Cereals Improvement, Oilseed Improvement, Grain Legume Improvement, Fiber Crop Improvement, Horticultural Crop Improvement, and Cropping Systems, with individual crops as subprograms), each headed by a Program Leader. While the research Sections would be abolished, each staff member would still remain attached to his existing disciplinary Department. Although the research Programs would be supervised by the Program Leaders, and funding would come through Programs, the Heads of Departments would still be responsible for all appointments, promotions, staff discipline, and the upkeep and maintenance of Department buildings. The Governing Board, at its meeting in October 1983, gave the Director the green light to proceed as he saw best. The case study does not indicate whether he went ahead with the proposal or not.

While this case is complicated by the association with the University, I believe it demonstrates how, without such a linkage, the preferred organizational structure, both for organizational effectiveness and the clarity with which the programs could be understood by

Ministry officials allocating the funds, was shown clearly to be that of the simple, multidisciplinary model.

Regionalization. Your institution, in addition to its central research station, will no doubt need to have some regional substations to adequately cover the agroecological zones of the country. Given limited resources, it is imperative that these be kept to the minimum necessary. A certain critical mass of scientists is essential for a viable research station, and experience has shown that widely dispersed, poorly staffed, intellectually isolated stations, often with poor communications, are not productive. Assuming, however, that there will be a few, well-managed and staffed regional stations, the question arises as to how their scientists should be organized *vis-a-vis* the foregoing discussion on multidisciplinary programs. A viable arrangement appears to be to have the commodity programs organized on a national basis, with appropriate staff assigned to the regional stations, according to the major commodities existing in each region. These would be responsible to their respective national program leaders for technical guidance and personnel issues such as appointment, promotion, and evaluation, and would be responsible to the regional station director for local administrative matters. Each regional station also should probably have a small, multidisciplinary, multi-commodity team to deal with the specific farming systems issues in the region. Those should be directly responsible to the regional director for both technical and administrative matters, and work closely with the commodity program staff. This arrangement is suggested for small- to medium-sized countries. For the largest countries, a decentralized system with more autonomy and stronger ties to the local political and farming community is probably more appropriate.

Unit Size. There seems to be an optimum size of research teams and institutions that produces the highest productivity and creativity. Thus, the most successful large companies use various mechanisms, such as project teams, project centers and task forces to obtain the advantages and innovation of smallness within large institutions (Peters and Waterman 1982). Parker (1977) reported that psychologists and sociologists have pointed to the desirability of limiting group size to between eight to 15. He also indicated that the maximum number of professionals under one director should not exceed 100 to 150. I have found these observations to apply to agricultural research. If the institution becomes so large that the director cannot be familiar with the work of each scientist, it is too large. The research programs seem to function best when the number of scientists making up the multidisciplinary team is within the range given above. Thus, *when* establishing commodity programs, too high a level of aggregation should be avoided. While there are economies of scale in providing some centralized services, such as accounting, purchasing, and maintenance for the entire institution, diseconomies of scale result from overcentralization of some types of functions and decisions, which are best left to the smaller teams. Sometimes a degree of efficiency may have to be sacrificed in order to improve effectiveness; decentralization of some services (clerical) and some decisions (small equipment purchases) illustrate this management consideration.

Hierarchical Relationships

Although the very word has an unpleasant ring in a research institution, and is an anathema to scientists, some degree of hierarchy is essential to any organization. It is the way functions are separated into progressively narrow assignments, with each level accountable to the one above. It is also a communications system, with each level simplifying the information it receives and relating it to other information. It is the way a division of labor can be accomplished through delegation of responsibility and authority.

So, some form of hierarchy cannot be avoided; but care must be taken not to have too many layers, nor should the hierarchy be "steep," with all the decisions made at the top and handed down. The Hughes study (Ranftl 1978) showed a minimum number of management levels to be consistent with effective operation of R & D organizations. Scientists must have direct access to top management; they must know that the head of the organization is familiar with their work and appreciates what they are doing. Management must be able to describe, defend and be enthusiastic about the work of each program, when speaking to the governing body, government officials and donors. This ability is weakened when too many layers are imposed between the director and the scientists. Thus, steep hierarchies, overly complicated organizational structures and rigid administrative procedures must be avoided.

A virtual absence of hierarchy, with decisions made chiefly by mutual consultation, and with "first among equals" leadership, is particularly relevant within the multidisciplinary research teams. This is one reason I prefer the title "leader" over "director" for the person who has the key responsibility of molding and guiding the diverse efforts of such a program.

Delegation is essential to effective research management. Ultimate authority and responsibility for the institute rests with the director; but he can find the time to adequately and calmly carry out the functions which rest uniquely with him by delegating a major part of his authority to his associates. The director has important responsibilities in developing and nurturing the corporate culture and institutional strategy; in representing the institution; in meeting important visitors; in unhurried deliberation to make key decisions. These cannot be performed well by a harassed chief, overly burdened by details that can and should be handled by subordinates. The same is true for the various responsibilities down the hierarchical ladder, so delegation is important at all levels. Delegation is important not just as a time saver, but also because those closest to the action are the ones in the best position to make the right decisions and take the correct actions.

That this is so, and that delegation of responsibility must be accompanied by passing to others commensurate authority, is well known and amply supported in management literature; but it is too seldom practiced. This is one of the most common weaknesses in research management that I have observed in developing country institutions. The managers are usually dedicated, hard-working persons, putting in long hours, but unable to cope with the overwhelming work load. Usually one can not have an uninterrupted discussion with a Third World research director without one of his telephones ringing, or the secretary coming in to ask him to sign an urgent document.

One of the greatest temptations I and other managers face is to take action on a matter that has been or should be delegated to someone else. There are two basic reasons for this error. One is to think we can do the job better than anyone else; the other is to take action on a letter arriving in the "in" box or respond to the request of a staff member who has obtained an appointment to see you, knowing what the subordinate who has this responsibility would probably do and thinking that it would save time to get it out of the way rather than referring it to the proper person. The result is a schizophrenic administration and loss of authority by those who need it.

One of the most common scenes in the ante room of a Minister is a large group of people hoping to get in to see him, or catch him for a moment as he emerges from his office, to ask a favor or to make a decision. The same scene is sometimes repeated outside the office of research managers. The dangers of extreme centralization of authority are demonstrated by Bryant and White (1982) in the quotation of the following statement by Dr. Luis Prieto, former Minister of Education of Venezuela: "When a request is denied by an immediate supervisor, it is sent to the President and approved without even consulting the immediate supervisor. This practice damages the principle of authority and responsibility. It then follows that all interested parties send their request to the highest authority and the immediate supervisors do not make decisions, fearful that they will be overturned." Thus, in yielding to the temptation to take action best left to others, managers themselves contribute to the creation of the circumstances which leave them hopelessly overburdened. The solution to this problem is appropriate delegation; and then not taking back the authority given to others.

The full meaning of delegation also includes the concept of allowing subordinates to make mistakes and supporting them, even when not fully in accord with their decisions. It also encompasses appropriate mentoring, or coaching, to keep mistakes from developing into major failures.

Participatory Management

> *"Fail to honor people, they fail to honor you; but of a good leader, who talks little, when his work is done, his aim fulfilled, they will all say, 'we did this ourselves' "* (Lao Tsu, quoted by Bennis and Nanus 1986).

Involvement of individual members of staff in the process of making decisions which directly affect them is an important component of effective research management. This means giving them a voice in the establishment of administrative policies and procedures, as well as in the relevant research strategies and plans. The ability to permit participation by subordinates and others without their feeling threatened is a recognized characteristic of successful executives (Arygis 1983). It is of special significance in scientific institutions, since scientists by training and inclination are averse to a rigid superior-subordinate relationship. Delegation and decentralization of decision-making and resource management are important components of participatory management; these have already been treated above. Another important mechanism in this process is the use of committees.

The careful use of committees is a very effective tool in managing a research institution. The advantages of shared deliberations include: the development of a closer relationship between the top management and the scientists; the fostering of a feeling of common purpose, shared interests, and a sense of involvement; stimulation of awareness of problems or constraints that the institution faces; improvement of communication between different levels and programs; and more effective decision-making through collective judgment. It also provides checks and balances against arbitrary decision-making by individuals. Committees should usually be of an advisory nature to the person who has the ultimate authority; but those managers who must make the final decision are well advised to act contrary to the collective judgment only rarely and after careful consideration.

The dangers of excessive reliance on committees, especially the great deal of time which these can divert from other activities, are obvious. Drucker (1966) stated, "meetings are by definition a concession to deficient organization. For one either meets or works." For the reasons given above, this is far from true in a research institution; nevertheless the warning it gives is clear. One way to keep time devoted to committee work to a reasonable level is to limit the number of standing committees and rely more on *ad hoc* committees or task forces to deal with single issues, and to disband them once the issues have been dealt with. Another is to chair the meeting more effectively to make sure that time is spent efficiently. Having a clear agenda of the issues to be discussed, and sticking to that agenda, as well as making sure adequate information is available in advance, are key. Ranftl (1978) lists fifteen elements key to conducting productive meetings.

The most important of these are:

- Ensure that meetings have useful content –avoid holding meetings when they are unnecessary– and determine if individual conversations would be more appropriate.
- Keep meetings as small in size as practical –select only attendees who are directly involved and able to deal effectively with agenda items.
- Be realistic about meeting length –end meetings before fatigue sets in. (Try to limit meetings to one hour, and generally never exceed two hours.)
- Keep the meeting in perspective, focus on objectives, control "hidden agendas," avoid pressuring, criticizing, preaching, etc.
- Maintain proper pace –keep the meetings on schedule.
- Summarize at the end of the meeting –state conclusions, recommendations, and actions– assign responsibility and due dates for action items, and close on an encouraging note.

Making Research Effective–A Summary

Although high-quality and well-motivated people are the most important research resource, these, as well as the financial resources, can be used in many ways. Careful planning points these resources in the right direction and channels them into priority areas, while effective evaluation and monitoring procedures ensure that they indeed perform as planned, or adjust the course as required. How the human and financial resources are arranged into organizational patterns greatly affects effectiveness and efficiency. For most agricultural research situations, the use of multidisciplinary teams with a commodity focus seems to be the best organizational building block to facilitate problem-solving research. Minimizing bureaucratic and hierarchic procedures and relationships, and involving staff in decisions related to them, contribute to a creative, productive environment.

CHAPTER V

MANAGING THE FUNDS

Budgets

Process

Budgeting is translating the operational plans into financial terms so that limited resources can be applied in the most efficient manner to carry out the activities described in that plan. It is, therefore, an integral part of the planning process. The programmatic aspects of planning have already been covered, but these must obviously be done in an interactive manner. The process should, ideally, go something like this: A preliminary operational research plan is developed; the plan is costed out and found to cost more than the level of funding that can be reasonably expected, so it is revised accordingly. Then management presents the revised program and budget to the governing board for approval, after which the changes resulting from that process are incorporated and it is submitted to the funding source (usually the national treasury) through the appropriate channels. The requested budget is approved, the funds are made available at the beginning of the fiscal year, and the institution proceeds with the planned activities, with regular reports issued by the appropriate budgeting and accounting section to those responsible for different parts of the budget and management, who monitor that the spending is proceeding within the budget guidelines and funds available.

In practice, it doesn't usually work this smoothly. Frequently the funding source approves a lesser amount, and the program and budget have to be adjusted accordingly. Even when approved, funds may arrive late, or the funding may be further reduced during the course of the financial year. It is in responding to such adverse conditions that management skills are most severely tested. The budget thus represents an important management tool in executing an effective research program in the face of limited resources.

Purpose

A budget serves several purposes, including: applying funds most efficiently; monitoring of the activities and expenditures against agreed plans; managing the unexpected; enhancing cost-consciousness; and facilitating cost accounting.

Achieving these purposes requires not only accurate and careful allocation of costs to planned activities, but also timely and appropriate reporting. Most institutions now use computers to produce budgets and generate reports on expenditures. But these dumb machines do only what they are told and the quality of what they put out can be not better than the information put in. Managers need timely reports of expenditures and trends for the major cost centers and types of expenses. A thick computer printout which provides too much information is almost as bad as one that contains too little. Cost center managers cannot readily detect which items are costing more than expected or are reasonable if the reports they receive don't indicate the costs of different items in a useful manner, but only indicate total unit expenditures; and the accountants can't produce the proper reports if the managers do not tell them what kind of information they need. Thus, useful monitoring and control can be achieved only in an interactive process to constantly improve the quality of reporting.

Many unexpected things can happen during the course of a fiscal year: funds may be reduced; a new discovery may result in the need for additional funds to pursue an exciting new line of work; inflation may be more or less than anticipated (usually more); an expensive machine or piece of equipment may break down. A research manager must be decisive and flexible in dealing with such events. Reserving a portion of the budget (at least 5 percent) for contingencies, and allocating some of the contingency money to be handled by those closer to the action than the head of the institution, are important tools in dealing with the unforeseen. Sometimes drastic restrictions of expenditures will have to be imposed. It is usually better to take such action in a careful, selective manner, rather than "across the board."

Financial management would be easy if funds were unlimited; but this is never the case. Thus, sound research management implies instilling a deep sense of cost-consciousness throughout the institution. The budgeting process, especially clear reporting of what things cost to those responsible for approving the expenditures, is an important component of this continuing process. Charging operational units for the real costs of centrally provided services and materials (e.g., fuel, vehicle maintenance, computer time, telephone, cable and telex charges, central laboratory analyses) is a powerful tool in controlling expenditures. A careful balance must be sought between charging for small items for which the accounting costs exceed the savings that might be made. Computers are beginning to swing this balance in favor of charging the operational units for more of these items (after, of course, allocating appropriate amounts in the budget for such expenditures).

Making charges for central services is an especially useful tool to control expenditure items that seem to be rising abruptly or appear to be excessive. I will give one example from CIAT. As a basic management principle, I have always opposed such charges because of the increased paperwork and administrative costs involved. Thus, gasoline for official center vehicles has long been a centrally funded budget item. However, an analysis indicated that the costs were excessive and unnecessary trips were probably being made. So, we distributed the fuel budget to the operational units and charged them for such fuel. The mileage on center vehicles and fuel costs dropped dramatically, as cost center managers saw that these costs were eroding funds that they needed elsewhere. We are now beginning to do the same thing with telephone costs, since the availability of direct international dialing has resulted in escalation of these costs.

The use of computers and charges of central services can also help get a better handle on what various activities really cost. Better cost accounting will not only make budgeting more accurate, but may affect program planning, since a clearer cost/benefit picture will be available to better assess research priorities.

Problems

Here are a few of the common deficiencies in the budgeting and financial reporting practices I have observed in institutions I have served or visited.

One is basing the budget for a program on last year's budget or, worse yet, on last year's expenditures, rather than on a realistic estimate of the costs of the activities projected. Solving this is easier said than done, as accurate cost estimates are often difficult, especially for new activities; but the growing use of computers should improve the predictability, and make it possible to avoid merely extrapolating last year's budget into the next. Using the previous year's expenditures as a basis may sometimes give a more accurate indication of real costs, but it has the serious potential disadvantage of rewarding programs with overexpenditures and punishing those that have been frugal.

A related problem is how inflation is dealt with. Sometimes, each cost center prepares its budget in the currency of the budget year, so it is difficult to distinguish between real volume increases and inflation estimates. A much better practice is to have each unit prepare its budget in constant currency terms, with the controllers adding the inflation estimates on an institution-wide basis. In this way, a certain level of activity and quantity of supplies –for instance, travel– are approved, with the budget adjusted from time to time to reflect the actual inflation experience.

Another common problem area is to provide adequately for maintenance of facilities and replacement of worn-out or obsolete equipment. For both efficiency and effectiveness of skilled scientists, an amount equivalent to not less than 10 percent of their basic salaries should be allocated for this purpose; otherwise, an expensive resource will be improperly utilized.

Finally, the mirror image of overly generous provision of centrally funded services and materials, discussed above, must be avoided; that is, micromanagement. Budgets should not be used to exercise overly precise control over every expenditure and activity. Detailed project management in which every expense and the use of every hour of the scientists' time must be allocated to a particular project is an intrusion of management into the world of the scientist, where flexibility of resource use and decentralization of resource management decisions are key to creativity and individual initiative. A portion of the time of each scientist and some of the funds at his disposal must be allocated in such a way that he can explore new frontiers and take risks on new approaches which may well produce the breakthroughs of tomorrow.

Resource Mix

An imbalance in the proportion of resources allocated to various expense categories is one of the most serious problems plaguing developing country research institutions. While most, if not all, need more funds, resource mix is often a more serious constraint than the absolute amount of financial resources available. A World Bank study showed that growth in agricultural output had declined severely during the late 1970s and early 1980s. In order to answer the question as to why this had taken place in the face of significant efforts to strengthen national agricultural research in developing countries, ISNAR embarked in 1984 on a major effort to construct a data base of the human and financial resources in a large number of NARSs, and how these have been changing over time. Von der Osten, Pardey and Trigo (1986) reported that the preliminary results of this study suggested a serious deterioration of the factor mixes. They emphasized that this demonstrates a need for better research management.

Capital versus Operations

I have earlier mentioned the need for capital investments and adequate facilities in order to motivate and make most effective use of the precious human resources in a research institution. This can, however, be overdone when a disproportionate share of funds are invested in capital and when the costs of maintaining excessively large infrastructure create an imbalance, making it impossible to pay scientists adequately or provide them with basic operating funds to carry out their research. Ruttan (1986) has also expressed concern about excessive investment in research facilities.

Many developing countries and international financial institutions have significantly increased their total investment in agricultural research, although in some cases this investment continues to decline. Pardey (1986) reported that when comparing the growth rates in agricultural research expenditure for the period between 1970-1974 and 1975-1979 with those for the period between 1975-1979 and 1980-1984, the percentage of countries in which the growth rate had increased had gone from 60% to 70%; 30 countries had increased the rate of growth in such investments, while 17 countries had experienced a decrease . However, this has in many cases not resulted in an increase in operational funds, since much of the increase in expenditure has been due to large loans which usually include a major capital component.

Fixed Personnel versus Other Operational Costs

The most disturbing aspect of the ISNAR study is that it confirms what many of us observing the trends in developing country institutions have suspected; that the expenditures per scientist are actually declining in many countries. The ISNAR data show that 65 percent of the countries are experiencing a decline in per capita support (ISNAR 1986), largely due to a rapid rise in the number of scientists in these institutions without a commensurate increase in their budgets. This increase in staff has occurred chiefly in the medium-sized agricultural research systems. Pardey (1986) reported that the percentage of NARSs with 11 to 50 scientists had declined from 34 to 22, and the percentage with 51-200 scientists had increased from 20 to 37 during the ten-year period between 1970-74 and 1980-84. When the two factors of allocation of a high proportion of increases in funding to capital and increase in the portion of the operating budget devoted to personnel costs are combined, the result is a dramatic shrinkage in the amount of flexible funds left to support the operations of each scientist. In some cases, this has reached the abysmally low figure of five percent (ISNAR 1986). There is no fixed minimum percentage below which operational funds should fall, but no one would doubt that five percent is far too low; even 20 percent is probably too low. A figure closer to the ideal would be 35 per cent.

There are several reasons why, once the payroll for the fixed costs of permanent staff has been met, so little is left for the operational expenditures on supplies, equipment, travel, and temporary labor. These are: pressure for research institutions to take on a large portion of the increasing numbers of professionals leaving the universities; failure to reduce staff when budgets are cut; and maintaining budgets constant while staff costs increase. Much of this lies outside the control of research managers, since it rests on political pressures, government policies and labor laws. To overcome these trends will require all the courage and political support these managers can muster.

Controls

I feel strongly that the head of an agricultural research institution should be an experienced scientist. This practice, however, has a serious built-in weakness; most of us who move from being a scientist to becoming a scientific administrator are woefully weak in training or experience in financial management, and tend to neglect getting involved in accounting and auditing matters in favor of concentrating on planning and management of the research activities. Furthermore, by training and inclination, we tend to sympathize with our scientific colleagues in their aversion to bureaucratic procedures and controls. Nevertheless, we do bear heavy responsibilities for ensuring that the funds entrusted to our stewardship are applied with efficiency and integrity. Thus, we must make sure that the financial management of our institutions is in the most capable hands; and we need to become sufficiently familiar with these matters in order to carry out this aspect of our management responsibilities effectively.

I learned this lesson the hard way. During the first few years of my management of CIAT, I devoted my energies largely to developing and sharpening the focus of our research programs, maintaining contact with donors, and strengthening our relationships with our national program partners, giving limited attention to the purchasing, accounting and auditing functions. Then we discovered that a very clever person had been systematically stealing from the Center. The amount of the loss, as a proportion of the total annual expenditures, was rather modest; but if not promptly and thoroughly dealt with, it could have had a disastrous effect on donor confidence and the moral fiber of the institution. I then had to turn more attention to the question of procedures and controls.

In addition to having very capable and honest persons in the key financial operations, it is essential that an institution have a strong and effective internal audit section. Practically all institutions have external audits executed either by the government comptroller office or by audit firms; but external auditors can only make spot checks and insure that accepted financial practices are being followed. They cannot effectively monitor that proper controls are in place and are being adequately followed. This is the task of internal audit. While internal auditors may, occasionally, discover a fraud, they are not policemen. Their main function is to work with the various sections to help them make sure that they are following established procedures and controls, to promptly plan and execute corrective actions where weaknesses are found, and to identify where and how improvements in procedures and controls need to be made. Their day-to-day operations should be under the supervision of the financial head of the institution, who is most qualified to give proper guidance and oversight; but their direct, formal responsibility must be to the head of the institution, with whom they must meet on a regular basis. They must also have direct, regular access to the governing body of the institution. An effective internal audit section is not inexpensive; it is, however, essential.

I have mentioned procedures in addition to controls, since the controls must be embodied within established, clear and written procedures. Proper procedures do not need to be overly cumbersome or bureaucratic to be effective; indeed, overly complicated and involved procedures can lessen rather than enhance the usefulness of controls. Herzberg (1974) has pointed out that administrative procedures that guard against hypothetical errors and imaginary irresponsibility breed the very carelessness and inefficiency that they were intended to prevent. In CIAT's case, we found that some fraudulent documents had been initialled in good faith by as many as 12 persons who, under the established procedures, had responsibility for approval. When too many persons are involved in the process, the responsibility and the authority of each is diluted to such an extent as to become a meaningless routine.

Dealing With Underfunding

The foregoing discussion of financial management has dealt largely with the mechanics of financial planning and controls –how to project funding requirements, deal with the unexpected, assure an appropriate balance of resource allocations, and insure honest and frugal management of the funds available. Most Third World research managers would probably react to that treatment as superficial, because it doesn't adequately deal with the most important fact of their daily management struggle: critical under-funding. This is a level of serious under-funding which cannot be handled by use of the contingencies line item of the budget, or by imposing minor savings. It has two faces: acute and chronic.

Acute under-funding can be the result of a drastic cut on the budget imposed by the government –frequently the result of across-the-board cuts in all official spending due to some financial crisis. Another, and probably most common, cause of acute under-funding is inflation. Inflation projections are usually based on optimistic government estimates. When the moment of truth comes, and inflation is dramatically higher than estimated, the treasury rarely adjusts the approved budget accordingly. When adjustments are made, they are usually less than the rate of inflation, due to government policies aimed at curbing the inflationary trend. At high rates of inflation, the budget of an institution can be seriously eroded in a matter of a few months. How the research director deals with such an emergency will most seriously test his mettle and affect the success or failure of his institution. If he does not act decisively, the individual scientists will have to. Morale could drop quickly, resulting in the loss of some of the best staff and a general decline in productivity. Those who remain may look for additional funds themselves, thereby distorting the research priorities. Or, they will be forced to do less and to do it less expensively; this usually means restricting their work to the experiment station, or worse, their offices. This results in the work becoming less relevant and the scientists' losing contact with farmers and extension personnel. The best way to avoid such a scenario is to have anticipated it, and to have sought alternative solutions. One solution would be to convince the authorities of the unrealistic nature of the budget and get it increased. Here the manager may need to enlist the help of members of his governing board. If this fails, a revised research plan, adjusted to concentrate efforts on the highest priority activities possible within the realistic funding situation, would be greatly preferred to the *ad hoc* actions likely if such advanced planning had not been done.

Chronic under-funding describes a condition in which the level of funding is seriously below that required for the work plan of the institution year after year. When this situation becomes evident, it is imperative to seek long-term solutions, either by seeking more stable sources and mechanisms for funding or by reduction of programs and activities. One mechanism for more secure funding is to have all or a part of the research paid for by the agricultural community which benefits from it by a levy on certain products, particularly agricultural exports. Another is to provide medium-term funding supplementation from external loans. Based on the rationale that research is a good investment that will produce added income in the future, some governments have resorted to international loans to finance both capital and operational budgets of R & D institutions. Through this mechanism, time is gained to find more permanent solutions.

Whether or not additional funding sources are found, a chronically under-funded institution may well need to reduce its level of activities to more realistically reflect the funding situation. This must be done carefully in light of the long-term strategy of the institution and an analysis of the comparative social benefits that would be foregone by reduction of various alternative programs. The temptation is to take the easy road; to freeze vacant positions –whether or not they are in the most relevant programs– or to implement cutbacks across the board. The effective research executive must resist such temptations.

Making Research Efficient–A Summary

Skillful budgeting is an essential component of the implementation of research plans. Monitoring budget compliance and application of effective controls are essential to ensure that the institution lives within its means. These financial considerations, common to all institutions, are often dwarfed in developing countries by overwhelming problems of under-funding, uncertainty of funding and imbalances in the mix of personnel, operational and capital resources. The Third World research manager faces special problems and requires extraordinary skills as he steers his institution through these troubled waters.

CHAPTER VI

MANAGING THE EXTERNAL INTERFACES

As Director of your national agricultural research institution you will not only need to look internally at the various functions we have discussed, but will also have to devote a great deal of attention to groups outside the institution, for it is only outside the institution that its impact is felt; and it is chiefly from outside the institution that the funds to operate it come, and where policies that govern its role are made.

Clients/Users

Surely a key facet of the institution's value system should be a strong user and client orientation; that is, unless the products are transferred to and adopted by farmers, they remain interesting, academic curiosities without affecting production or human welfare. Active contact with those engaged in the diffusion of the technology and the ultimate users and beneficiaries is essential to ensure adequate feedback to the technology generation process. Usually new technology alone –even when it has been developed with a good knowledge of the real conditions under which it will be utilized– is not enough to be adopted or have a social impact. To be successful, the products of a research system must normally be accompanied by an effective technology transfer system, appropriate government policies, and the availability of credit and inputs. The history of agricultural research is replete with examples of apparently superior technology that never made an impact on production because these factors were not in place.

For a research institution, the users can be defined at both the institutional and field level. From an institutional perspective, the "users" should be thought of as other institutions, particularly farmers' organizations, which will influence the research priorities and funding. At the field level, the users are clearly the individual farmers. Research institutions, depending on their value system and strategy, often identify special user groups (such as low-resource farmers or women) to which they will give special attention. The clients of a research institution, i.e., those to whom the product (new technology) is to be delivered, are clearly organizations which have responsibility for delivery of the technology to the users. This is usually an extension service; it may also be a users' association or a rural development institution. The research/extension interface is of such key importance that it is given separate treatment below.

Agricultural research is most effective when it is a component within a well-developed national plan. Such a plan, developed for each of the most important agricultural products of a country, should clearly delineate what is needed for success and the role each national institution will play in it. While the research institution should have an important role in such a plan, other actors, such as the seed sector, those involved in marketing and price policies, those responsible for agricultural credit, and the appropriate extension and development agencies will all need to work together in a coordinated manner. The leader of the research program dealing with that commodity, or a key member of that research team, is often the best person to take the leadership in developing and promoting the implementation of such an integrated, national plan. In doing so, he will have to cross institutional boundaries and overcome the barriers that often exist among the various agencies involved. Here is where the difference between creativity and innovation seems important. It takes creative people who have vision to generate new technology; and innovative people who have entrepreneurial talents to get it applied. Such innovators must have the tenacity and willingness to persist, despite institutional barriers, to insure implementation. Such people should be identified and used to work with the various agencies involved to turn useful research into results.

The Research/Extension Interface

One of the most important gaps in many agricultural technology systems is that between the research and extension agencies. Some countries have tried to bridge this gap by placing both within the same institution. This is a positive development. I can see no advantage to keeping them separate; it should be easier to develop effective collaborative arrangements between groups within the same institution. But this is not a panacea. Alas, I have observed many countries in which agricultural research and extension were housed in the same institution and yet the cooperation between them was practically non-existent. The problem is more functional than organizational. So, whether they are in the same organization or separate, the essential task is to get them to work together in an effective, integrated and complementary fashion. In Part Two we will be taking up the importance of on-farm research; this arena presents an ideal interface for the two functions. A further mechanism used successfully by various institutions is to place extension personnel, such as extension specialists for specific commodities, within the research institution. Another useful tool is for the research institution to conduct short courses for extension personnel to update them on the latest technology. Any such mechanism should be seen as a two-way street: with research personnel learning about the real needs of the farmers from those working most closely with them, and for extension personnel to become better equipped with the most current scientific knowledge needed for their tasks. What is important is for the distinction between those generating new technology and those responsible for its diffusion to fade into near oblivion, with both groups working toward the common goal of increasing the productivity and well-being of farmers.

Other Knowledge Sources

Within the country, it is important to maintain close ties with universities, other research institutions, private sector research and agro-industrial organizations to keep abreast of their work, avoid unnecessary duplication and develop beneficial collaborative arrangements. Opportunities abound in most countries to capitalize on under-utilized scientific talent and facilities through mutually beneficial interinstitutional collaboration. At the same time, one of the most common and sad truths about institutions in many countries I have visited is the degree of isolation, competition and institutional jealousy that exists. It is one of the greatest challenges for a research manager to overcome these barriers in order to make most efficient use of all the human, physical and financial resources and to stimulate intellectual cross-fertilization.

Horizontal cooperation in research among developing countries is a highly desirable and growing trend. Regional cooperation on commodity-oriented or other types of networks is an excellent way to share information and materials. A particularly useful model for smaller research institutions is to share research responsibilities among members of the cooperative network, with each taking on projects on specific topics of importance to the region for which that institution, by virtue of its specialized staff, facilities or ecological conditions, has a comparative advantage. However, with the growing number of networks being promoted by various organizations, some care must be taken to participate only in those in which there is a clear mutual advantage in doing so, rather than generating more networks for networking's sake, and the consuming of staff time such participation implies.

Finally, professional contacts and cooperative arrangements with institutions in developed countries, when useful to the priority activities of your institution, should be encouraged. There are many highly specialized scientists and facilities in these countries which can be employed to provide basic research support and help solve problems that cannot readily be accomplished with the staff and facilities at your disposal. Various donor organizations are willing to fund such cooperation if projects of clear value to the developing country can be identified.

Policy Makers

Governing Body

In the case of a semiautonomous research institute, the governing body which sets the policies and approves the budgets of the institution represents one of the most important interfaces for a research director (although it might be argued that this is not really an "external" interface). The governing body is an important link to the outside because it represents the clients before the institution and can be used as an important means of representing the institution to the outside world. Hopefully that governing body will be constituted in such a manner as to usefully represent the various sectors which the institute serves. In this respect, it serves the very important function of insuring that the institution really meets the needs of its constituency. One of the weaknesses of the semiautonomous institute model versus having the institution being an integral part of the Ministry of Agriculture is that it may tend to be too autonomous, i.e., engaged in activities not seen as being the most relevant to priority national needs. Since the funds, in most cases, eventually come from the national treasury, the institution must avoid being too autonomous, or its support will soon wither. A representative governing body is a good mechanism to guard against any such tendency.

A well-informed governing body can also be "used" by the research director for a number of purposes. One is to enthusiastically inform persons and organizations outside the institution of the accomplishments and value of the institution. Another is as a sounding board for difficult decisions. Mintzberg (1983), in his useful chapter on the role of boards of directors, quotes Bacon and Brown: "The chief executive occupies a lonesome post; from time to time he must resolve a matter in which he needs counsel, yet may be reluctant to discuss these matters with subordinates." Such issues may usefully be treated with the governing body. At times when drastic personnel reductions are indicated, and political support is needed, that body may also be a valuable partner in this difficult task.

Government

Since the funds for the institution usually come chiefly from government, a research manager must give special attention to keeping appropriate ministry (especially agriculture and finance) and legislative members informed about the programs and accomplishments of the institution. Here the old saying that "nothing succeeds like success" is highly relevant. It will be difficult to sustain enthusiasm and support without tangible results. However, agricultural research is a long-term activity, with the lag time for impact exceeding the normal length of service of key officials and legislators. Therefore, a dual-stream approach may be necessary, including some activities that produce quick results and others that are of a longer-term nature. Whenever possible, key officials should be invited to the institution or the farmers' fields to see the results. On such occasions, brief, results-oriented presentations (preferably in the field) should be made, in preference to long, technical reports. Results must be presented in a manner that convinces government planning and budget officers that agricultural research is an investment with a high payoff in social benefits.

Policymakers should also be informed of government policies that hinder adoption of useful new technology. The economists on your staff will be particularly useful in this exercise.

Funding Sources

In addition to the national treasury, other funding sources, such as local foundations, growers' associations, and external donors must be cultivated.

External, technical assistance donors are worthy of special mention. This is a case where it may be in order to "look a gift horse in the mouth." I don't mean that the well-intentioned desire of these agencies to help should be overlooked or that their assistance should not be appreciated. What is important is not to let offers of financial, material, or personnel assistance distort the research priorities of the institution or impose long-term costs that would affect the resource mix adversely. I mentioned earlier, when discussing the subject of operational support to scientists, that a large portion of external funding for research is in the form of capital, and that the development of excessive infrastructure can impose long-term obligations on the budget. While most such aid is probably beneficial, the Third World is full of bad examples; let me describe a few from my personal experience.

A few years ago, I visited a remote station in a small, impoverished South American country. The station had almost no operating funds; in fact, they had not planted the pasture trials we came to see because there had been no funds to buy fuel for the tractor. At the same time, they were just completing the installation of a million dollars' worth of highly sophisticated laboratory equipment provided by a donor country. Much of this equipment I would never have approved for purchase in the international center I direct because I considered it too exotic for our needs and too expensive or difficult to maintain.

On another occasion, I visited the central research station of a small Central American country that had just completed building an excellent set of research facilities with loan funds from another donor. These were good, and needed; but the greenhouses were of a type of construction inappropriate for the tropics. Following designs developed for temperate regions, they were closed; thus under the tropical sun they required enormous amounts of air conditioning to maintain conditions in which plants would grow. It will be difficult for that institution to meet the recurrent energy costs.

Earlier, in a small Asian country, I witnessed the great difficulty technicians were having in installing a new balance that had been procured under a foreign aid project for the new soils laboratory. It was so extremely sensitive that the passing of trucks on a highway outside the institution affected its readings; a foam rubber cushion had to be installed around it. I doubt whether such a degree of sensitivity was required to meet the urgent research needs of that country.

The point of these example, and I am sure you know others, is that facilities or equipment, even if they come as gifts or as a part of soft loans, are not free. They have operating and maintenance costs; and if inappropriate, they may divert critical resources and the time of scarce scientific manpower from more relevant work.

Building Bridges–A Summary

A research director cannot look only inwardly; he must develop effective linkages with many external bodies in order to ensure the complementarity of various research efforts, facilitate the transfer of research results, promote appropriate policies, and obtain the amounts and kinds of resources needed. Indeed, the head of an institution probably should spend as much, if not more, time dealing with external relations as that spent on matters internal to the organization. It is, after all, from outside the institution that the resources it requires are obtained, and where its effectiveness is measured.

CHAPTER VII

MANAGING THE MANAGERS

Building a Management Team

Well, no one said it was going to be easy! If you've stuck with me this far, you will surely be convinced that being an effective manager of an agricultural research institution is a daunting and complex task and a heavy responsibility. But while it is not easy, I can think of nothing in life more rewarding than creating excellence in an institution dedicated to improving the lives of millions of people. Developing a corporate culture and designing and managing a research program dedicated to the eradication of hunger and poverty will require your best efforts. But the task is too large and the importance too great for any one person to attempt to accomplish it alone. A research leader must build a management team around himself. In the end, choosing and grooming that team to work together with you in leading the institution may well be your greatest accomplishment. The famous industrialist, Andrew Carnegie, chose this epitaph for his tombstone: "Here lies a man who knew how to bring into his service better men than himself."

Working closely with well-chosen lieutenants and grooming them to co-administer the institution with you is of importance beyond the fact that one person cannot do the job alone. It is the means whereby the corporate culture can be carried on after you leave the scene; and since the headship of a research institution is often a political appointment, leaving the scene usually doesn't mean dying, or leaving at retirement age. Various writers on management have stressed that the skills of management and leadership are best learned through experience, so those working closely with the head of the organization are, in effect, serving an apprenticeship that will groom them to be future directors themselves.

I strongly believe in team management. This goes beyond mere delegation, but involves sharing of information, and meeting frequently to arrive at joint management decisions. When a colleague fails to execute a task in the way the chief executive would have done it, it does not necessarily mean that he is incompetent, but rather that he was not in possession of the same background information. Thus, ways of sharing information within the management team, such as copying key correspondence or circulating the "chronological file" of each person on the management team, serve as important means to ensure that all act in harmony.

Earlier, I mentioned the usefulness of the Myers-Briggs Type Indicator in differentiating among individuals according to intrinsic basic preferences. One major division, especially among persons in management positions, is between those who find out things by sensing and those who do so through intuition. The former are more realistic and practical; while the latter are better at coming up with new ideas and possiblilities. It is good if both types are included in the management team.

The head of the institution and members of his management team must all be skilled in three sets of abilities: technical competence, management skills and leadership skills. I shall assume the technical skills as given, and concentrate on the last two.

Enhancing Management Skills

The most important management skills include: delegation, decision-making, concentration, negotiation, and use of time. Delegation has already been treated in the discussion of organizational issues.

Decision-Making

Managers must make many decisions; and good managers make them quickly rather than calling for further studies. Making decisions quickly does not mean making them hastily. Decisions should be made on principle and on the highest level of conceptual understanding. Too often, policy decisions are made to meet a current emergency without sufficient thought on the broader implications of such decisions. Thus, the best decisions are made when all the facts are available and the implications considered, and when alternatives have been adequately explored. These often emerge best in meetings of the management team in which there is constructive disagreement, as disagreement stimulates imagination. The person chairing such a meeting should clearly define the problem, divide it into subproblems when necessary, spell out the facts, and encourage disagreement. He should not start out with the assumption that any one course of action, even the one he initially favors, is right and others are wrong. Usually in such discussions, a consensus on the best course of action will emerge; and if it doesn't, the one having to ultimately decide will at least do so on the basis of informed opinion. Although vacillation and procrastination must be avoided, incubation of the problem is sometimes needed. When this is done, a time limit should be set and a decision taken; sometimes no decision is worse than an imperfect one. Part of the decision process is a plan for implementing the decisions taken, also best done in meetings of the management team.

Negotiation

Most human relationships are in one way or another a negotiation process, and this is certainly true in management of an organization. Setting of priorities, developing the budget, personnel actions, dealing with the labor union, meetings with the governing body or government officials, division of labor between research and extension agencies or between research organizations, or deciding on which of a list of equipment requests should be approved, are just a few examples of the many negotiations that fill the life of a research manager. Like decision-making, these are best done on principle rather than through power. The chief executive is often in a position to "win" in a dispute; but the "loser" is diminished in the process, and the victory may turn out to be a costly one in the long run. Therefore, it is best, whenever possible, to attempt to negotiate in a manner in which both parties win. This means understanding the basic interests of both parties and exploring multiple options, trying to find those in which there is mutual gain. In such "principled," "win-win" negotiation, it is important to separate the problem from the people, focus on interests, not positions, and base discussions on objective criteria (Fisher and Ury 1981).

Concentration

A research manager must deal with many totally different issues each day. A key management skill is to take these one at a time and concentrate full attention to that activity before moving on to the next. To do this effectively means prioritizing activities in order to deal with the most important first, rather than dealing with them in the order they appear in the "in" box. In whatever order they are dealt with, the key element is intense concentration. This is incompatible with a totally "open door" policy in which anyone can come in or any phone call is passed through at any time without any control. Here I basically disagree with various writers who advocate an open door policy as an important leadership quality; but I am open to compromise. Management colleagues, program leaders and individual scientists should have open and ready access to the director, but such access must be limited to certain fixed periods of the day in order to allow consolidation of periods of several hours per day to deal with matters requiring uninterrupted attention. Of course, as with all rules, this should have exceptions, since emergencies will arise when it is essential that someone speak to you, and what he has to say may even affect the matter you are concentrating on; but the general principle should be observed.

Use of Time

The subject of concentration has already got us into one of the most important and difficult problems a manager has to deal with: how to manage the limited time at his disposal. The sad reality about time is that there is no more of it; each of us already has all there is; it cannot be accumulated, saved, or retrieved; and we are forced to spend it at a fixed rate: sixty minutes per hour and twenty-four hours per day. Some executives kid themselves by thinking they are creating more time by working longer hours; but there is a limit to the number of hours a day one can effectively apply the intense concentration required. Beyond that limit, the work is not well done; and taking work home on a regular basis only robs one of the relaxation required to work effectively the next day.

Since we cannot control or create more time, it is ourselves we have to manage in order to use what we have most effectively. Some of the available time will inevitably be required for duties only the chief executive can perform; and these are not restricted to major policy matters. He will have to open meetings, sign diplomas, meet with important visitors who will see no one less than the director, attend social functions; and he cannot say "that's not my job, I'm here to make policy and deal with important issues only." The trick, then, is to learn how to organize and use the time remaining in the most efficient manner. This requires the making of an inventory of how time is being used and looking for the time wasters that can be eliminated. In doing so, we must also look for the way we may be wasting out colleagues' time.

Some useful ways to use time more effectively include: planning and budgeting the work to be done at the beginning of each day; don't waste time on unimportant issues, or on ones that can be delegated to others; try to handle each piece of paper only once —when you pick it up, deal with it, rather than set it aside to handle at a later time.

Enhancing Leadership Skills

Importance and Origins of Leadership

In choosing and developing a management team, leadership skills are probably more important than management skills. Good management will make the institution more efficient; good leadership will insure its effectiveness. Transforming an average institution into one of excellence, converting adversity into challenges and opportunities, shaping vision and making it become a reality, require top executives who are not only visionary, but pragmatic; who are both demanding and sensitive; who are creative, yet realistic. This requires a blend of managerial and leadership talents.

Whereas management skills can be learned through a combination of formal training, experience and example, it is not clear to what degree leaders are "born" or "made." Most opinions I have read on this subject come down on the side of learning; but this learning may begin quite early in life. Ranftl (1978) states that the development of a true leader is a lifelong process heavily influenced by early childhood development. Bennis and Nanus (1986) report that most leaders in their study indicated that they knew early in their lives that they were good with people, and that when they were in leadership roles, they were successful. On the other hand, there is little doubt that, whatever leadership talents one brings into adult life from childhood, these can be greatly enhanced. Hickman and Silva (1984) state that leaders are made, not born, and devote their book to describing the skills required for creating superior organizational performance.

Vision

Effective leaders have vision. Hickman and Silva give an excellent description of what that entails. They state:

The visionary leader:

- Searches for ideas, concepts, and ways of thinking until clear vision crystallizes.
- Articulates the vision into an easy-to-grasp philosophy that integrates strategic directions and cultural values.
- Motivates company employees at all levels in the organization, attempting to understand their concerns and the impact the vision has on them.
- Acts in a warm, supportive, expressive way, always communicating that "We're all in this together, like a family."
- Translates the vision into a reason for being for each employee by continually relating the vision to individual cares, concerns, and work.
- Concentrates on the major strengths within the organization that will insure the success of the vision.
- Remains at the center of the action, positioned as prime shaper of the vision.
- Looks for ways to improve, augment, or further develop the corporate vision by carefully observing changes inside and outside the organization.
- Measures the ultimate success of the organization in terms of its ability to fulfill the vision.

People Orientation

Good leaders are people-oriented. They strive to deal with each person as an individual, with openness and fairness. Because of this, they are respected; people will follow their leadership, even at times in which they may disagree with them. As a result of their attention to people, they can stimulate them to bridge the large gap that usually exists between the status quo and the full potential of each individual.

Being a Winner

Leaders are winners and make others feel like and become winners too. They see mistakes as a learning process; failure is not part of their vocabulary. This does not mean that they do not have weaknesses, but they learn to recognize their strengths and compensate for their weaknesses. Their positive self-regard causes them to nurture their skills and develop their talents. Bennis and Nanus (1986) point out that if we come to expect more of ourselves, then we come to expect more of others, and they respond with better performance. They also refer to the fact that great leaders never think of failure. They call this the "Wallenda effect," referring to the sad case of the great tightrope artist by that name who fell to his death only when he started to worry about falling.

Third World agricultural research managers must be leaders who think of themselves as winners and stimulate those in their institutions to win the battle against hunger and poverty; the stakes are too great to contemplate failing in this task.

The Leadership Team–A Summary

Directing an agricultural research institution is not a one-man show. The effectiveness of of the director will depend greatly on the quality of the persons with which he surrounds himself and the extent to which he is able and willing to delegate responsibility and authority to them. The entire team of the director and his lieutenants will need to possess both management and leadership skills in order to exercise appropriate control as well as to provide vision for the institution and inspiration to its staff.

PART TWO

A RESEARCH STRATEGY FOR SUSTAINABLE DEVELOPMENT WITH EQUITY

CHAPTER VIII

THE ISSUES

A research director must be more than a manager and a leader; he must be a strategist. Most of this letter has been devoted to the first two. In this part, I wish to pass on my views on some issues that must be taken into account when developing a research strategy, followed by a discussion of the implications these have for the nature of a research strategy apt for agricultural research institutions in the Third World.

Developing Country Research

In the 1950s and 1960s, a common misconception in various developing country and technical assistance circles was that the agricultural production technology that had been generated for temperate, industrialized countries could be transferred to increase productivity in mostly tropical, developing countries. That was found to be erroneous in great part. Many scientific methods and management principles are broadly applicable. As Alfred Whitehead (1925) has stated, "The great invention of the 19th century was the invention of the method of invention." That applies universally, and is what we dealt with in Part One. But it was soon realized that technical innovations needed to be designed for the unique ecological and socioeconomic conditions prevailing in the developing countries.

Thus, by the mid-1960s, attention shifted from developing strong extension services to transfer presumably applicable technology to Third World farmers to strengthening research institutions in the development of technological innovations *in situ.* A personal experience will serve to illustrate this shift.

When, in 1961, I joined USAID as a young entomologist, to be assigned as an "entomology advisor" to what was then Cambodia, I was given the usual three-week orientation in Washington. During that time, thinking it would be useful to know as much as possible about the insects of that country and what had been done before in entomological research in that region, I asked my superiors for time to do some bibliographic research in the USDA Library. I was told that that was unnecessary, since my task would not be to do research, but to help develop an extension service to demonstrate known insect control methods to farmers. Once in Cambodia, it became evident that little was known of the crop pests or their natural enemies in that country, nor of locally applicable integrated pest management methods; so, field surveys and experiments would be necessary. In reporting on this work, I had to be careful to label these experiments as "demonstrations," since "research" was not a legitimate activity. A few years later, USAID became one of the strongest supporters of research in developing countries and the strengthening of the institutions to carry out this essential task under local conditions.

What is different about conditions in developing countries that would affect the type of research strategy most relevant there? There are both socioeconomic and ecological issues.

Socioeconomic Conditions

One important factor is the much more rapid increase in demand for food in developing countries. This is a result of two simultaneous phenomena. One is the more rapid increase in population, especially in recent decades, when improvements in medical services and cleaner water have greatly reduced infant mortality, something not yet compensated for by reductions in birth rates. The other is the increase in consumption of basic foods (along with the changes in consumption patterns resulting in greater use of cereals for production of animal products), which is the natural result of economic development. Current rates of population growth average 2.1 percent in developing countries, *vs.* 0.6 percent in developed countries. These translate into estimated increases in demand for food and feed in developing countries in the next two decades of 2.7 percent per annum (Paulino 1986). This is a more rapid rate than the pace of increases experienced in the developed countries at the time agricultural production was rising precipitously. Thus, the challenge of achieving production increases to meet the demand for food and fiber in the forthcoming decades is a formidable task.

Another factor is the nature of the farmers themselves. Because of land tenure problems, a different urban/country ratio, and higher population densities, most Third World farmers cultivate smaller parcels and/or have smaller numbers of livestock; for convenience, they shall be referred to as "small farmers." They also have less capital at their disposal, so I shall call them "poor" farmers. This means that they are less able to take risks and are less able to invest in purchased inputs or labor-saving machinery. Such poor, small farmers also generally have a lower level of education than their counterparts in industrialized nations; this does not mean that they are less astute, but it does affect the complexity of the production technology appropriate for them and the nature of diffusion of technological innovation.

Infrastructure is also less developed. Less-developed marketing structures and poor transportation facilities increase the margin between farm gate and retail prices. Less-developed transportation, particularly roads, increases the costs of production inputs and limits access to them. The combination of lower farm gate prices and higher input costs greatly influences the input/output factors.

High rates of population increase and the inability of urban industries to absorb the growing numbers of people joining the labor force in many cases result in greater unemployment or underemployment; thus, employment generation becomes an important factor when considering what the best innovations are. This is compounded by the shortage of capital, reducing the value of capital-intensive *vs.* labor-intensive technology.

This brief summary of socioeconomic factors in developing countries demonstrates that they differ greatly from those of developed countries; they also differ greatly from country to country, or region to region, within the Third World. This heterogeneity underlines the need for *in situ* research to develop innovations most suitable to local circumstances.

Ecological Conditions

Much of the developing world is geographically tropical. The edapho-climatic conditions of the tropics have an important influence on the nature of technological innovation most suitable to these regions. High rainfall in many areas has leached the soils, so that many are badly depleted of nutrients and are frequently highly acid, with the attendant saturation of the exchange capacity with aluminum ions. High rainfall and the attendant leaching also affect the efficiency of fertilizers. The same high rainfall, often coming in intense tropical downpours, along with the fragile nature of many of the soils, increases erosion hazards. Where the opposite –insufficient rainfall– is the case, the lack of well-developed irrigation facilities make drought an important factor in agricultural production.

Tropical conditions seem to be particularly propitious for the development of plant and animal diseases. Splashing rainfall disseminates plant disease organisms, and high humidity promotes their growth. Year-round growing conditions also accentuate the buildup and carry-over of disease organisms.

The tropics are well known for the abundance of their invertebrate fauna. Many more species of insects, for instance, are found in the tropics than in temperate climates. From the standpoint of pest control, this is not all bad; it also means that there is an abundance of natural enemies. Thus, particularly in the centers of origin of the host plants, a balance has evolved that limits the destructiveness of the many species of pests. I personally have many times observed unsprayed fruit trees in the tropics with several species of scale insects, but with individual branches showing the residual effects of severe local outbreaks that had subsequently been controlled by natural enemies. These might well have required regular chemical control if a disruption of the balance resulted in their spread. As an entomologist dealing with apple, pear and walnut insects in temperate California, I have also noted the speed with which certain plant-feeding insects, like aphids, increase their populations in the spring. This is because the few phytophagous pests that have survived the winter can increase rapidly on the first leaves while the entomophagous insects, having little to feed on until the population of their prey builds up, take a while to reach a population level so as to be effective as natural control agents. This difference in the population dynamics favors biological over chemical control in the tropics. It does not mean that insects never reach damaging levels requiring treatment under tropical conditions; but does mean that such artificial control is less often essential, and that the implications of such treatments must be taken into account.

For both insect and disease control, tropical developing country conditions have several other implications that are different. One is that the washing effect of intense rainfall seriously limits the efficacy of chemical control applications. Another is that less-educated farmers are less likely to read and follow printed warnings (if they exist) on pesticide containers. This danger is compounded by the fact that, under the hot, humid conditions of the tropics (and with the limited resources of the farmers), it is much less likely that protective masks and clothing will be used properly, increasing the toxicity hazards to persons applying the control chemicals. Furthermore, the infrastructure to monitor pesticide residues adequately is usually poorly developed, increasing hazards to consumers.

Like the socioeconomic factors, the agroecological conditions of tropical, developing countries are so very different from those of the temperate regions –and are so heterogeneous– that most agricultural production technology cannot be usefully transferred without substantial local adaptation. *In situ* research is clearly indicated; and the strategy for that research must be considerably different than a research strategy designed for temperate, developed countries.

Large or Small Farms

The fact that holdings are smaller, and that small farmers have fewer resources to control the conditions under which they operate, also affects the suitability of research station results. Large, mechanized, irrigated farms have more in common with most experimental stations. Technology developed on such stations is more directly applicable, and can thus be more readily adopted by large farmers.

The degree of diversity among individual, small farms, without the resources to diminish such diversity partially by the use of fertilizers, soil amendments and irrigation, is another factor affecting the strategy and per unit costs of agricultural research if the goal is to help small farmers.

Irrigated versus Rainfed Agriculture

The large and highly beneficial impact of the "Green Revolution" technology was partially due to the initial emphasis on irrigated wheat and rice. This emphasis was logical, since it gave a quick payoff to research investments. However, much more attention is currently being given to these crops under nonirrigated conditions. Furthermore, both national and international research organizations are devoting a much larger effort to a wide range of other commodities grown chiefly under rainfed conditions. This makes the task of increasing productivity much more complex, and greatly affects the research strategy employed.

Fertile versus Marginal Soils

Most of the world's most fertile, high-quality land is already under cultivation. Part of the research efforts will, of course, be directed to increasing productivity on these prime lands, along with "maintenance research" to protect gains already made. However, as population pressures push production into more marginal areas, increasing attention must be given to developing technology for soils that are less fertile, physically more difficult to manage, and that contain various toxic elements. This must be done in the face of a growing depletion of the most readily accessible mineral deposits and rising energy costs. These developments will clearly have an impact on research strategy.

Stability of Production

Increasing production and productivity is not enough; such increases must be stable. Stability of production can be divided into three basic categories: spatial, temporal and systems.

Spatial stability of technology is commonly referred to as wide adaptability. This is highly desirable in terms of research efficiency, as it means that the cost of generating such innovations can be spread over a large area, reducing the per unit costs of the technology. The new wheat varieties developed in Mexico, which had such a major impact on wheat production in Asia, are a good example of such technology. One reason for this success may lie in the fact that the selection of successive, alternating generations in the breeding process was purposefully done in two ecologically distinct regions of Mexico. The new rice varieties developed by IRRI were also quite widely adopted in irrigated rice production areas, but have been less successful than wheat in covering the area devoted to the crop, due to the broader range of conditions under which rice is grown and the greater diversity of consumer preferences (Biggs 1986).

But individual farmers care little whether an innovation is useful in some other country or region; what concerns them is whether what does well today will do so in future years, when climatic and biotic conditions may be different. This is what I mean by temporal stability. (Here I am concerned chiefly with year-to-year variation in performance; longer-term stability will be dealt with under the section on sustainability). While traditional production practices result in low levels of productivity, they have the merit of being relatively stable under changing conditions. The trick is to increase productivity without losing this stability.

Systems stability refers to the ability for a new technology to perform well under different farming systems and a range of management practices. Most small farmers grow more than one crop in association; and these associations often involve a bewildering range of species. They also vary considerably in the quality of management, such as weed control, land preparation, and pest and disease control. No research program can afford to develop a different technology for each of these combinations; innovations must be sought which will be stable across a range of them. Going for high harvest index (the portion of the total biomass converted to the economic portion of the crop) has been one of the best means to develop high-yielding varieties; but this often reduces the ability of the crop to withstand adverse conditions. Combining higher biomass production with higher harvest index may be possible by selecting for greater photosynthetic efficiency. Recent development with cassava (CIAT 1987) shows promise that this is possible.

Sustainability

Sustainability of production is a matter of growing worldwide concern. I am convinced that this is not just another "fad" which will soon be replaced by another, but a genuine, serious problem which will be with us more and more as we attempt to meet the needs of a growing population on a finite planet. The great challenge to research strategists and scientists is to find ways of increasing production in a manner that these gains are not purchased at the unacceptably high cost of robbing future generations of the natural resources to attain the quality of life to which we all aspire.

The problems faced in this regard are formidable. Population pressures in many places are leading to unsound farming practices, resulting in high rates of soil erosion and overgrazing; salinity is increasing in many areas with increased reliance on irrigation; and forests in several regions are being decimated. Better incorporation of agroforestry practices will need to be investigated in order to help overcome some of these problems, as well as the growing shortage of firewood (Lundgren 1983). The potential problems of genetic erosion and of genetic vulnerability of large areas devoted to a single variety must also be dealt with. Greater investment in genetic resource conservation, more work on associated cropping and varietal mixtures, and more attention to stable resistance mechanisms are indicated. I will not try to complete the litany of present and potential problems here. Excellent, more detailed treatments of the problem of sustainability and possible solutions can be found in recent publications by the World Resources Institute (Dover and Talbot 1987), the Worldwatch Institute (Wolf 1986) and the Technical Advisory Committee of the Consultative Group on International Agricultural Research (1987).

A partial solution to the environmental problems resulting from expansion of production onto lands unsuitable for agriculture is through new technology which improves productivity in more suitable areas. On a recent visit to Germany, I was surprised to learn that much of the land now in forests was cultivated in the Middle Ages. The ability to return these marginal areas to more proper land use patterns, despite a much higher population, can be attributed to development of much more intensive systems of agricultural production. A history of this intensification is described by Brinckmann (1922). Of course, much of this intensification has been accomplished through the use of large amounts of agricultural chemicals. Attempting to increase the intensity of the utilization of the less fragile areas at lesser rates of purchased input use than the current farming practices of Europe and North America presents a difficult challenge.

As Bryant and White (1982) have pointed out, developing countries will, understandably, find the concerns about sustainability heavy with irony. Just as many of them are on the threshold of modern advances in industrialization, they are told to beware of the environmental hazards which lie in its wake.

But the environmental concerns and questions of sustainability of agricultural production are not limited to the developing countries. There is growing and justified concern that the agronomic practices in North America and Europe cannot be sustained without serious costs to future generations. All of us involved in agricultural research, wherever located, will need to take these issues seriously; we must become more sensitive to the environmental impact of the products of our research and develop a stronger "sustainability perspective" in all the research programs.

Whether in developed or developing countries, agricultural researchers, especially those in countries with high degrees of urbanization, face not only the challenge of these reasonable concerns, but will have to do so in a political climate increasingly affected by persons with little understanding of the problem. Hite (1983) has perceptively observed: "Farming by its nature rearranges nature, substituting controlled environments for wild, pristine environments. Urban people are sometimes quite genuinely shocked when they see what farmers do to a rural landscape. That makes them susceptible to anyone who raises a public outcry, legitimate or not, regarding farming practices that make use of unfamiliar chemicals or produce nonaesthetic changes in the natural environment. Farmers, an apolitical minority whose national electoral strength is hardly worth cursory attention, will be living with public policies that are fashioned not in response to their practical needs but the perception of urban voters."

Farmers, especially those with more limited resources, cannot be blamed for actions based on a relatively short time frame. Because of the nature of their land, labor and capital resources, and their short-term needs for food and income, it is not reasonable to place on their shoulders the responsibility for current sacrifices in the interest of sustainability. Rather, this must be built into the technology generated for their use, aided by appropriate government policies which will make such technologies attractive and encourage their adoption.

Equity Considerations

Implicit in most agricultural research is the expectation that its benefits will be equitably distributed; often there is the additional goal that these benefits should somehow be skewed toward the poor in order to help overcome existing disparities in income.

In the various statements made regarding research management in Part One, especially those related to value systems and personnel motivation, some were made to the effect that the work of the institution would contribute to alleviation of poverty. This is based on two important, and rather sweeping, assumptions that need to be explored. The first assumption is that the results of agricultural research *should* benefit the poor; that a bias toward addressing the problems of poverty is part of the national, and therefore institute, policy. I believe this is a fairly safe assumption; at least, if we base it on the rhetoric of most national leaders. Most government officials in developing countries would probably agree that the goals of development include doing various things to overcome the deprivation of the very poor. Baum (1985) has listed the goals of agricultural development to which most countries subscribe as: growth, sustainability, stability, equity and efficiency. Michael Todaro defined development as "a multi-dimensional process involving major changes in social structures, popular attitudes and national institutions, as well as acceleration of economic growth, the reduction of inequality and eradication of absolute poverty" (Bryant and White 1982). It is within this context that I address the issues of agricultural research in relation to development.

The second assumption, which I generally accept, but which is more likely to be questioned, is that improved agricultural production technology *would* benefit the poor. A related question is whether not it is possible to so design technical innovation that it will preferentially benefit the poor; that is, have a pro-poor bias in its impact. How these questions are answered depends chiefly on government policies; but the national and institutional agricultural research strategy can have an effect.

New technologies are clearly an important ingredient in the raising of living standards. This was well summed up by Nobel laureate Theodore Schultz, when he stated, "Living standards in modern Western countries are orders of magnitude, superior to those of the early seventeenth century. Had the triumph of the market meant only a more efficient use of the technologies and resources then available, the gains in living standards would have been minuscule by comparison. What made the difference was the stimulation and harnessing of new technologies and resources" (Landau 1982).

The question of whether a particular innovation benefits the poor preferentially, benefits all equally, or benefits the rich at the expense of the poor, has been debated mostly in relation to the impact of the new wheat and rice varieties in the so-called Green Revolution. Lipton (1985) thoroughly dissected this topic and analyzed the vast literature which has been generated on it. While expressing various reservations and qualifications regarding the modern varieties (MVs) aimed at improving the impact of future work of this nature, his overall conclusions were that: *"small farmers* (often after a time-lag) adopted no less widely, intensively or productively than others;" "MVs increased *labor use* per acre-year, and especially hired employment," and "poor people's *consumption and nutrition* were better and cheaper with MVs than without them." He also stated that "the general fear that 'going for yield' must increase vulnerability (and thus harm the poor) is unfounded. 'Going for intensity,' however, may increase pest and disease attacks." The area found most unequivocally to have pro-poor benefits was that related to the genetic resistance to insects and diseases incorporated into the MVs. Lipton stated, "MVs...enormously reduce disease and pest loss and risk. This helps poor farmers, most vulnerable and with least information and cash, more than the rich." Another important point made by Lipton, of interest to research strategists, is the need, when dealing with pest problems, to work on "important" problems (those taking 5-10% from 100% of the farms), instead of "interesting" ones (those taking 80-100% from 2% of the farms).

Some may question whether it is fair to expect scientists to add equity considerations to their already considerably crowded and complex research agenda, or to blame them when apparently scale-neutral technology benefits large farmers or rich consumers due to government policies outside their control. Obviously, agricultural research alone cannot be expected to resolve all the social problems or inequities; but I am convinced that research policy and strategy can be designed in a manner which will direct the benefits so that they contribute to poverty alleviation. These can be divided into three components: choice of commodities; choice of regions; and research strategy. Choice of which commodities will receive highest priority can be done in a manner favoring those which figure most importantly in the consumption patterns and market budgets of the lower income sectors. Choosing to concentrate limited resources most on those commodities produced mostly by poor farmers, and deciding to work most in those regions where small farms predominate, will also help tip the balance of the research towards a pro-poor bias. The commodity and region choices are usually made at the national level. What is most important for the manager of a research institution is the type of innovations generated for those commodities and regions. It is possible to develop technologies that will give a comparative advantage to low-resource farmers and poor consumers. How to do this is an important component of the research strategy described in Chapter 9.

Biotechnology

The new developments in the field of molecular biology and tissue culture, commonly grouped under the term "biotechnology," offer opportunities, but also present some important problems which are a matter of increasing concern to Third World agricultural research leaders. Some of the possible applications of genetic engineering of particular relevance to developing country agriculture include improved drought tolerance, enhanced nitrogen fixation, improvement of photosynthetic efficiency, and the use of monoclonal antibodies for identifying viruses and indexing of materials for their presence. Tissue culture applications include: cleaning up of existing materials from diseases, rapid multiplication and *in vitro* storage of clonal material, increase of genetic diversity through somaclonal variation, and the use of another culture to accelerate the process of genetic improvement, especially for climatic regions permitting the growth of only one generation per year. A more complete description of biotechnology applications most relevant to developing country situations can be found in the proceedings of a seminar held on this topic at the International Rice Research Institute (IRRI 1985).

Problems occur with the high costs and the high degree of privatization of this type of research. There is growing concern that developing countries will, for these reasons, lag behind the developed countries and multinational companies and end up having to buy the technology. This fear is compounded by recent court decisions permitting the patenting of genetically engineered materials. Only the largest of national agricultural research systems can afford to mount major efforts in this field; and even in these, there is a danger of diversion of funds that are needed for more traditional work on priority subjects.

The large commercial companies will probably only be interested in crop and animal products with a large market; thus the problems of rights on products of biotechnology will probably not be important for many of the crops grown exclusively in developing countries, especially those grown largely by small farmers. By the same token, the important work on basic mechanisms required for practical application of biotechnology may be neglected for those crops. The international agricultural research centers are actively developing a modest capacity in this field in order to serve as a bridge between the more basic work going on in molecular biology and its practical application; and to use tissue culture techniques directly in their collaborative activities, such as safe transfer of clonal materials. In order to benefit from such collaboration with these centers and other research institutions, developing countries will need to develop some facilities and competence in tissue culture. This is not too expensive; but they should be very cautious about embarking on research in genetic engineering. Even in the United States and Europe, most of this type of work is beyond the resource limitations of public-funded institutions.

Strategy Considerations–A Summary

Any research director must be a strategist; but the task of designing a strategy in developing countries is complicated by a number of considerations. It is particularly important in these countries that technological innovations be designed so as to contribute to the alleviation of social problems rather than exacerbate them. Much of the agriculture in developing countries is in the tropics; and population pressures are pushing production into more fragile ecosystems under conditions marginal for sustainable production. These elements accentuate the environmental considerations that must accompany any research strategy. Biotechnology offers new tools to deal with some of these problems; but it brings with it the dual danger of being left behind if not enough is done, and of diverting scarce resources from more immediately applicable research if too much or the wrong kind of research is done in this promising field.

CHAPTER IX

RESEARCH STRATEGY IMPLICATIONS

Minimum Inputs

Clarification of the Philosophy

A strategy that emphasizes the generation of technology minimizing the need for purchased inputs is responsive to many of the issues touched on above. As I have briefly described in an earlier publication (Nickel 1984), emphasis on minimum input levels is an important component of an environmentally sensitive, equity-friendly research strategy. When I first began promoting this approach, in the mid-1970s, it was met with deep concern and some criticism by those who felt it advocated a return to the pre-Green Revolution breeding strategies of trying develop high-yielding varieties with no inputs. It was rightly pointed out that the great advances made with the new, fertilizer-responsive varieties would not have occurred had breeders continued along this line, or had government officials not made bold moves to make fertilizers available at the time the new wheat and rice varieties were disseminated. There was, and continues to be, concern that too much talk about minimum inputs will lull uninformed policy makers into complacency by leading them to think that there is an easy way out; that agricultural development can somehow be accomplished by a combination of peasant wisdom and organic farming, without investments in research, chemicals, and irrigation. These are all reasonable concerns, but based on a misinterpretation of what was meant by this philosophy. I will, therefore, begin this section by a discussion of what is meant, and what is not, by this approach.

It *does not* mean that I believe there is a "free lunch," especially as regards fertilizers; that somehow the world will be able to feed itself and eradicate poverty without the use of any purchased inputs. It *does* mean that there is much that can be done in technology design that makes possible major increases in the production of certain commodities with low levels of input use, and that makes more efficient use of the materials applied. Much can still be done to better exploit existing genetic variability, and additional variability which will become available through biotechnology. Looking for the solutions to production problems in the "seed," rather than in the "bag," is an important component of a minimum input approach.

It *does not* mean that small farmers should not be encouraged to use fertilizers and other cost-effective inputs. Indeed, small farmers, of all people, can benefit most from judicious use of agricultural chemicals in order to get the most in terms of sustainable production from their small parcels. It *does* recognize that many small farmers, even when they should use such inputs, will not have the resources to buy, nor have access to, technology designed for small farmers, so that they will not be dependent on their use. It recognizes that access to the benefits of scale-neutral technology is limited to the extent that its efficacy is contingent on factors not available to many farmers.

It *does not* mean to imply any criticism of the initial emphasis by IRRI and by Borlaug and his colleagues on developing new rice and wheat varieties that would respond to fertilizer applications. Indeed, as Lipton (1985) has stated, "If the farmers of the Third World today used the same cereal varieties as in 1963-4, and everything else were unchanged, then tens of millions of people would this year die of hunger." It *does* mean that there are now other, more complex tasks on the research agenda of national and international research institutions; in fact, even while the initial new rice and wheat varieties were being developed, other work was in progress at these institutions in line with this agenda. Resistance to diseases was always an important feature of the new wheat varieties. And there has been much progress since IR-8. The results of these efforts took longer to achieve, but have since had a major impact. In rice, for instance (IRRI 1986), IR-42 is described as a variety that is particularly good for subsistence farmers because it yields well with fewer external inputs. It is said to yield more than other varieties in low fertility and problem soils and is known for its efficiency in utilization of applied nitrogen. Many of the newer rice varieties have a much broader range of resistances to pests and diseases than the old varieties. It is asserted that IR-36's insect resistance has saved farmers nearly 500 million dollars in reduced insecticide use.

Components of the Application of the Philosophy

The minimum input approach can best be understood if it is divided into its various components. The most important areas in which this philosophy can be applied in crop production are: plant protection, use of soil amendments, tillage practices, and fertilization.

Plant Protection. Genetic resistance to pests and diseases stands out as one of the most obvious areas in which research can find ways to reduce the amounts of purchased inputs required to overcome important production constraints. For many crops, pests and diseases represent the most important source of pre- and post-harvest losses. Many examples of successful application of this principle already exist. This approach is not new, but is being applied to a much greater extent with the greater availability of large gene banks of the major crops in the international centers. It has taken on greater urgency due to the growing problems of pest and disease resistance to various chemicals, induced outbreaks caused by destruction of natural enemies, and environmental and health hazards of excessive pesticide use.

Genetic resistance is, however, not a panacea that will solve all problems. In many cases, resistance sources have not been found, or the resistance may be overcome by changes in the organisms; thus, pesticides will continue to be an important part of the plant protection arsenal. They must, however, be used in a rational manner, in integrated pest management programs. Genetic resistance is an important component of such programs.

Soil Amendments. Tolerance can also be found to various soil problems such as acidity, alkalinity, salinity and various toxicities. The problems of soil acidity are particularly widespread in high rainfall areas of the tropics. The traditional way to combat this problem has been to apply large amounts of lime to the soil; but liming is impractical in most places due to high costs of transportation of large volumes through relatively underdeveloped road infrastructure. In many cases, a better solution is to choose crop or pasture species or cultivars that are well-adapted to acid soil conditions. Doing so not only has the advantage of eliminating or greatly reducing the amount of lime needed, but has several beneficial ancillary effects. One is that keeping the soil acid makes it more practical to use less expensive and more readily available rock phosphates to provide the phosphorus fertilization often essential in tropical soils. Under the acid soil conditions, the applied rock deposits are gradually acidified, making soluble phosphorus available to the plants. Another advantage is that resistant plants are better able to place their roots deeply into the soil, beyond the depth that would normally be affected by liming, and thus achieve better tolerance to periods of drought.

Tillage. Reduced or zero tillage is a practice of growing importance in agriculture. Its main advantage is sometimes thought to be in reducing energy costs, especially of nonrenewable fossil fuels consumed in normal, mechanized tillage practices. These savings may be less than some assume, as the use of fossil fuels to produce the herbicides normally required in zero tillage operations represents a considerable energy and cost factor. What is probably of greater significance with reduced tillage is the reduction of soil erosion and the increases in organic matter gained from such practices. Organic mulch has long been known to have many beneficial effects on the physical and microbiological characteristics of soil. They have also been shown to be very beneficial in control of plant diseases spread by splashing of rain on bare soil. But farmers have been reluctant or unable to engage in the very laborious task of cutting plants and carrying them to the fields to provide such mulch. Production of mulch *in situ* with zero or reduced tillage methods provides a very promising alternative. The use of alley cropping as a part of reduced tillage is showing promise as a practice which can make available nitrogen and other nutrients serve as barriers to erosion, and provide sources of firewood. More work needs to be done to identify the best species of plants, particularly legumes, for different agroecological conditions.

Fertilization. Reduction in the need for fertilizers is probably one of the most controversial components of a minimum input philosophy; and the one most difficult to achieve. It is recognized that what can be done in this area is limited; but given the importance, and the even greater future importance, of fertilizers in crop production, anything that can be done to reduce the requirements for and increase the efficiency of fertilization will be of great significance.

There is no escaping the fact that increased crop production will require more nutrients; doing so without adding the nutrients will result in "soil mining," and eventually destroy the usefulness of the soils. But this is only part of the story. Developing plants that use the nutrients they receive in a more efficient manner will reduce the additional amounts needed to increase production. There is evidence of genetic difference in such intrinsic nutrient requirement for some species, even with current technology. More progress is likely, though probably not soon, with genetic engineering methods.

Another important consideration is the reduction in the great amount of fertilizer that is wasted through leaching and volatilization, and by being tied up (in the case of phosphorus) in unavailable forms in the soil. The work with pellets and slow-release fertilizers is promising. Much more needs to be done in this field.

The other most obvious way of reducing nitrogen fertilization is through symbiotic fixation –for use in the plant– of the nitrogen freely available in the air. This has been effectively developed for several grain and forage legumes, and many strains effective for specific cultivars and species under tropical conditions have been identified. More needs to be done to make storage, transport and application of the inoculants more efficient and effective under tropical, developing country conditions. One important problem is that some grain legumes still do not fix significant amounts of nitrogen, especially under the lower altitude tropical conditions. Breeding to select cultivars more effective in nitrogen fixation is making progress, but it is slow and complicated by the fact that, although the N in the air is free, the process that makes it useful to the plant is not; it requires energy the plant may need at the same time to produce leaves, flowers and seeds. Thus, this topic is particularly difficult with crops having a very short growing season. For the same reason, I am not optimistic that crops other than legumes will be induced, through new genetic engineering techniques, to fix nitrogen without loss in crop yield in the near future.

Although it involves more than just fertilizers, this is probably a good place to say a word about "organic farming," or the production of crops totally without the use of "artificial" chemicals. This is becoming a popular practice in a number of developed countries; and, because it is a commercial success there, some naively think it is a viable solution to the sustainable food production needs of the Third World. What must be remembered is that the reason this is a commercial success is that the low yields achieved with organic farming are compensated for by the higher prices the limited number of devotees to this practice are willing to pay. Producing less at higher prices is a luxury that the affluent can afford; but it is not a practice that will make available larger amounts of food at lower prices, as must be the goal in developing countries. The value of organic material and the viability of some nonchemical solutions have been discussed above, and need to be taken seriously; but pure organic farming is a nonstarter.

Decentralization

More attention to the needs of small farmers and to marginal areas implies a need to decentralize much of the research effort. While this is an important research strategy, it conflicts with some of what has been said earlier about research management. A balance, therefore, needs to be struck between the greater relevance and specificity achieved by a more decentralized research and the loss in efficiency and effectiveness resulting from excessive fragmentation of effort and proliferation of research stations. A great aid in reaching such a balance is more accurate information on the agroecological and socioeconomic information in key regions. A better definition of the major differences in these factors as they affect priority crops and animals will make it possible to keep to a minimum the number of stations needed to cover the most important regions and commodities adequately. The research impact can be decentralized to a certain degree without excessive dispersion of staff and facilities. This will be greatly facilitated if much of the evaluation of technology in its final stages of development is done on farmers' fields, as advocated below. To the extent that much of this work can be done by the regional teams dedicated to on-farm research, the more specialized scientific staff can be located at headquarters and at a few key regional stations.

On-Farm Research

General Comments

A common myth, repeated frequently by government officials, is that there is plenty of good agricultural production technology already "on the shelf;" they add that what is needed is not more research but more effective extension efforts to get this technology out to the farmers. The truth is that the farmers have not adopted much of what extension agents (if they ever see one) recommend, because the technology is not suitable for their conditions. Too often, extension recommendations are standard sets of practices that do not take into account regional and local edaphoclimatic and socioeconomic conditions. Furthermore, the technology on the shelf, if there is any, has more often than not been developed in monoculture on a research station, and has not been evaluated or validated under real farming conditions.

The farmer is often categorized as backward and conservative because he does not adopt the new technology packages recommended to him, when in fact he has often been wise not to do so. Farmers may be poor, but they are not stupid; they may be illiterate, but they can count. They have many times demonstrated amazing alacrity in adopting new technology when it represents a clear improvement, as measured by values important to the farmer, and when a market exists for the additional production.

Because of the importance that new production technology respond to the real needs and conditions of farmers, it is essential that more is known about these factors. This will require surveys of farmers' practices, resources and aspirations, as well as better characterization of the diverse agroecological conditions in which farmers in the country operate. There is also a need to evaluate technology under these diverse conditions during the generation process and to validate promising technology under farmer management practices. This calls for doing more work on farmers' fields, with their participation in the process.

Farming Systems Research

Because of the factors discussed above, and a perception that a strict commodity approach was incomplete, a wave of activities under the general banner of Farming Systems Research (FSR) has arisen, and a plethora of different aid-funded projects have been developed during the past two decades. A good deal of valuable work has been done and useful concepts developed; but there has also been a considerable amount of nonsense generated. Countless conferences and seminars have been held on the subject, and a whole new jargon-filled terminology developed by the evangelists of this new religion has often done more to confuse than to clarify. Many ill-conceived and poorly implemented research activities have been carried out in developing countries under the banner of FSR, often developed outside the established research organizations through external funding. The variety of different approaches to FSR by the contractors implementing such projects, and the diversion of manpower resources to work with them, have confused and sometimes alienated developing country officials and researchers, many of whom had already been engaged in FSR (albeit often without calling it that) for a long time. For these reasons, the very term FSR has come into disrepute in some circles. This is unfortunate, because the concepts are valuable and should not be abandoned. I shall not attempt here to review the vast literature generated on this subject. Plucknett *et al.* (1986) have compiled a useful list of the twenty-five recent publications they consider best on this subject. I would add to this list a paper by Stoop (1986), which proposes a sensible, stepwise approach to integrating a farming systems perspective into national research programs.

I agree with the general concepts of FSR listed by Sands (1985): "(i) FSR is farmer-oriented; (ii) FSR is systems-oriented; (iii) FSR is a problem-solving approach; (iv) FSR is interdisciplinary; (v) FSR complements mainstream commodity and disciplinary agricultural research; it does not replace it; (vi) FSR tests technology in on-farm trials; (vii) FSR provides feedback from farmers."

I also find the description of the "activity areas" of FSR delineated by Dillon *et al.* (1978) useful. These are Base Data Analysis (BDA), Research Station Studies (RSS), and On-Farm Studies (OFS). The main problem with these is their description of BDA as "the collection, collation and analysis of data on the many factors characterizing the environments and farming systems of a region. While the collection of more information about the conditions and needs of the farmers is essential, the holistic nature of the activity has often trapped FSR activities in this mode, without sufficient attention to the other two aspects. "Holistic" is a nice-sounding word, but can become an excuse for endlessly studying and describing what farmers do and why. As Lipton perceptively pointed out (1986), FSR is a good servant, but a bad master.

The degree to which research institutions should engage in FSR, and how it is organized within the national agricultural research system, depends greatly on the size of the country and its research system, as well as the complexity and diversity of its commodity mix and agroecological regions. Thus no one formula can be proposed that is universally applicable. However, especially for small- to medium-sized countries, I favor an arrangement by which the farming systems perspective, and the concept of on-farm testing, is integrated into the research programs of the institutions rather than the establishment of separate FSR entities. While FSR is necessarily different in an international research center than a national research institution, the way in which we have dealt with this subject at CIAT may be helpful in illustrating this concept.

In the early 1970s, CIAT established, along with its commodity programs, a program called "Small Farms Systems Program," to ensure that the work of the Center would address the needs of small farmers and deal with issues pertaining to low-resource farmers such as those described above. However, a bewildering array of activities was developing within that program, and these did not seem to be effectively linked to those of the commodity programs. Furthermore, when I asked each scientist in that program to describe the products of his research and the clients for that product, I received a broad range of answers. Thus, after careful study and a workshop on the subject, it was decided to discontinue that program and instead incorporate these concerns into each commodity program. This was accompanied by the placement of an economist and a cultural practices agronomist into each program, and the establishment of an Agroecological Studies Unit. The former would give special attention to on-farm work, while the latter would develop data bases to characterize the zones in which each of the commodities was grown. This has worked very well; I am convinced that CIAT now does much better FSR than when this activity was treated as a separate entity. Various external reviews of the Center have confirmed this; and Plucknett *et al.* (1986) now hold up the CIAT programs as exemplary in the nature of their FSR.

OFR/FSP

Given the misunderstandings surrounding FSR, and the dangers of being too "holistic," I prefer to refer to these activities as On-Farm Research with a Farming Systems Perspective (OFR/FSP) (Byerlee 1982). This type of work embraces all three (BDA,RSS, and OFS) components advocated by Dillon *et al.* (1978), but restricts the BDA to only the essential surveys required to delineate the research areas. The number of target areas for OFR should be kept to as few as possible, and in each target zone a cluster of farms or villages covering the range of variability should be chosen, so as to reduce the costs involved. In each zone an initial characterization survey should be conducted to identify the major systems employed and constraints faced by farmers. This facilitates the FSP part of the equation. These initial, exploratory surveys should be carried out in a short period of time, two weeks or so, with the aim of moving on to testing as soon as possible. Although initial characterization is rapid, diagnosis continues as the trials go on. The on-farm studies include research control, joint research/farmer control, and farmer control of trials. Station research to help overcome constraints identified by the on-farm studies is an important component. Although not strictly part of OFR, its execution by the appropriate members of the multidisciplinary commodity team is more likely because of the integration of these activities within the program. Regional, multidisciplinary and multicommodity teams can be made responsible for carrying out the major on-farm studies, with the active participation of the national commodity program scientists, all of whom should be working with a farming systems perspective.

OFR/FSP should not normally strive to develop entirely new farming systems (NFSD in the terminology of Simmonds 1984). The farmers themselves are usually in the best position to develop the farming systems best suited to their particular circumstances. The role of OFR/FSP is to identify, test and validate improved components which the farmers can utilize in making their systems more productive.

There should be active participation of the farmers in the on- farm research. Their expert knowledge of their systems and their farming skills are an invaluable resource in the survey activities; and their natural tendency to experiment is an important aid in the design of experiments and evaluation of results. While their involvement in technology assessment has been an important component of OFR/FSP, the use of their knowledge and skills to guide the direction that technical innovation should take is a more recent development. Ashby (1986) has described innovative work in utilizing the farmer more in the definition of criteria for testing, especially in the design of trials, and how different types of farmer participation affect on-farm trial management.

Examples of how the constant experimentation by farmers and their opinions can assist in designing and evaluating new technology are described by Voss (1986) in relation to bean improvement in the Great Lakes region of Central Africa. There, beans are grown almost entirely in mixtures. This presented a daunting challenge to bean breeders. Before developing a breeding strategy and designing an on-farm variety and an agronomic practices testing program, an anthropologist and a nutritionist surveyed a large number of farms in the region to learn more about the farming systems employed, what farmers see as the major problems, and how the ingredients of the mixtures are selected. It was found that the composition of the mixtures was not haphazard, but carefully determined by farmers (who are mostly women), based on various qualities, including cooking time and soil fertility requirements. Different mixtures are used for the more fertile land near the household than those planted in the less fertile areas. It was found that a high proportion (78%) try new varieties first in pure stands before incorporating the ones they prefer into the mixtures. One agronomic practice identified as having great promise for increasing production on the very small holdings in that region was to introduce climbing beans to areas not now using this practice. By obtaining the reaction of farmers to this practice in farms where this was tried for the first time, it was learned that many farmers would probably adopt it if the major problem of availability of staking material could be solved. Interviews with farmers in areas where this practice is already common were used to find how they have solved this problem.

An important component of OFR is on-farm varietal testing (OFVT). This practice has for years represented an important tool to evaluate promising new lines to assess their performance under the conditions in which they will have to excel in order to succeed. More recently there has been a move toward testing not only finished, pre-release materials in this way, but to make selections earlier in the breeding/ selection sequence on farmers' fields. At CIAT, we are doing methodological research to determine how to involve farmers more in the selection process. OFVT, in general, and its use at earlier stages in particular, offer great potential for developing varieties more likely to succeed and to benefit small farmers; however, there is resistance to this practice in some circles. The reservations have to do with the fact that when farmers are involved they are likely to keep the seed of the materials they consider superior, multiply it for future production, and perhaps even give or sell it to their neighbors. While some consider this a desirable means of technology diffusion, others look on it as material "escaping" from the formal varietal release procedures. For crops which have a high rate of multiplication, and thus a potentially rapid rate of dissemination, this is a legitimate concern which must be kept in mind. Another reason such "escapes" are looked upon with disfavor is that in some countries the research institution gets much of its favorable publicity from varietal releases, and may consider this of more immediate importance than production increases brought about by informal spread of new varieties. The fact that some reward systems for scientists are linked to varietal release further clouds the scene.

In on-farm trials it will be necessary to consider various methods of gauging the desirability of any innovation. The traditional measure of yield per unit area may not always be the most appropriate, especially where land is not the scarcest factor. Yield per unit of labor in critical periods, per unit of energy, or per unit of cash input will sometimes need to be utilized (Hildebrand 1985).

Farm wives are often better judges as to whether or not a new variety has the desired grain quality. Commercial middlemen are averse to taking risks on new, slightly different grain types and may either refuse to buy them or greatly mark down the price, even when consumer studies in the urban markets show that housewives accept the grain type. A good indicator as to whether market problems with particular grain types are real or artificial is to observe whether farm families keep varieties which are higher yielding but are marked down in commercial channels because of their grain quality. Those that are kept for own consumption by farm families provide useful information about grain types which will have a positive social impact. Breeders should give more attention to developing such varieties than to putting excessive effort into precisely meeting very demanding standards for an elite market. Lipton (1985), referring to the price discount applied to some of the high-yielding cereal varieties (MVs), stated, "a great advantage of most MVs to the poor consumer is their 10-15% price discount. Breeding for stability and quantity maintains this discount, and does most for poor consumers. Breeding for quality, palatability and gourmetry harms them, by raising prices."

Conclusions

A research leader must be a research strategist; and to be one in the Third World is, as this discussion has demonstrated, a complex, daunting task, especially when the goal is to develop technology for small farmers growing a wide range of commodities in a bewildering array of systems under marginal, rainfed conditions. It becomes even more difficult when the goals of sustainability and equity are added to the agenda. It becomes impossible when budgets are reduced at the same time. Government officials responsible for allocation of resources, and donor agencies which help to fund agricultural research, but wish to add their own preferences to the agenda, must be aware of the fact that on-farm research, while cost effective, is more expensive than traditional on-station research, and that developing technology which will result in increases in production that are sustainable and equitable is also expensive. But they are worth the investment –an investment which must be made if the immense potential of modern science to enhance the well-being of current and future generations is to be realized.

Winston Churchill summed up well the situation in which you find yourself as you face the challenge of leading your country's agricultural research. He said, "To every man there comes in his lifetime that special moment when he is figuratively tapped on the shoulder and offered the chance to do a very special thing, unique to him and fitted to his talent; what a tragedy if that moment finds him unprepared or unqualified for the work which would be his finest hour." I hope this letter will be of some help in preparing you for that task.

Sincerely,

John L. Nickel

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Research Management for Development: An Open Letter to a New Agricultural Research Director by John L. Nickel is a frank and lucid account of all aspects of agricultural research management that may be encountered in developing country institutions. In Part One, Dr. Nickel's discussion encompasses the creation and maintenance of an institutional value system, planning human resource needs, motivation for excellence, and the cultivation of attitudes that promote effectiveness, in addition to practical aspects of personnel selection and evaluation. Also in Part One are chapters on Managing the Research (at national, institutional and individual levels; multidisciplinarity; program *vs.* project; participatory management), Managing the Funds (budgets; resource mix; dealing with under-funding), Managing the External Interfaces (research/extension; clients/users; governments and funding sources), and Managing the Managers (building the management team; enhancing leadership skills; decision-making). Concrete, practical examples are provided.

In Part Two, Dr. Nickel presents a research strategy for sustainable agricultural development with equity, discussing socioeconomic and ecological conditions, and the importance of biotechnology. He concludes with a dissertation on a minimum input strategy, including comments on farming systems research and the key role played by on-farm studies.

John L. Nickel, Ph.D., currently Director General of the **Centro Internacional de Agricultura Tropical** (CIAT) in Cali, Colombia, where he oversees the work of 120 research scientists, has dedicated a total of 38 years to the improvement of world agriculture, living and working for most of this time in seven different developing countries in Asia, Africa and Latin America. His previous experience includes research and administrative positions in the International Rice Research Institute (IRRI) in the Philippines and the International Institute of Tropical Agriculture (IITA) in Nigeria; he also served as Dean of the Faculty of Agriculture at the University of East Africa (later Makerere University) in Uganda. In the course of his career, he has made working visits to agricultural research stations in 52 developing countries, and to farmers' fields in 43 of these.

Significantly, Dr. Nickel has spent the last 23 years as a manager of agricultural research. He is therefore especially qualified as a mentor for those who will follow in his footsteps in this field, wherever they may be based.

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